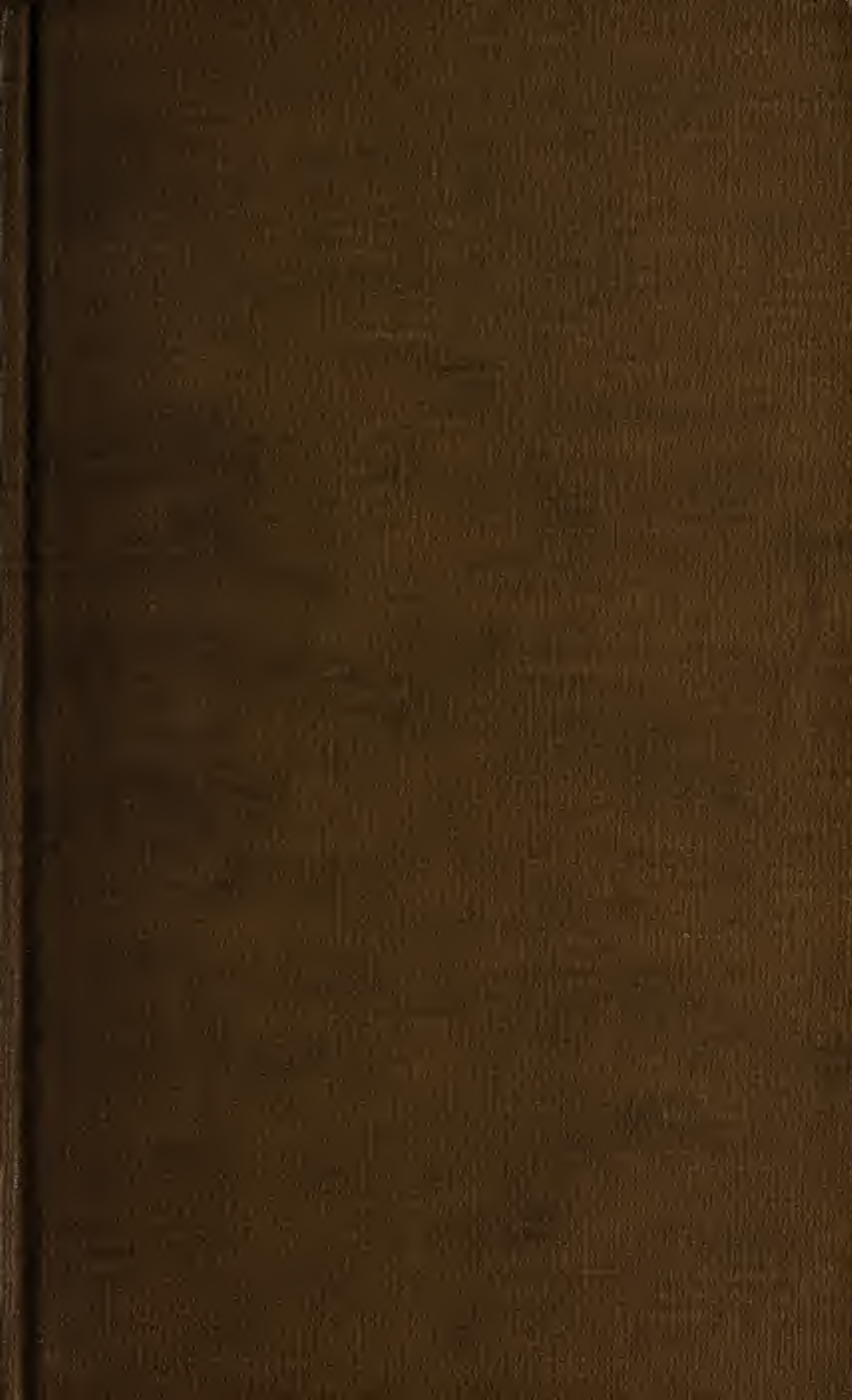
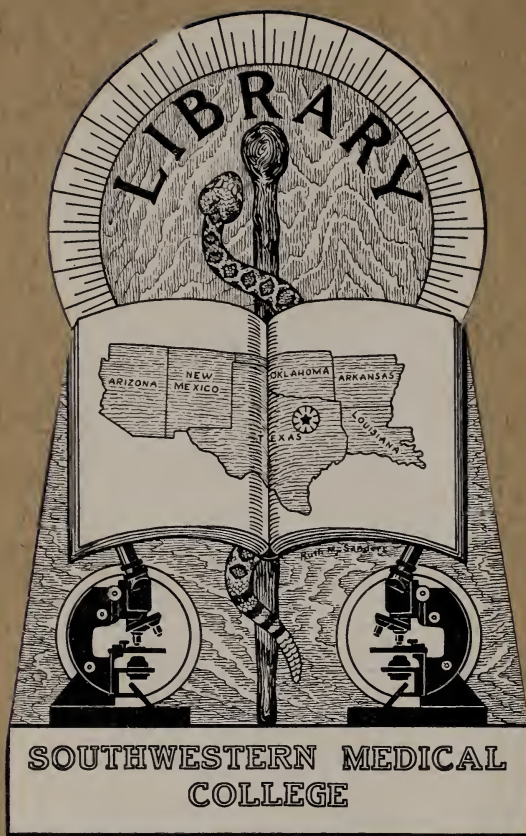






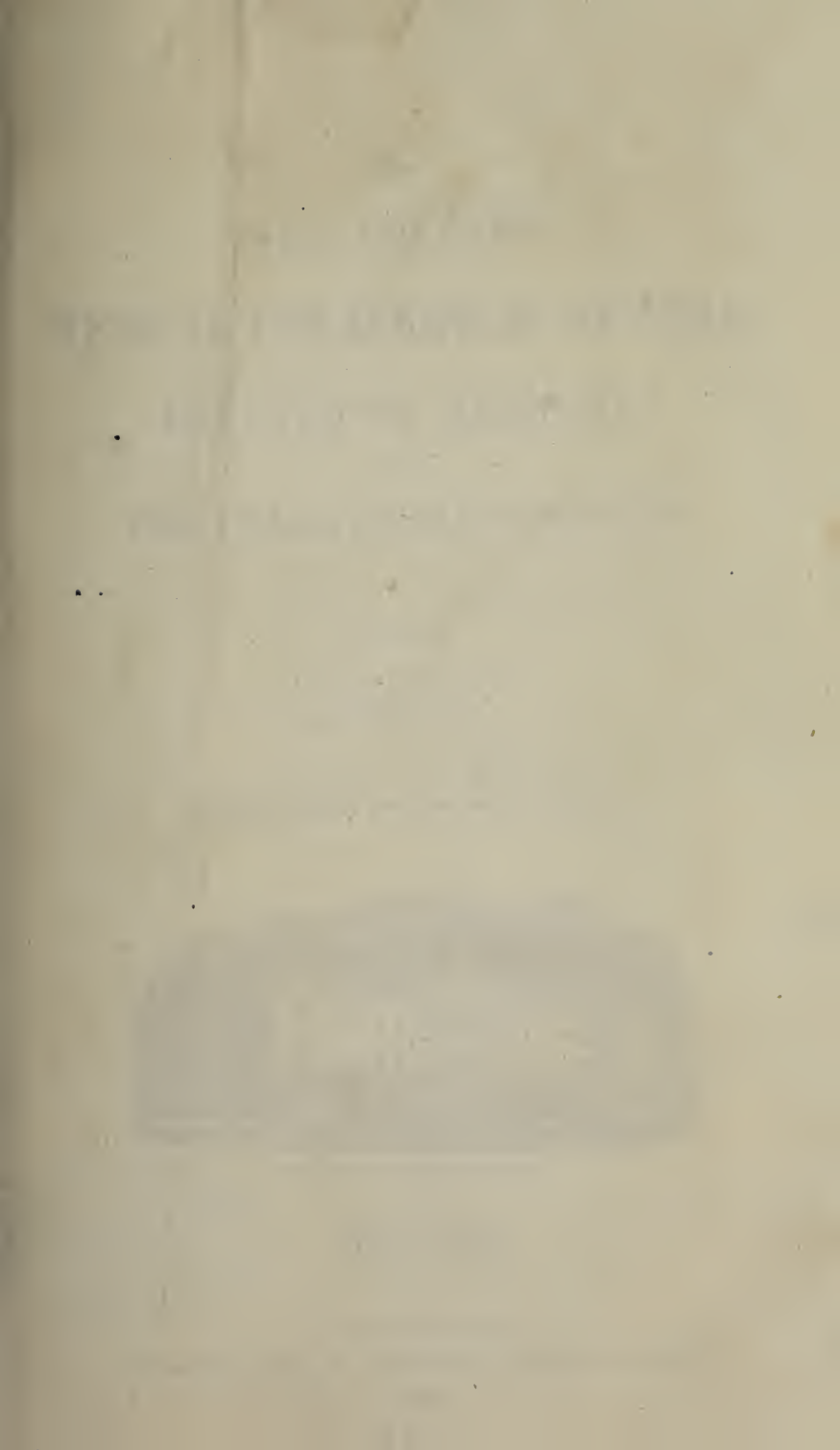
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THE
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NEW ORLEANS

MEDICAL AND SURGICAL JOURNAL,
DEVOTED TO MEDICINE
AND
THE COLLATERAL SCIENCES.

EDITED BY

J. HARRISON, M. D.

A. HESTER, M. D.

“Summum bonum Medicinæ, sanitas.”—GALEN.



NEW-ORLEANS CHARITY HOSPITAL

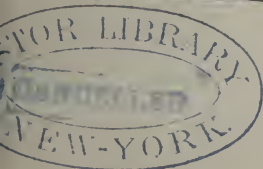
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THE NEW ORLEANS MEDICAL AND SURGICAL JOURNAL.

JULY, 1848.

Part First.

ORIGINAL COMMUNICATIONS.

- I.—*Remarks on some of the Diseases which prevailed in the 2d. Regt. Mississippi Rifles, for the first six months of its service.* By THOS. N. LOVE, M. D., Surgeon to the Regiment.

The melancholy history of the 2d. Miss. Rifles has been a subject of deep interest and heart-felt sympathy with the people of Mississippi. The peculiar sufferings of the regiment; its marches through different climates, and the fatal diseases, which have carried to their long homes so many of its gallant members, are fruitful themes of thought and investigation. Six months after our regiment had entered the service we had sustained a loss of 167 by death, and 134 by discharges. This is enough to awaken anxious enquiry into the causes which have produced such destruction in our ranks. It shall therefore be my object in this communication to give a short history of the epidemics with which we have been afflicted, and in the same connection give a history of the troops, their condition, privations, sufferings, the weather, climate, and all the circumstances which had more or less influence upon the health of the men.

The ten companies of volunteers called for by the President in the latter part of the year 1846, to serve during the war with Mexico, met at Vicksburg, between the 2d. and 6th of January 1847. During the march of the respective companies from their residences to the place of rendezvous the weather was most delightful—equal to Indian summer. Most of them left home without having provided themselves with sufficient clothing, intending to appropriate in this way the money which Government allowed them for this purpose. But few of them were provided with wollen clothes and hardly one in ten with flannel. The Governor of the State had rented ware-houses which were fitted up and appropriated as barracks for the companies as fast as they assembled. These were very uncomfortable for men lately accustomed to feather beds and warm buildings, but these were perhaps the best quarters that could have been

procured. The troops were however destined to enjoy these quarters but two or three days. The officers appointed for the purpose of organizing the regiment selected an encampment $2\frac{1}{2}$ miles above Vicksburg, which in honor of a gallant officer they named "Camp McClung." The men were rapidly mustered into service; not having been subjected to a close inspection the result was that many weak, infirm, and broken in constitution had entered the army. Indeed many had volunteered for the purpose of restoring their health. As fast as they were mustered they marched to the camp, which proved to be a very injudicious selection, situated upon a low bank of the river, exposed to a wide sweep of the north and west winds. Before the men had fairly pitched their tents or became rested from the fatigues of the march, the weather became very inclement. The whole encampment was covered with mud and water. The blankets and clothing of the men were saturated with water. The young soldier's couch was made upon the damp and chilling earth, rife with disease and death. Add to these misfortunes, they were suddenly placed upon the diet of camp life. On the 10th of Jan. one of the most remarkable changes in the weather occurred I remember ever to have witnessed. The previous night the windows of the very heavens seemed to have been opened, and torrents of rain came like a flood over the encampment. Early in the morning the cold north wind came sweeping down from the broad bosom of the Mississippi, bringing with it a storm of sleet and hail. The situation of the troops now became truly distressing. The inclemency of the weather was such that it was impossible to furnish them provisions and wood sufficiently or regularly. Some muffled themselves up in their dripping blankets and huddled together in their cold and comfortless tents; some hovered over the smoking fires, calmly submitting to the pitiless peltings of the storm, and others, with their wet and frozen blankets close around them, wandered forth through the streets friendless, homeless and houseless. Language fails to give an adequate idea of the sufferings of our men. They felt, "as if the very marrow of their bones was congealed."

These causes, which I have briefly enumerated:—fatigue, exposure, insufficient food and clothing, were soon followed by the most remarkable and disastrous effects—influenza, rheumatism, pneumonia and a disease more formidable than them all—*cold plague*. In this condition the regiment embarked on board steamboats for New Orleans. Our men were here prostrated by dozens; unprovided with medicines, or even a shelter, they were compelled to seek that comfort which their friends could best procure for them. The situation of the troops became so distressing that they were removed as fast as possible to the Battle Ground below New Orleans. All but two companies were transported upon the 13th & 14th of January. Two companies had taken quarters in Vicksburg and were not transported until the 18th. The citizens of Vicksburg no doubt remember well the eccentricities of a Captain who marched his company about the streets day and night, through mud and water, the result of which was subsequently manifested in a greater proportion of mortality among his men than almost any other company.

During the transportation of the troops to the Battle Ground, their sufferings if possible were increased. Every day we had more or less rain; the cold wind shifting from every point of the compass. The men

were crowded upon steamboats, with their wet tents and damp blankets piled about them, poorly provided with shelter and no conveniences for cooking. It was distressing to go among them at night and hear the incessant coughing of hundreds, and the lamentations of the sick, suffering with cold and calling for the simplest wants.

It was during showers of rain and blasts of cold wintry winds that they erected their encampment upon the Battle Ground, and upon an earth saturated with water, they made their beds. One hour the sun shown out beautifully, the next the soldiers were seen muffled in their blankets, turning from the cold wind, and the next they were found huddling in their tents from the torrents of rain. Not a dry foot of land was to be found in the whole encampment. The heat of the fevered patients vaporised the dampness of the blankets, creating in their tents a dense, suffocating steam.

In this condition medical treatment was unavailing, and not until the seal of death had fixed the destiny of many a brave soldier, did our Government officers pretend to offer the least assistance, and at last it was only to the dying soldier they gave a scanty couch of straw within the walls of a cheerless building, far more comfortable however than was our former condition. Many of our sick had already sought lodgings at their own expense in private hospitals and boarding houses. When comfortable quarters were not allowed by officers whose duty it was to procure them, our officers very justly granted permission to their men, who were sick, to seek for themselves lodgings in the city. The removal of the sick to the hospitals was a distressing scene. On one occasion more than fifty were taken to Dr. Luzenberg's Hospital. They were first taken in cabs, to the river, then crowded into the cabin of a small towboat, then again into cabs to be transported to the rail-road and thence into the cars, and before they were safely lodged in the hospital it was midnight; many of them apparently suffering with severe inflammatory rheumatism, and every effort to move them was uttered an involuntary scream of agony. It would be ungrateful in me not to acknowledge in this connection the kind attentions and unceasing energy with which Dr. McCormick, the Medical Purveyor of New Orleans, labored day and night to afford us relief. The private physicians of the city were exceedingly kind in bestowing their services, of whom none is more deserving of our sincerest gratitude than Dr. Fenner.

The next step was to remove the troops upon the transports, for which we had waited impatiently for several days. Here our sick list continued to increase. The situation of the troops was but little better than in camp. In the holds of three ships were crowded nearly 800 men. Their tents, blankets, and clothing still very damp, the weather being so inclement that it was impossible to dry them. The berths were made of green pine plank which were as cold and hard as marble. Upon these our sick men were confined. They were not even allowed straw mattresses although they were earnestly required and demanded, and could have been procured for one dollar a piece. I have seen the sick soldier in his delirium, raging with madness and writhing under the terrors of disease, tear his flesh upon the rough sides of his rugged couch. At length all our sick were removed from the ships to the hospital and on the 30th of January we were sailing fast from the scene of desolation, with the sanguine

hope that we had left behind us the scourge. Indeed, we had left behind *eighty* of our men that were destined never to join us again. But our brightest hopes were soon at an end. The dark cloud of disease still hovered over us. The holds of the ships offered scenes distressing to the most callous heart. The evaporation from the dampness of the blankets and tents settled in great drops upon the ceiling. The holds of the ships were soon crowded with the sick. The effluvia was intolerable. The attendants were young and inexperienced. The sea became rough and the companions of the sick were no longer able to give their kind attention. They had nothing to eat which a peevish appetite would crave. Here the soldier was deprived of the simple attentions of a kind mother and sisters who come around the couch of a sick man like ministering angels. Through the long dark night the rolling ship would dash the sick man from side to side bruising his flesh upon the rough corners of his berth. The wild screams of the delirious, the lamentations of the sick and the melancholy groans of the dying, kept up one continual scene of confusion. We had a long tedious voyage—four weeks we were confined to the loathsome ships and before we had landed at the Brasos, we consigned twenty-eight of our men to the dark waves.

We have in this short history of human suffering sufficient causes to account for the terrible disease that prevailed among our troops. But in this place I can not pass over the charges of intemperance and the loose rein that our officers gave their men, as other causes to which our misfortunes are readily attributed by others. As regards intemperance, I am not able to say, from a close observation of all the results which followed the exposure of our men during this period, that it proved injurious to them, but rather upon the contrary, that those who drank freely of ardent spirits escaped comparatively the diseases which afflicted our regiment with so much mortality. These charges were urged so vehemently by our enemies at New-Orleans that I have deemed it necessary to draw up a table for the purpose of arraying the truth in contradiction of these imaginary evils. I have no desire to vindicate intemperance, but simply to bring before the world the plain truth. I have therefore with the aid of the company officers drawn up a table which is intended to give the diseases, habits, ages, occupation, residence, and places of death of all the men who have died during the first six months of service. I did this by taking the muster roll of each company, and gathering from the company officers the history of each individual case as accurately as possible. In regard to the diseases, the table cannot be relied upon as accurate. To the question "of what did he die?" the reply was often given by the company commandant, "*that disease* at New-Orleans." In reference to the habits of intemperance, I urged the officers to give me a correct statement, as the names of their unfortunate members were not to be exposed. *Very temperate*, are those who were not in the habit of drinking any at all. *Temperate*, those who drank without becoming intoxicated. *Intemperate*, those who were fond of drinking and spreeing. *Very intemperate*, those who were constantly drunk where they could procure liquor to make them so.

I might as well here allude to some other objects of this table. It will be seen that the average age of the dead is less than the original average age of the companies. In another table of the discharged, I find the aver-

age age greater than that of the dead, but still less than the original age. This will suggest to us at once that the policy of mustering into the service men of a very tender age is not to be recommended. It will be seen also that those from the country have suffered much worse. But it is true that a much larger proportion are from the country; yet it is not less true that the young farmer from the peaceful walks and rural shades of a quiet life, is unprepared to meet the exposures and irregularities of a camp life.

Perhaps I could not undertake a more difficult task than to delineate the peculiar features of this remarkable disease. The want of reference to books; the imperfect sketches I have retained of its ever changing symptoms; the removal of the patients to the hospitals; meeting in every step of professional duty the most difficult obstacles; all tend to render analysis and the application of general principles difficult and doubtful. I must therefore content myself with an imperfect outline of those cases which came under my observation.

I find myself at once in a doubt under what head to arrange an affection which presented so many forms. Of the many appellations given to it, none I think more appropriate than that of *cold plague*. Although it is allied more closely to typhoid pneumonia than any other disease, yet this is a term lately applied by our western physicians to a more definite form of disease. The pneumonic symptoms were so often absent that we cannot use the latter term, as it will not convey a correct idea of this peculiar affection. Neither do we claim any partiality for the vague term employed, but for the obvious reason that we know no name for it. Although we had influenza prevailing as an epidemic, still there was a marked difference in the two affections. Under the title I have selected, there is no restriction to any particular organ, and it is under such a term alone that we can convey a faint idea of a malady that seizes almost indiscriminately upon almost every organ of the human frame. The forms of its attacks were so various and insidious that none were prepared to meet it, or knew how to escape the fatal snare. Neither can we, according to the general acceptance of the term, consider this disease as an epidemic. It was confined alone to our regiment; we hear of it nowhere else. The Pennsylvania troops were encamped on the Battle Ground at the same time, and appeared to enjoy good health. They were however well clothed in wollen goods and had become better accustomed to exposure and the vicissitudes of camp life than we; and besides, they were from a cold climate, and the causes which were operating upon us, had little or no influence upon them. We had no reason to believe that there was any peculiar poisonous agent in the atmosphere. I saw nothing which would justify the belief that the disease was at all contagious. Every one seemed to turn an eye to our severe exposure at Vicksburg as the source of all our affliction. It is beyond all dispute that those who were more exposed, who were poorly provided with flannel, who were unaccustomed to sleeping upon the cold ground, and those who did not take the precaution to keep up the vital energies with internal stimulants, but contented themselves with a poor and insufficient diet, proved to be the unfortunate victims. The Captain of Company (G.) provided his men with additional blankets at Vicksburg and gave them liquor at New-Orleans; while the captain of Company (I.) with unusual military

ardor exposed his men by marching them unnecessarily about the streets of Vicksburg—see the result: the mortality of one is *nine*, the other *twenty-four*! The members of Company (C.) and (D.) were men of tender age, and from the retired walks of rural life, and thus we may account for their great loss. I might pursue these comparisons further, but I have said enough to show the influence of local causes.

We have then a disease before us which springs from a well known and defined origin. To define, in short, its most striking characteristic would be to say that it was a disease produced by long and severe exposure in connection with other causes that I have briefly enumerated, *in which the nervous system sustained the greatest injury, and lesions, both organic and functional happened to those organs least prepared to sustain themselves.* The nervous system, the organs of respiration and those of locomotion, were generally the organs most involved; while those connected with digestion were not only comparatively free from attack, but remained for sometime in a good condition. The attack generally came on suddenly, without any premonitory symptoms, sometimes ushered in by a chill, and at other times with a severe pain in the head or in some remote part of the body. The duration of the disease depended altogether upon the part affected, the severity of the attack and the state of the constitution. Some of the first cases which occurred under my observation were characterised with livid splotches upon the extremities, not of that petechial or eruptive character attendant upon malignant fever, but seemed to be the result of capillary paralysis. These splotches which were irregular in shape, varying from the size of a small pea to that of a half dollar, presented no elevation of the skin, and were by no means a uniform attendant upon this disease. One of the strongest general features in this affection was its great tendency to metastasis. The patient would first complain of severe pain in some remote part of the system, which would in a few hours change its locality and perhaps finally attack some internal organ. I well remember a case of this kind. The patient first complained of severe pain in the wrist; this lasted several days, slightly swollen and so painful that he seldom slept. His general health appeared good. At length the elbow joint became affected, some swelling &c. exceedingly painful to the touch. Suddenly the pain in the wrist and elbow joint ceased. I was called to prescribe for an incessant vomiting. The tongue became dark, resembling charred wood. The patient sunk under an uncontrollable diarrhœa.

Another case similar to this, was a man of fine constitution, aged 20, attacked with severe pain in the knee and ankle joints, some swelling and exceedingly tender. In a few days a metastasis occurred; the right eye being now the principal seat of pain. There was no apparent inflammation at first. There was a constant flow of tears. The pain was so severe that the patient could not sleep. The temporal artery was opened, cups applied, nape blistered, and cold applications used. Notwithstanding this active treatment, the eye became swollen and inflamed, and the finale was the destruction of that organ. The convalescence was long and slow. There were many cases of this character, the eye being the seat of the disease. I was very much shocked at the result of the first case; I supposed it to be only a violent ophthalmia, but the organ was actually destroyed. Ever afterwards I anticipated having a severe malady to treat when

I found a patient suffering excruciating pain in the eye with an abundant flow of tears. We could not consider such cases at all dependant upon congestion of the brain or its membranes; for there was no delirium or any disturbance of the *sensorium commune*.

One of the most prominent types of this disease was great congestion or capillary paralysis—the patient pulseless, the extremities covered with livid splotches, while the patient was insensible to his condition. The first case that I met was a form of disease so new to me that I must acknowledge my indebtedness to the accomplished Drs. Crump and Balfour for some valuable hints. The case referred to was a young man of robust constitution, temperate habits, had been sick 12 hours, complained only of his feet and legs. They were swollen, slightly painful or rather of a benumbed sensation. They were covered with dark purple spots of various sizes, and as cold as marble. Tongue clean, stomach quiet, easy breathing, somewhat restless, pulse imperceptible at the wrist. The patient thought if I could restore “right feelings in his feet,” he would be able to join his company the next day. Pediluvia with mustard, cataplasms with mustard, active stimulants internally were used, but no reaction was produced and death followed that night.

Another case of the same character occurred in a young man of good constitution and temperate habits. Soon after walking upon the boat, as we were about leaving Vicksburg, he was taken with a chilly sensation. I was soon called to see him—pulse almost imperceptible at the wrist; lips purple; great prostration. I gave him tinct. Opii. ʒ ii. and brandy; cataplasms with mustard applied to the extremities and hot stones to his feet. In four hours reaction was produced. At bed time he took twenty grains of quinine. The next day he had another chill. Here the same treatment was vigorously adopted with the addition of stimulating enemata and friction in order to produce reaction. Early the next day he was removed to a private boarding-house in New-Orleans. I heard no more of him but presume he died.

A young man of temperate habits, rather delicate constitution, was much exposed on the boat in going down the river. In the city he was taken after dark with a chill and pain in his head. He took a little medicine and went to bed. In two hours he complained of being very cold and must go to the fire. In a few moments he said that he felt a tingling sensation and then a burning all over as if he was in the fire. In six hours he was delirious and incapable of taking medicine; pulse imperceptible; lips purple; coma supervened and the extremities became dark and purple. Death sealed his destiny before daylight.

One of the most alarming forms of this disease was in the sudden attack of the throat and air passages, with an abundant effusion of serum by which the patient was suffocated in a few hours. I witnessed more than two cases of this kind. A young man had gone to bed apparently well. At 11 o'clock I was called to see him—found him insensible, breathing with difficulty; pulse weak and frequent; lips pale; a wheezing and rattling sound indicated impending suffocation. Shortly after midnight the patient died as a young child with croup.

Some cases assumed the form of congestive fever, ushered in by a long protracted chill, coma, imperceptible pulse, reaction with fever, restlessness, followed the next day with an exacerbation, increased in

intensity, proving fatal the third or fourth day. Others again were considered to be gastro-cephalic irritation. Those which assumed the common form of typhoid pneumonia were ushered in by a chill, weak, frequent pulse, great prostration, heavy weight at the præcordia and frequent sighing. But what is most remarkable there was scarcely ever any difficulty in breathing. But it is still more remarkable that in this form of the disease as well as many other forms of it, the stomach and bowels were quiet and frequently in a healthy condition, even at the end of the fifth day. Delirium occurred at an early stage, the tongue soon became thickly coated, the teeth and lips covered with dark tenacious sordes. The patient sunk into a deep coma about the eighth day; low muttering delirium; opisthotonos and death closed his career without, in many cases, any apparent signs of a free and easy expectoration.

But the most terrible modification of this strange malady was in those cases involving the brain and spinal marrow. I do not remember a single case of recovery where these organs were assailed.

Cases of this kind proved fatal from twelve to forty eight hours. The patient complained first of a chilly sensation, with a violent pain in the head calling out: "O, Doctor, do something for this pain in my head." In these attacks there was at first but little variation in the pulse, tongue clean, breathing natural. In many cases the unfortunate victims became delirious in six hours, raging with madness, requiring several of their companions to hold them in their berths. These cases were always attended by opisthotonos of the muscles of the neck. There was a peculiarity in the voice and lamentations of the sick that seemed to strike terror to the hearts of the by-standers. I cannot describe this sound—it soon became a diagnostic, the more terrible because it was a sure indication of a fatal termination. I have no language that can convey the consternation manifested in every countenance at seeing the athletic and blooming members of the regiment cut suddenly down without a moment's warning.

The first case of the kind which came under my observation was in camp below New-Orleans. The patient, a man of middle age, temperate habits, robust constitution, was walking about 12 hours before I was called in company with Asst. Surgeon Kinchloe to see him. We found him insensible, in a state of coma, pulse unusually full and bounding, breathing deep and sonorous, teeth clenched and foaming at the mouth, head thrown back, eyes fixed, pupils contracted. We opened the temporal arteries, blood flowed in a full stream from each, and so much heated that steam was given off from the currents as from boiling water. But no relief was obtained—the patient sunk in half an hour with the last pulsation full and strong. Here it might be well to enquire what chemical process was going on in the animal economy which produced such unusual appearances of the circulating fluid; but the limits of this article will not allow speculation.

I was called to a young friend but a few hours after he was seized with violent pain in the head. He had just returned from the city, where he had indulged in a hearty meal. I prescribed an emetic of Ipecac. This afforded no relief. His agony was so great that he frequently screamed aloud. His tongue was clean, his pulse good, skin natural, breathing easy. I gave him 3 ii. Tinct. opii, and ordered pediluvia with mustard.

Still no relief—pain insupportable: cups to the temple and nape. In six hours after I first saw him he was delirious and all efforts to save him were unavailing. Opisthotonos, coma, and death in thirty-six hours. He had one evacuation while comatose which was perfectly healthy.

In another case I was called upon to prescribe for a young man who had gone to bed without feeling any symptom of disease except headache. His pulse natural, tongue clean, no difficulty of breathing. His constant complaint: "Dr. do something for this pain in my head." Cups were applied freely to the temples and back of the neck, cataplasms with mustard, and a large dose of spts. turpentine and castor oil internally. The medicine operated in a few hours, producing healthy evacuations. At daylight I found the patient delirious and in a few hours more, death closed the scene.

I have attempted to give a brief outline of the most prominent features of this malady. I need hardly say that there were many cases in which the different forms of this disease were combined in one, as if to increase the sufferings of the unfortunate victim. When the mucous membrane of the stomach and bowels became affected, it was more the result of a metastasis than a primary location. I regret that I had no opportunity for making post mortem examinations; no correct conclusions can therefore be drawn of the pathological condition or history of this insidious malady. All is enveloped in obscurity. I have been able to form but one conjecture and this is drawn from analogy. It has been seen that when the eye became the seat of the disease, there was a sudden effusion of serum resembling an abundant flow of tears. We might ask if there was not some analogy in the pathological condition of the serous membranes of the joints when they became suddenly swollen and so remarkably painful. If this be true, we might reasonably suppose that such was the case when the throat and air passages became the seat of the disease and quickly poured out such an amount of serum as to suffocate the patient in a few hours. We might with the same propriety account for the violent pain in the head. A sudden effusion of serum or blood upon the brain or within its ventricles, would of course produce delirium, coma and death. We can not account for these cases upon the ground of congestion; for the circulation was often undisturbed, and therefore it would be unreasonable to suppose that we could have congestion of the membranes of the brain or even of the parenchymatous structure of the organs with so little constitutional disturbance. We know that when the perspiration on the cutaneous surface is suddenly checked, it will frequently reflect itself upon the lungs and produce pneumonia. Persons long exposed to severe cold, with the vital energies already depressed for the want of a stimulating diet, may receive such a shock upon the nervous system as to produce speedy death. Through the nervous system might be transmitted such influences to the brain. How the lesion is here produced and where is the point of its incipency, I am sorry to acknowledge my ignorance.

In the treatment of this disease no one was confident of success. I invoked the aid of all I had learned from books or medical teachers, but found it unavailing. I used depletion, emetics and cathartics in vain.

Sometimes large doses of quinine seemed indispensable, but these were injurious instead of beneficial. The words "I have a pain in my head," pointed unerringly to the result. No tongue can tell of the ten thousand difficulties thrown in the way of the physician in treating disease in a camp where all is privation and in every countenance is depicted alarm and consternation. It is easy to imagine what a depressing influence is exerted upon the human mind, and how unavailing the healing art, however well directed, when the young soldier is prostrated upon the cold ground, unprotected from the keen blasts of the wintry winds, with no kind nurse or companion at his side, he turns a wishful eye homeward, muttering the sweet words of "*Mother*," "*Sister*." Not only are these influences manifested upon the destiny of the sick man, but they wield an alarming influence upon the vital energies of those who might, under other circumstances, have escaped the fatal snare.

It has been remarked, as a general rule there was no derangement of the digestive system; and that the nervous system received the shock. Perhaps the sentient extremity of the nervous system, and the capillary circulation of the cutaneous surface were the first to receive the injury. The main object would therefore have been to restore these organs to healthy action, and enable them to resist the depressing influences around them. This was best secured by internal stimulants; brandy and opium were the best articles. The pediluvia with mustard; cataplasms with mustard; hot rocks to the extremities and friction, were valuable adjuncts. Were I called again to treat such a disease I should rely upon brandy freely and opium to allay irritation and the external applications above mentioned. If the patient were not immediately relieved and we had reason to believe that the disease had become fixed, the treatment would vary according to circumstances. In cases involving the lungs, blisters were unavailing—they became sources of irritation to the delirious patient. I have no confidence in blisters in cases of typhoid pneumonia. It was necessary to husband all the patient's strength; avoid all sources of irritation and causes of debility; and shield the mucous surfaces with mucilages.

The convalescence was long and tedious. It seemed impossible for the constitution to recover its usual vigor. The disease died away as we reached the coast of Mexico, giving place to other diseases, of which I will give some account at a future time.

Buena Vista, Mexico, March 1st, 1848.

A Table showing the Diseases, Habits, Ages, Deaths, etc., of the 2d Mississippi Rifles.

	DISEASES.				HABITS.				AGES.				OCCUPA- TION.		RESID- ENCE.		PLACES OF DEATH.		REMARKS.											
	Letters of Comp.	Cold Plague.	Rubeola.	Varicella.	Dysentery and Diarrhoea.	Pneumonia and Diseases of the Chest.	All other diseases.	Very temperate.	Temperate.	Intemperate.	Very intemperate.	20 and under.	Over 20 & under 26	Over 25 & under 31	Average age.	Farmers.	Mechanics.	Other occupations.	Country.	Town.	Died before leaving N. O.	Died on the Gulf.	Died in Mexico.	Total deaths.	Original number of the Regiment.	Average age when the Reg't was organized.	Discharged.	Transferred, promoted and deserted.	Remaining.	
Commanders.																														
Capt. Blythe, . . .	A. 3	1				3	6	9	4			6	5	2	21½	13	8	2		13		4	2	7	13	88	23½	18	4	53
" James, . . .	B. 7		2	1		4	1	6	6	2	1	4	8	2	123¼	8	5	5	8	8	7	7	3	5	15	89	25	6	8	60
" Liddell, . . .	C. 20	3	3	1		4		24	3	1	12	10	3	3	323	26	2	2	26	26	2	16	4	8	28	90	23	14	7	41
" Acker, . . .	D. 12	1	1	1		1	6	4	13	1	3	8	9	2	223	14	4	3	15	15	6	13	4	4	21	93	22½	18	4	50
" Jackson, . . .	E. 5	1	3	1		1		9	1			6	2	1	121½	9	1		9	9	1	4	1	5	10	84	22½	24		50
" Daniels, . . .	F. 12	3		2			2	16	3			7	8	1	324	15	4		16	3	9	4	6	19	90	25	11	3	57	
" Clark, . . .	G. 7	1					1	5	4			1	4	1	326	6	3		6	3	6	1	2	9	87	27	8	5	65	
" McWillie, . . .	H. 10		5				1	6	10			7	5	3	121	14		2	15	1	5	5	6	16	91	23	13	3	59	
" Estelle, . . .	I. 16	1	2	3			2	14	9	1		6	10	4	424	16	1	7	17	7	15	1	8	24	88	25	10	13	41	
" Buckley, . . .	K. 5		1	3	1		2	4	8			2	3	4	327	11	1		11	1	1	3	8	12	85	27	12	3	58	
	97	3	18	14	14	21	97	61	4	5	59	64	23	21	132	19	16	136	31	80	28	59	167	885	24	134	50	534		

II.—*Operations for Cataract.* By C. S. FENNER, M. D., Canton (Miss.)*Editors New Orleans Medical and Surgical Journal.*

GENTLEMEN:—The following cases you are at liberty to insert in your Journal if you deem them of sufficient interest.

T. E., now aged 22, had an attack of measles when about four years old, followed in a few weeks by cataract in both of his eyes. I first saw him in December 46, 17 years after the attack of measles, and found that no efforts had been made for the restoration of his sight. On examining his eyes, the crystalline lens in the right eye presented a whitish opacity corresponding exactly with Mr. Mackenzie's description of soft congenital cataract. In the left eye the cataract was capsulo-lenticular. The iris moved freely to the stimulus of light. The patient could distinguish brilliant colors when held very near his eyes and was able to detect the shade of solid bodies placed between himself and a strong light. The senses of hearing and touch, as is usually the case with the blind, were both exceedingly acute. His muscular system was well developed, yet he had an inability of freely using the inferior extremities, owing, I think, partly to the fact of his confining himself entirely to the house and rarely taking exercise. It is usually the case that the retina when long excluded from light, becomes amaurotic, so that if the cataract is removed the improvement of vision is so slight as scarcely to warrant an operation. This patient enjoying good health, his ability to distinguish bright colors, the acting of his pupils, with his power of distinguishing different shades of light, induced me to attempt the removal of the cataracts. December 28, '46, after dilating the pupil of his left eye with belladonna, I introduced a very small delicate spear-pointed needle through the cornea near its junction with the sclerotica, and tore away the capsule, the almost fluid milky lens mingling with aqueous humor. On withdrawing the instrument the opaque capsule floated in the axis of vision leaving not more than one third of the pupil clear. This was removed some months afterwards by another operation, which consisted in introducing the needle through the sclerotica, tearing the capsule from its attachments, and carrying it to the bottom of the eye, where it still remains. The right eye was operated on the following March in the same manner as the first. After cutting the capsule the opaque lens was absorbed in a few days, leaving a perfectly clear pupil. Scarcely any pain or inflammation followed either of the operations.

M. E., sister of the patient above described, had an attack of measles at about the same age as her brother and in a few weeks afterwards was entirely blind from cataract, and remained in that condition until March 1847, a period of 11 years. Her case very nearly resembles that of her brother: she too is unable to walk unless supported by an assistant, finding it difficult to balance herself. Her pupils were active, she was able to detect bright colors, and to distinguish different shades of light. I performed the operation of Keratonyxis on both eyes, a period of ten days intervening between the operations. The opaque lenses disappeared in a few days, leaving the eyes entirely clear. Vision as expected was very limited. Neither of the patients had any recollection of ever having seen, and therefore knew nothing of the appearance different objects present to the eyes. They had great difficulty in directing the eyes towards an

object they wished to see. After an article was shown them several times and they had become familiar with its appearance, there was no difficulty in knowing it afterwards—still they would not seem satisfied until the sense of touch was exercised when all doubt would be instantly removed. A large quantity of the black and white seeds of the water-melon was given them to assort, placing the black in one pile and the white in another which exercise would rapidly be performed without a single mistake being made. They point out the fowls and domestic animals walking about the yard and distinguish trees and their large bodies at a distance. They seem to have no just appreciation of distance. With the ordinary toy picture books for children they are highly delighted and will gaze for hours on the gaudy colors, at the same time constantly endeavoring, with the fingers rubbing over the surface, to feel the colors. In congenital cataracts, when the opacity is not removed at an early age, all command is lost over the muscles of the eyes, giving those organs a rolling, unsteady trembling motion. These patients still retained the power of moving their eyes together, which power they had acquired during the period of infancy in which their vision was good. In the patient first described I think the left eye is slightly affected with convergent strabismus. Convex glasses somewhat improve their vision which is still imperfect, though I see no reason why their sight should not continue to improve.

III.—*Observations and Remarks upon the action of the Sulphate of Quinine.* By WM. A. BOOTH, M. D., of Thibodeaux, La.

Peruvian Bark irritates. This effect arises mainly, if not entirely, from the mechanical action of the woody mass in which nature has enveloped its virtues. Owing to this circumstance it has to be used with great caution, and limited with great precision to certain pathological conditions. Stripped of such a source of irritation, the *modus operandi* of quinine and the limits to its use have become questions of interesting discussion.

Having used this medicine freely for the last eight years, I have formed an opinion relative to these matters and will express it.

All admit that quinine is that ingredient of the bark which possesses in the highest degree the anti-intermittent or anti-periodic power. I believe this power depends on an *anti-congestive* property, and this property to be the result of a *tonic action* upon the nervous system.

That quinine is efficacious in the relief of congestion, no one will deny. All in the least experienced have seen it produce this effect. A case occurred to me some years back in which the extremities were cold, the pulse feeble and skin clammy, the tongue dark and dry, the head aching. I gave calomel to act on the secretory system; quinine in ten grain doses to *relieve the head* and restore warmth; and applied counter-irritants to aid it. My objects were accomplished. Such cases occur to every practitioner in malarious sections. The effects produced and its chief cause are acknowledged, yea asserted by all modern authors. As such it is unnecessary to argue this point. But it may not be so clearly seen or readily admitted that quinine acts on the same

principle when it prevents a chill. The cold stage is a temporary state of congestion. Saturate the system with quinine and you prevent it, as well as the subsequent reaction, which is but a consequence of it, and the profuse sweat, which is a result of the reaction when the tendency is to health. Quinine then evidently prevents an "ague fit" by preventing its cold or congestive stage. It is however denied that the hot and sweating stages are consequences of the cold stage, and this denial may appear to invalidate my position. Watson bases his scepticism on two points:—1st, that the alleged cause and apparent effect are not in proportion. In this I think he is mistaken; his mistake originates in forgetfulness of an intermediate cause. Congestion tends to depress the system. The *vis medicatrix naturæ* resists this tendency and produces reaction.

Two individuals are attacked with chills. In one the coldness is alarming; the succeeding fever mild and short. In the other, the coldness is scarcely perceptible; the ensuing fever violent and of long duration. This almost universally happens. Why? Answer: The depressing cause (the miasm) acts more mildly in the first case. It consequently brings the system more completely under its influence before the alarm is given, which awakens resistance. Then the powers of nature, struggling to bring back the healthy state, succeed more gradually, and the result is that reaction does not so far overleap the bounds of healthy circulation. In the second instance, the cause is more violent and irritating. So soon as it enters the threshold of the system, the preservative sentinel is aroused: all its powers are called into vigorous action. The state of depression is removed by this sudden exertion, and the reaction thus suddenly effected is intense and violent, going far beyond the point of equilibrium. That the insidious action of the depressing cause has a great deal to do with the result, is evident from cases of real congestion. One of the worst forms of this disease I have ever attended was characterized by hope and comfort on the part of the patient; the former being but partially dispelled even after I had arrived and announced the startling fact, that he must die, unless powerful remedial means proved successful. Here the cause was violent, but not irritating, and had *stealthily* brought the whole system under its dominion. The sequel of such cases shows the correctness of my theory. Reaction in them is almost always healthy. Get the patient warm, and he is convalescent. This view of the subject is beautifully and forcibly, though unintentionally exemplified and sustained by the case of M. Brachet, as related by Watson. In this instance reaction and perspiration followed invariably the cold produced by bathing.

The second circumstance upon which this charming writer bases his scepticism is that chilliness does not always precede reaction. Although this is true, his own authority will sustain the assertion, that reaction is invariably preceded by depression. It is frequently discovered in those fevers previously thought to be continued, when by becoming dangerous or protracted they excite stricter scrutiny, that at a certain period of the day, a slight coldness in his fingers, toes and extremity of the nose, imperceptible to the patient, but quite perceptible to the touch of a by-stander, takes place; being immediately followed by a violent exacerbation. This depression is congestion or produces it. It, in *some de-*

græcæ, precedes all fevers whatever the cause, and occurs much oftener and more regularly throughout the course of those excited by malaria, than is usually supposed. The fact is, I believe, an ague fit is the true representative of all miasmatic fevers, the difference consisting chiefly in the prominence and shortness of its symptoms. The cold, the heat and the sweat being too palpable to escape observation. In remittent fever these stages occur, but are masked. In continued fever a "febrile oppression" or depression constitutes *the first*. The three, seven, nine or twenty one days of excitement, *the second*; the period of restoration, *the third stage*. The necessity for profuse sweating being rendered unnecessary to its gradual appearance and long continuance, and by the depleting and curative treatment.

In congestive fever, death cuts short the disease in its first stage, or medicine eradicates it by the time it has restored the system to the point of reaction.

Whether this theory be correct or not, it is *undisputed* that the more marked the state of depression or congestion, the greater the necessity for quinine and the more certain and efficacious its action. This state is one of *debility*. To resist debility a tonic or stimulant is indicated. Quinine has been found by experience to meet this indication. It therefore is a tonic. This appears exceedingly plain. The weight of authorities, too, accords with this view of the question.

Williams, Brown and Pereira call it tonic as though the fact were indisputable. Eberle, Wood, Dunglison, Gerhard agree with them. Paris styles it "eminently tonic." Morton (*vide* McIntosh's Practice) says, the same rule must regulate its employment in febrile excitement which governs that of all *stimulants*.

In opposition to these sentiments a few affirm that quinine is a sedative. "A sedative in whatever dose it may be given is never followed by the slightest indication of excitement: *it directly and primarily DEPRESSES the powers of life*." (Paris) A chill or state of congestion is a state of depression. Quinine is calculated to produce depression. Therefore it will prevent or relieve it. This is homœopathic logic with a vengeance. Its correctness, if established, would place that system of quackery upon an immoveable basis—the basis of truth.

Imagine, if imagination be capable of such a feat, Dr. Bell in consultation with a village Doctor, who has imbibed *his doctrine* relative to this specific. The case one of congestive chill. The first has passed off leaving the patient composed, but *Feeble*. Both physicians believe that the occurrence of another will prove fatal. They proceed to consider the means of preventing it. Dr. Bell commences the consultation by saying:

"This sir is a plain case. Quinine is the remedy."

V. D.—"I agree with you, but I administered my last dose of this medicine yesterday, and the village apothecary is out; none can be obtained in time. To meet the emergency I have provided myself with Hydrocyanic Acid."

Dr. B.—"Hydrocyanic Acid!" (with a sneer.)

V. D.—"Yes sir, Hydrocyanic Acid. Quinine, you teach, prevents a chill and relieves congestion by its sedative action. This acid is a

sedative. It therefore acts on the same principle. Why not try it!

Dr. B.—“I will not argue with you, sir. I won't, however retract my opinion. Quinine is a sedative, but it will not answer in a case like this to risk any other of the class. In its absence I propose Opium.”

V. D.—“Opium! Why that is a powerful stimulant. And I should think where a sedative was the certain remedy a stimulant in the same stage of the same case would injure or kill. But you know, Doctor, and I'll give any thing you say.”

Dr. B.—“Well, then, I suggest that you give the opium and conjointly camphor, or Virginia snake root or cayenne pepper tea. The last two articles alone are very efficacious in such cases.”

V. D.—“Well, Doctor, as I said before, I know you know, but it puzzles me somewhat, that quinine should be a sedative and yet that its adjuvants and substitutes should invariably be tonics or stimulants. Fully impressed with its sedative power, I determined to act according to this impression and therefore brought with me the Hydrocyanic Acid. I now however begin to see into the matter a little. Quinine is a peculiar sedative, which acts *like or with or in the place of* tonics or stimulants, resisting debility and imparting strength, whereas Hydrocyanic Acid is a sedative, which produces sedative effects.”

Dr. B.—“You are right, sir, you have caught the idea. You take a more comprehensive view of this subject than most of your brethren, you certainly must be in the habit of *studying* those *LONG* articles of mine among which a *few* *SHORT* ones of Stokes' are interspersed.”

V. D.—“I am proud to say, you have been my teacher.”

The foregoing dialogue intimates what I intend to show more fully;—that sensible Physicians do not *always* practice what they preach. Dr. Bell pronounces quinine a sedative yet supplies its place with tonics or stimulants.

I am aware that this specific sometimes, yea frequently, acts as a sedative, if we use the word sedative as synonymous with quiescent. But even this is not its primary action. The case previously given to show its anti-congestive power is in point. I had one last month equally as much so. The patient, a stout negro man, (about 25 or 30) had a congestive chill about 2 o'clock, the day before I saw him. The chill was very violent, lasted two hours, was attended with cramps in the bowels and *excruciating pain in the head*. Pulse was hard and a little quicker than natural. He was bled by the 'overseer, (a pint of blood was supposed to have been taken), grew better under the bleeding. Immediately afterwards ipicac. was given, and repeated until it produced emesis: a great quantity of undigested food was thrown up. No fever followed. The pain in the head and cramps in the bowels passed off, after which he suffered excessively with pain in the knees. Had several good operations during the afternoon. About day-break pains in the knees ceased and again attacked the head, being very violent at intervals of ten or fifteen minutes, these intervals being occupied by stupor. This was the statement given when I arrived at 9 o'clock, A. M. The condition of the patient was then as follows: Pulse 76, natural in force, breathing 40 in the minute, complained of some pain across the breast after a long inspiration, when specially questioned. Tongue

covered with a black or dark brownish fur over the middle, somewhat dry—had a dead feel. Slight tonsillitis also existed. Spine was tender from one end to the other, extremities were rather cool, wrists a little moist. *The pain across the forehead was agonizing*, relieved however by the same intervals of stupor. Had him nearly covered with mustard plasters, applied hot bricks to his feet and gave him forty drops of laudanum and fifteen grains of quinine. An hour and three-quarters after called again. He now sleeps most of the time, says *his head hurts very little*: his skin is rather warmer, pulse the same. Repeat the dose, adding half a scruple of calomel. Half an hour after, his pulse is 120 in the minute and stronger, breathing 38. At the expiration of two hours from the administration of the last dose, gave him the same quantity of laudanum and calomel, and half the quantity of quinine. Skin now quite hot. The calomel was continued until 9 ij had been taken. The quinine was recommenced at 10 o'clock, P. M. by the attending Physician, who arrived after my departure, and gave it every two hours in five grain doses, then every hour, in combination with camphor and cayenne pepper, until my arrival.

At half-past 9 A. M., I found the patient's pulse 104 and strong, skin rather hot and slightly moist, tongue moist and not at all dark; spine scarcely tender, eyes not so red, quite sensible when spoken to, disposed to sleep, breathing natural, *head entirely easy*. I am not prepared to report this case farther. I was unexpectedly called to visit it again two days afterwards and then perceived no material change. The quinine had been discontinued. A few days subsequently I learned that the patient died. The fatal termination has, however, nothing to do with the action of the medicine up to the period of its discontinuance. The case seems to me to show clearly, how quinine sometimes produces quiescence; and yet, that that effect is the result of its anti-congestive or tonic action. The pain in the head was quieted, but the pulse was quickened and strengthened, and the extremities warmed. It is true, bleeding relieved the head one day and quinine the next, yet it does not follow that they should be classed together. Every one knows that bleeding is directly sedative and debilitating. None however dispute that it frequently acts indirectly as a tonic; and still no one wishes to class it with tonics. Neither should quinine, which is given usually to resist debility, and usually has that effect, be classed with sedatives, because in a particular pathological condition, it indirectly produces quiescence or sedation of one organ or the whole system. Two years ago, a case occurred in my practice, admirably calculated to establish both the stimulant and sedative influence of this medicine. A lady of nervous temperament and great delicacy of constitution, sank exhausted from a natural and very easy labor. She was with great difficulty revived and bid fair to do well. But in a few days a symptomatic fever of an irritative character commenced, and continued about ten days. It originated, I think, from retained and morbid lochia. On one afternoon her pulse became very quick and very weak, a clammy perspiration covered the surface; her countenance was cadaverous. I sent for a professional brother, and in the meantime gave in a full dose of elixir vitrol, three grains of quinine, a large dose for her. In an hour her pulse became fuller, stronger, and twenty beats slower. The

Physician sent for arrived, approved my theory of the case, but asked, why I gave quinine. I replied, because it is tonic. He smiled and proposed wine. At the expiration of two hours it was given and with similar effect. Telling the patient our difference of opinion, these remedies were, when circumstances required their exhibition, with her consent, administered alternately. No difference in their operation was discoverable except that quinine produced buzzing in the head and she thought its effect more permanent. This case evidently *demand*ed tonic or stimulant treatment; quinine and wine were tried alternately: their action was almost identical. The consulting physician thought it demonstrated very clearly, that quinine was a sedative, but he did not deny that it was an equally plain demonstration of the strengthening and stimulating power of wine. The fact is, the ultimate effect of both was quiescence or sedation. But this condition was the result of the tonic previously imparted to the nervous system. A convalescent is *weak*, his nerves are irritable. The least exertion agitates, and then depresses him. Give him Comp. Tinct. Gentian, and improve his diet. Under the use of this regimen he becomes strong, and with the return of strength the weakness and nervous irritability disappears. Here the effect is sedation. Would it not, however, be absurd on that account to deny that the treatment was tonic? In this, but in no other way is quinine a sedative.

Opium possesses along with its stimulant property, a power to ease pain, to calm nervous irritability, to stupify the nervous system. From this circumstance it is sometimes, but not with technical propriety, called sedative. The ease, the calm, the stupefaction it produces, are merely different degrees of narcotism. Quinine does not approach the class of sedatives so nearly as opium. It has no direct tendency to relieve pain. Who would think of putting it in an aching tooth, or expect it to ease the pain of ordinary colic or dysentery. The calm that sometimes follows its use we have shown to be a secondary effect. Nobody accuses it of possessing narcotic power, such an accusation on the part of the "contra stimulant advocates" would be fatal to their cause. For Paris says, "a narcotic in small doses *never fails to increase the vital force*" and the profession coincides with him. Notwithstanding this, Dr. Bell intimates that quinine has much more affinity with "*narcotic sedatives*" than with the sub-class of stimulants, but about forty pages further on he adds: "it limits its action very much to the nervous system, producing *neither* NARCOTISM on the one hand, nor vascular excitement on the other." We may therefore conclude from our own reasoning, and by the authority of Paris, the profession, and our most formidable opponent, that if quinine be a sedative, it does not belong to the same class as opium,—the class of narcotics. Dr. Bartlett does not condescend to argue this question, nor does he call quinine a sedative. He modestly says, "certainly there is no propriety in regarding it as a simple tonic or stimulant. In congestive fever, at least, it *does not* act as a tonic or stimulant; and no known tonic or stimulant can be substituted for it, or supply its place. It is a specific anti-periodic. It is endowed with the peculiar property of arresting or counteracting this pathological process, characterized by periodicity; it stands in a specific relation to this particular form of disease, and

this is the entire sum and substance of *our* knowledge of the matter—just as easily packed in a nut shell, as blown out into an empty balloon.”

This paragraph contains more dogmatism than philosophy. The philosopher loves investigation. It is calculated to supply it. A specific is a something, in the existence of which I do not believe. This infidelity is becoming common. Quinine alone gives support to the old notion, and Dr. Bartlett in another part of his work admits, and so does every other author, that it is remedial in diseases *NOT* “characterized by periodicity.” The case in which wine was alternated with it, confirms the general experience. Mortification, *passive* hemorrhages, profuse mucous discharges are not thus characterized. Yet Pereira pronounces quinine useful in such cases, “*if tonics and astringents are obviously indicated.*” Purulent ophthalmia of new born infants does not call for “a specific anti-periodic.” Yet Dunglison says “should there be a threatening of disorganization of the cornea, and along with this concomitant symptoms of *want of tone*, it may be advisable to administer tonics, or *sulphate of quinine.*”

In the summer of 1841, I was called a little after sunrise to see a sick man. I found him in complete collapse from active and neglected diarrhœa, covered him with mustard, directed his attendants to use repeatedly stimulating frictions to the extremities, (telling them to rub the skin off if they could,) and gave him conjointly with opium and calomel, three grains of camphor, eight grains of carbonate of ammonia, ten grains of quinine every two hours, and brandy ad libitum. After having taken about eighteen grains of camphor, forty of ammonia, sixty of quinine and a quart of brandy, the collapse still continuing, being unable to rouse him up to take his medicine, and thinking I heard the death-rattle, I desisted, and pronounced him in articulo-mortis. Returning in a half an hour to his bedside I found him still alive, and thought I perceived as light amelioration, a more distinct flutter of the pulse and little diminution of the stupor. A blister of flies was applied to the back of his neck and over his abdomen. The frictions with the cayenne pepper tea were recommenced, and the remedies above mentioned continued irregularly, until the clammy sweat disappeared, the surface became warm and the pulse distinct. Febrile reaction succeeded during the night and lasted several days. Under a cooling, soothing and attentive treatment, the irritation or inflammation of the stomach and bowels, which had supervened, had subsided, and he recovered. Now this patient's struggle for life was extremely hard and its success extremely doubtful, but if quinine “*directly and primarily DEPRESSES the powers of life,*” every grain administered would have rendered that struggle harder, and the quantity taken must, it would seem, have made failure inevitable. Were it only “a specific anti-periodic,” it would have been superfluous, and the administration of superfluous medicine in a case, where *so much* had to be taken, would be an absurdity, if not a crime. The quinine either had no effect, or it aided or diminished the disposition to reaction. Upon the first supposition the ideas just expressed recur. According to the last, its operation would have been injurious, perhaps fatal. If it aided the action of the stimulants, having no power to regulate the secretions or restrain diarrhœa, it must have done so by acting

similarly to them. This view, whether right or wrong, is sustained by the highest opposing authority. Dr. Bell quotes from Dr. May, in illustration of the operation and potency of quinine, the following statement. I give you the substance. The disease was fever of "a malignant character of *three weeks duration*;" "*the patient was in a state of EXTREME PROSTRATION, pulse 108 in a minute and exceedingly small and compressible, copious watery discharges passing from his bowels six or seven times a day, as he lay, without the power to control them, and by no means the least unfavorable symptoms were colliquative sweats, parched lips, an utter inability to sleep, which had lasted several days with delirium at night.*" 48 grains of quinine were given in two hours. "*The effects were such, that in one hour after the patient had taken the third portion his pulse was reduced to 88 in the minute, with a more than corresponding increase in volume,*" that is, became twenty beats slower, and *a great deal stronger.*

The foregoing were extreme cases. The least aggravation would have been death. "*Extreme prostration,*" *real debility* had to be combatted. Neither were characterized by periodicity. In one, quinine was used in combination with the most powerful stimulants, in the other *alone.* In both it relieved debility. *Did it do so by weakening or strengthening?*

These cases are not solitary; nor is the practice now singular.

The theories I am opposing are invariably contradicted by the practice of those who maintain them. Thus, Dr. Bell, who denies that quinine is tonic, affirms that it gave tone to the system of a patient who had been seriously sick "*three weeks*" and was in a state of "*extreme prostration.*" He also takes care to premise bleeding in cases of excitement.

M. Maillot, who, if I have not misunderstood the matter, actually conceived about the same time, without any intercommunication, the same notions, takes a similar precaution, and unfortunately for their propagation, states, that a patient afflicted with "the algide form" of fever, under enormous doses of this *sedative*, "began in a few hours to *recover warmth and the heart to act more forcibly.*" The next morning the amendment being slight, quinine was again given. "*Strong reaction then took place and recovery commenced.*" The only adjuncts were ether and counterirritants.

Bartlett, who denies that it is tonic or stimulant, and asserts that the term antiperiodic expresses all its power, calls it "the sheet anchor" in the cold fit of congestive fever, and advises it to be "thrown out at once," *in conjunction* with external and internal stimuli. He also quotes in his treatise on Typhus Fever with great approbation the following remarks of Dr. Gerhard: "It is difficult to conceive the *extreme prostration*, in which our patients were left *after* a severe attack of fever. The skin is usually cool, and the pulse weak and fluttering, but there are still muttering delirium, and great feebleness. Under these circumstances, wine, combined with quinine, and a nutritious diet, produced an effect almost magical."

Dr. Boling, of Alabama, in giving his opinion expresses these contradictions with singular succinctness and candor. "As to the *modus operandi* of quinine" says he, "I have been able to form *no very satis-*

factory opinion; the observations of one day generally altering or modifying the opinions predicted upon the experience of a previous day. *At one time I was disposed to look upon it as a sedative or contra-stimulant*, and as a general rule this is its most manifest effect; and yet I have seen very few cases where it appeared to act as a stimulant. Its most general effect, however, is that of a sedative, *more certainly reducing* and controlling the action of the heart and arteries than any remedy with which I am acquainted.

This quotation contains three distinct announcements. 1st. That the Doctor could "form no very satisfactory opinion" concerning the *modus operandi* of quinine. 2nd. That he "*was disposed to look upon it as a sedative*" though it sometimes "appeared to act as a stimulant." 3rd. That it was *the most certain* and powerful sedative, with which he was acquainted. This latter proposition having been demonstrated by premising *the stimulant digitalis*, or combining with the quinine *the stimuli*, calomel, tartar emetic and nitrate of potash.

Whatever is true is reasonable. Reason and experience always coincide. If quinine were a sedative in any sense of the term, the higher the arterial excitement, no matter what the cause, the more appropriate and efficacious would be its action. Experience has proved that this is not so: every one gives it in the calm between ague fits. No body gives it in the hot stage of an ague fit. Experience therefore says, that it is not a sedative, and reason responds, "Amen."

There are two ways to relieve congestion—to lessen the load of blood; or to strengthen the system, or the organ oppressed, so that it can raise or remove it. The first indication is answered by venesection and counterirritation. The second by affording an increase of power. Stimulants and tonics afford this power. Quinine does the same. It therefore does not *depress*, but *strengthen*.

Gangrene requires tonic treatment. I tried quinine in a bad case of this sort last year. It met my expectations. Samuel Cooper, Pereira and others advise its employment in such an affection.

A chill is a state of debility. "A person on the brink of a paroxysm of ague becomes *weak, languid, listless*;" (Watson). Again, the same author says:—"Among the circumstances which predispose to ague, *debility* has a powerful influence." Quinine prevents this condition and counteracts the predisposition spoken of. Does it accomplish these ends by *debilitating* or *invigorating*?

Look at this question in whatever light we may, (save the deceptive glare of prejudice); take Paris' or Webster's definition of sedative, still the same conclusions recur. The fact, *that quinine prevents, resists, removes DEBILITY*, stares us in the face.

I have dwelt thus long on this part of my subject, because the medical mind is wavering; right views relative to it are practically important, and *the new notion* is gaining ground, especially among the youth of the profession, causing a sinful waste and extravagant abuse of this costly and precious salt under consideration.

The question now arises: What sort of tonic is Quinine? It would be easy to reply, it is a tonic "*sui generis*," and stop; but I prefer continuing the investigation.

In fever, the first system attacked is the nervous. It becomes disa-

bled. The circulatory system being entirely dependent on it, can not properly perform its functions: It consequently becomes deranged, and as a necessary result of its continued derangement, the secretory system stops, or becomes disordered. Quinine has no tendency to excite or regulate the secretions. Its direct power over the circulation is excessively limited; so much so, that it is thought by some to exert a sedative, by others, a stimulant influence upon it. All, however, admit that its power is great, and its operation speedy. This power must certainly be expended on one or all of the three great systems. This we have seen is not done with regard to the two last. It must consequently be expended on the first mentioned system, and this is the nervous. For this reason, I consider it a nervous tonic.

This opinion is further confirmed by the mode of curing the pathological condition just described. These are two. 1st. By restoring the secretions and controlling the circulation by remedial measures—thus alleviating the secondary derangements and preventing their reaction upon, and aggravation of, the primary derangements. This plan holds death at bay, and thereby gives the nervous system time and a chance to recover its tone. Though a slow plan, it is the one we are usually compelled to adopt in continued fever. In those cases, however, in which the prostration is so great as to threaten immediate dissolution, and demand speedy relief, quinine, as previously noticed “is the sheet anchor.” It acts quickly and with power. A medicine can only act quickly and *generally* by acting upon and through the nervous system.

Again. “The earlier symptoms (of chill) are all indicative of debility, and a *depressed state of the nervous functions.*” Quinine prevents, and removes this “state of the nervous functions” without, as a general rule, producing any decided vascular excitement.

I might press this point further, but I believe all admit that quinine acts *primarily* and *mainly* upon the nervous system. All, consequently, who admit it to be tonic, will perceive the propriety of styling it a *nervous tonic*.

I have designedly used the words tonic, and stimulant, as synonymous. They are so. Every tonic is stimulant—every stimulant is tonic. A tonic is a slow stimulant. A stimulant is a fast tonic. The one is slow in its action, and permanent in its effects—the other quick and evanescent. But both stimulate. Quinine occupies a middle ground. It is more speedy in its operation, and evanescent in its effects, than the medicines usually classed under the head of “tonics.” It is less so than those usually classed as “stimulants.” But, the fact that it *possesses* the tonic or stimulant property, in a *modified* FORM, does not prove, that it is *destitute* of it. Another quality of quinine is astringency. This is presumable from its taste, or rather the corrugation produced by its application to the tongue. The correctness of this assumption is confirmed by its effects when taken internally. Every one of experience in the use of this drug, is aware, that the better the secretory system is performing its functions, the more proper its administration, and the more satisfactorily it fulfils the indication for which it is given. Every such one has seen the tongue and fauces become dry under its use, and the action of the liver diminish or cease. These effects arise at least

in part, from its astringency, and teach the propriety, and frequently the necessity of preceding it by, or combining it with, mercury. Hence also arises, in part, the very appropriate custom of conjoining it with minute doses of ipecac. when the skin is dry and there is no contra-indication present.

The astringency of quinine is sometimes directed to the kidneys with such force, as to cause suppression of urine. I had a case last January, in which this effect was produced. The patient was a negress, aged eleven years, quite delicate, disease remittent fever. The remissions were so decided, and the period of exacerbation so well marked, I thought it probable the attack might be cut short. Prescribed quinine, grs. ij. every two hours during the remission. It was given twice in this way until ten successive doses were taken, without producing the slightest effect upon the head or the fever. The paroxysm still recurring, I ordered three grains of quinine to be given at a dose. This was repeated at the same intervals of time, until the quantity taken amounted to forty-five grains. This prevented the paroxysms, but produced no deafness or pain in the head; says, when her attention is specially directed to it she hears some roaring. The urine, however, ceased to flow during its administration. She passed none for twenty-four hours, then discharged, without complaining of pain, a small quantity of blood. No more quinine was prescribed. Under the use of flaxseed tea and spts. of nitre, the secretion was soon restored; the hæmaturia did not again appear. The slight characteristic effects occasioned by the medicine in this case, may be accounted for by its determination to the kidneys; producing the hemorrhage, by its acting as a local irritant after causing suppression. Four cases in which quinine produced similar effects, are reported in the "London Lancet," for November 1847.

Cases, apparently conflicting with these have been kindly reported to me by Dr. McLeod, a respectable physician of this vicinity. The contradiction, however, is only apparent. The quinine may have brought on the hemorrhage from the womb, as from the kidneys, by being absorbed into it, and then acting as a local irritant—or it may have done so by disgorging other internal organs, and throwing an undue quantity of blood upon them.

As these cases are novel and mysterious I will detail them. The three first occurred in South Alabama, the three last in this Parish.

Case 1st. Mrs. L., (ætat. 45,) spare, but of good constitution, had enjoyed good health until attacked with Tertian Ague, on the 2nd of September, 1845, at 10 o'clock, A. M., September 4th, arrived just as the cold stage had passed off—ordered cold drinks during the day, and fifteen grains of calomel at night, to be followed by Ol. Ricin, $\frac{3}{4}$ ss. in the morning.

September 5th. Patient has no fever, is quite comfortable, medicine operated very well; ordered sulph. quinine, grs. xv., to be divided into two portions, one to be taken at 6, the other at 8 o'clock, the next morning.

Sept. 6th, afternoon; was sent for in haste, the patient had missed her chill and had no fever, "but stated that soon after taking the last dose of quinine her head had become very much affected, and the menses

had come on, a thing which had not happened *for three years* ;” requested her to keep quiet. Called the next day. The discharge had ceased ; it never returned. The lady speedily regained her health.

Case 2nd. A negro girl (aged 22 or 23) who was generally healthy and menstruated regularly, was attacked with intermittent fever a few days ago.

October, 23. Ordered calomel grs. xx. at night, and a small dose of oil in the morning ; also ten grains of Quinine at 8, and the same quantity at 10. (Noon being the usual time for the chill.)

Oct. 24th afternoon. Patient has missed the paroxysm, “but soon after taking the last dose of quinine her menses came on, although her regular time had been some eight or ten days before.”

This woman soon recovered and afterwards menstruated regularly.

Case 3d. Mrs. D. (aged 19) was healthy and menstruated regularly.

August 9th, 1846, had a chill at 9 A. M., took at bed time, calomel, grs. x., senna tea in the morning. August 10th. Prescribed 10 grains of quinine at 5, and the same at 7 A. M. “In the afternoon found my patient had missed both chill and fever ; but soon after coming under the influence of quinine, or as she expressed it, when her head commenced roaring, her menses came on notwithstanding the regular time was only two weeks past.”

August 18th. This patient was again attacked ; 15 grs. of quinine were prescribed with like effects.

A few weeks afterwards she was again attacked but refused to take quinine in any form, because she believed it would bring on her courses.

Case 4th. August 18th 1847. Mrs. ——— was attacked with chill at 9 A. M., took calomel at bed time. It operated freely, causing great irritability of the stomach which passed off in the course of the day.

20th. Prescribed two doses of quinine, ten grains each, the first to be taken at 5, the last at 7 A. M.

Soon after she took the second dose “her menses” appeared. The paroxysm did not recur. This lady’s age is about 38, habit plethoric, general health first-rate. She had menstruated about two weeks previously.

After the lapse of a week she was attacked again. Fifteen grains of quinine divided into two portions, having been preceded “by a little preparatory treatment,” were administered and followed by similar consequences,—the prevention of the chill and the appearance of the menses.

This lady likewise in a subsequent attack refused to take quinine on account of the last effect.

Case 5th. Mrs. ———, daughter of the lady mentioned in case 4th, aged 22, previously healthy, was attacked with chill on the 24th of August 1847 ; prescribed mercurial cathartic at night.

Aug. 25th. Chill recurs.

Aug. 26th. Gave a scruple of quinine in two hours. Chill was prevented, but discharge from the womb appeared soon after the second and last dose. This lady had her courses two weeks before this attack, and there had been previously no irregularity.

August 27. The hemorrhage has ceased.

Case 6th. A negro woman, fifty two or three years of age, who had not menstruated for *six or seven years* and whose health was generally

good, had a chill on the morning of July 18th 1847. Her bowels being costive, an ounce of castor oil was prescribed during the intermission. It operated freely.

July 19th. Four grains of quinine were given at 4 A. M. and repeated at intervals of two hours until three doses had been taken. At half past eleven, chill recurred. Ordered calomel $\mathfrak{z}$ i. at bed time, and 10 grs. of quinine at $\frac{1}{2}$ after 6 next morning, to be repeated in two hours.

July 20th. Had no chill. Quinine affected her head greatly. The last was soon succeeded by a discharge of blood from the womb, which "continued until the effects of the medicine wore off."

July 26. This woman had another attack. Quinine was prescribed as first stated. It produced "the same unpleasant feelings of the head "and "flow of the menses."

About a month subsequently the old woman had a third attack, which was treated similarly and with the same results.

Since that time she has not been sick and "there has been no sign of any menstrual flow."

Dr. McLeod remarks that he is not prepared to say positively that the discharge which followed the use of quinine was the menstrual secretion "but at all events it was bloody and so very similar that it would be very hard to distinguish the one from the other." This is his only comment. I have none to add.

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My theory of the modus operandi of quinine is the result of my experience. The circumstances under which I apply it may therefore be easily inferred. As an anti-periodic I deem it applicable to all affections characterized by periodicity, regardless of their name, location or supposed nature.

As an anti-congestive it is applicable to all cases of *acute* congestion. I mean to describe by the word "acute" those cases, in which the congestion appears suddenly, or admits of or requires speedy relief. The quinine to precede, accompany or follow depleting, mercurial or other remedies, according to the time allowed and the condition of the patient. When it is admissible the last is the most judicious course.

The anti-periodic and anti-congestive powers of quinine being mere results of a tonic or stimulant property, I esteem it an appropriate remedy in debility. As however it is a tonic or stimulant *sui generis*, acting with less rapidity than the last, and less permanency than the first class, common sense would suggest that medicines plainly belonging to one or the other of these classes should be selected, where rapidity of stimulation or permanency of effect is desirable.

The astringency of quinine and the fact that it sometimes acts as a local irritant add weight to these considerations.

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In intermittent fever small doses at short intervals during a great part of, or the entire intermission, are preferable to large ones, at long intervals, or just before the approaching paroxysm. The small doses *generally* affect the head less, and their prolonged administration prevents the

accession of the paroxysm earlier than had been anticipated. My plan is to commence with the quinine sixteen or eighteen hours before the usual time of its appearance, and give three grains every two hours until 3 ss. has been taken. By this means I receive the advantages just mentioned, and also guard against the mere delay of the paroxysm.

I do not however forget that "circumstances alter cases" and vary accordingly. In West Tennessee doses one third less are equally efficacious.

Quinine is not called for in continued fever except to answer some casual indication; or to resist or remove debility or "the extreme prostration" spoken of by Bell and Gerhard. In Remittent fever it is generally objectionable. The disease cannot be *safely* cut short. This is the general rule. It is proper however to remark that I have seen cases apparently of this type arrested by quinine. For this reason, I always try it when the attack is unusually prolonged; or the remission decided and the secretory system in good condition. If the *experiment* fails, and is of no perceptible disadvantage, I repeat it in the next remission; but not again unless a change takes place. By this course I never do the patient any permanent, and rarely any temporary, injury. The practice here recommended must not be considered entirely applicable to cases characterized by congestion, or a *tendency* to it. In the fall of 1844, I attended cases having this tendency apparently in a very slight degree. In them the only alarming symptoms were, that the heat of the skin was not in proportion to the vascular excitement, or the strength of the pulse to its frequency. The former was nearly natural, the latter varied from 100 to 120 in the morning, rising towards noon to 120 or 140 in the minute, and was *rather* soft and compressible. The tongue was natural. The secretions and bowels easily regulated. Under the ordinary treatment the first two or three of these cases proved fatal. The last, though equally as violent were cured by keeping the patients *from the beginning to the end of the attack*, especially on the critical days, under the influence of Opium and quinine; enough calomel or blue mass being conjoined to counteract their astringency and keep up, correct or restore the secretions. One general remark and I have done. Periodicity is frequently smuggled into the system by intermixture with other diseases. In miasmatic regions it must *always* be looked for, and if discoverable or *even* suspected, the *experiment* of giving quinine should be fairly tried. The existence of inflammation will not prevent its utility. In such a complication the exacerbations aggravate the local affection more than the stimulant action of the medicine. I had three cases last month thus complicated. All had disease of the lungs. One was a plain case of Chronic Bronchitis. Under active depletant and alterative treatment, this affection was reduced to a certain point, but then remained stationary. Observing decided febrile excitement towards evening I gave quinine as for a chill. The fever did not reappear, and the patient convalesced. A week or so afterwards I found him, several successive afternoons, in a profuse sweat, unattended with and not preceded (so far I could learn) by fever. The quinine was given as before; the sweat did not again appear.

IV.—On the effects of Quinine in Puerperal Peritonitis, and other inflammatory diseases. By LEWIS SHANKS M. D., of Memphis, Tenn.

The following case so fully demonstrates the safety and utility of quinine, given in large doses, in Puerperal Peritonitis, that I have been induced to report it, with some additional remarks on its effects in other inflammatory diseases, as the result of my own experience and observations.

Harriett, a servant woman, aged about 30 years, had enjoyed good health during her pregnancy, with the exception of varicose veins in her leg, which had several times toward the close of her pregnancy given rise to hemorrhage by bursting. On the first day of April the hemorrhage was considerable, though not enough to produce any complaint of weakness.

April 3rd. Her child was born. Labour favorable. Hemorrhage moderately free.

April 6th. From the 3rd she was reported as having been comfortable—bowels constipated. On this day, in the evening, Dr. Patilla was called to see her. During the morning she had been chilly. In the evening—fever considerable—pulse frequent—pain in the hypogastric region very severe—tenderness very great and the Lochial discharge suspended.

Treatment. Anodines were freely administered.—Hot fomentations over the abdomen; to be followed by oil to move the bowels freely.

7th. The Lochial discharge partially restored—pain much mitigated.

8th. The oil had operated frequently and freely during the previous evening and night.

The discharges watery. Chilliness and coldness of the extremities through the morning. About 12 o'clock, the pain again became severe and Dr. Patilla was called to see her.

Between 3 and 4 in the evening I saw her with Dr. Patilla. At that time, although she had taken 3jss. of laudanum and some grs. xx. of Dov. powder with grs. xij. of calomel, and stimulating fomentations had been used, the pain was intense—constant complaint of suffering, and the countenance expressive of agony—tenderness over the whole abdomen so great that she could not bear to be touched—some distension—tongue, full size, thinly covered with a brownish coat and moist—pulse about 120, small but somewhat corded—the veins in the extremities and on the surface rather shrunk—external heat moderate—thirst considerable, and the Lochial discharge nearly or quite suspended.

Treatment. A grain of Sulph. morphine was administered. Thirty to thirty five ounces of blood were abstracted. Fomentations of a decoction of hops, as hot as could be borne, frequently reapplied over the abdomen. The pain not being relieved—in an hour, by these means, fifteen grains of quinine with nearly a grain of morphine were given. The same quantity of morphine to be repeated in another hour if the pain continued.

9. P. M. The Morphine had been given as directed. The pain still complained of; but not so great. Ten grains of quinine and twenty grains of calomel given—the fomentations to be continued, and the same quantity of quinine to be repeated at 5 in the morning.

9th. 8. A. M. Pain much less severe—tenderness still great, all over the abdomen.—Lochial discharge somewhat increased.—Pulse about 100, fuller and softer—superficial veins better filled—tongue coated, but moist—thirst less urgent—perspiration very free, and had been so through the latter part of the night. So decidedly under the influence of the quinine as to complain chiefly of the deafness and roaring in her ears, and confusion of her head.

Treatment. Quinine and calomel each grs. vj. with $\frac{1}{2}$ of a grain of morphine to be given regularly every four hours. The fomentations to be resumed if the perspiration ceased and the pain returned or increased.

6. P. M. Complains of the deafness and roaring in her head. Pain and tenderness diminished.—Abdomen less tense.—Pulse 90, soft and full.—Perspiration copious and has continued so through the day. Two free dark bilious passages since the morning. The Lochial discharge increased.

Treatment. The same powders to be continued every four hours until 9 o'clock in the morning.

10th. 10. A. M. Symptoms improved. Complains chiefly of the confusion in her head produced by the quinine.—Pulse 85.—Skin still perspirable. Medicine suspended.

6. P. M. Decidedly better—pulse 85—dark discharge from her bowels—Lochial discharge in moderate quantity. Pain relieved—tenderness diminished. Some signs of pytalism.

Treatment. Quinine grs. v., morphine gr. $\frac{1}{2}$ to be given at bed time; and the same quantity of quinine without the morphine to be given three times the next day, and once the following morning.

The soariness and tenderness of the abdomen gradually subsided, and convalescence was established, with but slight pytalism.

Remarks. Within 36 hours from the time the quinine was commenced, she took 70 grains, and in all about 100 grains.

It was given in large, sedative doses, at first in this case, in the height of the inflammatory stage of the disease, not only from the theoretical belief of its anodine and antiphlogistic influence, but from its effects in a similar case, though not so violent, which I reported in the New-Orleans Journal for 1847, in which it was relied upon alone after depletion; but in larger doses at first. Thirty five grains were given when the pain was very severe at 10 A. M., and 10 grains at 9 P. M., and 10 grains repeated twice next day, without any combination, as the patient's stomach could not tolerate the use of opiates in any form on account of the excessive nausea and vomiting they produced, and mercurials were not deemed necessary.

In that case, as in this, when the system was brought so fully under the influence of the quinine, as to diminish the general nervous sensibility, which was evinced by the relief of pain, and the insensibility to sound, profuse and continued perspiration was produced, more like the perspiration produced by the full influence of Tartarised Antimony, than any thing else. To this sedative and sudorific effect of large doses of quinine is its curative influence, in cases of extensive inflammation of serous membranes, to be attributed; for by its influence, used in that way, vascular action, produced by general nervous excitement from local irritation, or inflammation, after sufficient abstraction of blood, may not only

be controlled, but its cause removed, and its effects relieved, by its influence in subduing morbid excitability—equalising the circulation—producing copious perspiration, and thus relieving the congestion and inflammation of the interior serous membrane and other viscera, and preventing their consequences—adhesion—effusion—suppuration, &c.

It may be supposed that the urgent symptoms in this case were paroxysmal, and that the influence of the quinine in curing the disease was owing to that form. Against the probability of this may be urged as a convincing objection, the fact, that she had no previous exposure, attacks, or habit, indicating a tendency in the system to an intermittent form of disease.

The effects of quinine in relieving pain and curing diseases are not limited to those cases alone which are paroxysmal; and its employment confined necessarily or even properly to the intermissions of pain or of diseases.

If the experience of every physician does not afford such results, in the reported cases in the Medical Journals of the day, ample testimony is furnished, upon good authority, that quinine given in full doses will relieve neuralgic pain—the pain from inflammation of the frontal sinuses—the pain from boils, and almost every description of local pain; not only, when it is intermittent, but when continued; and not only when given in the intermission, but given during the paroxysm or the time of the greatest amount of suffering.

In demonstration of the unequivocal effects of quinine in relieving pain and inflammation of the most delicate and important organs, the following case is very striking and conclusive.

Mr. C., aged about 35 years—a native of Virginia—of good constitution, had resided in Memphis about two months, when he was attacked with an inflammatory remittent fever, in the month of July 1837. The case was permitted to progress three days, with no other treatment, than the use of mild laxatives.

On the fourth day of the disease, in the evening, when I was called to see him, the fever was very high—decidedly inflammatory—pulse frequent and corded, and pain in the head very severe, accompanied with great sensibility to noise and light.

I bled him freely—administered active cathartics—kept cold applications to the head etc.

The next morning the fever was still high—pain in the head severe, and had been so much so through the night as to prevent him from sleeping.

He was bled again freely, and put upon the use of Tartar Emetic in broken doses, so as to produce vomiting and purging. In the evening pulse still tense—pain in the head very severe—sensibility to light and noise increased, compared with the previous evening. He was bled again, and after the bleeding directed to take 10 grains of quinine every three hours until its influence was decidedly established.

The next morning he complained chiefly of the roaring in his head and the deafness, in place of the pain and excessive sensibility to noise and light, which he suffered so much from the previous day. The surface was relaxed, and the perspiration free and comfortable.

The quinine was continued in 10-grain doses repeated three times during the day and twice the next day, to keep up its influence and re-

lieve the inflammatory action, by its sedative and diaphoretic, and indeed by its febrifuge and general evacuant influence; as all the secretions and excretions of the system seemed to be increased by it. There was no return of pain in the head after his system was fully impressed with quinine. Convalescence was favorable and rapid.

In further illustration of the result, of many years careful observation and experience of the effects of large doses of quinine, so repeated as to produce, and keep up for a time, a decided impression, it is proper to state, that it can only be used in that way with safety and benefit, in cases where there is considerable vigor of constitution, and more than the normal state of vascular excitement, at the time of its employment, or expected to occur by the time the system is impressed with its febrifuge influence. If this condition of the system does not exist, and especially if it be much weakened by previous disease, quinine given in large doses may so overwhelm the nervous system, as to place the whole organism below the point of healthy or curative functional action.

Under these circumstances, and in conditions of the system, not tolerating its use in large doses, I have seen cases, when the frequent, small and feeble pulse, the cold extremities, the relaxed skin, the profuse perspiration, and all the symptoms indicating collapse, and a fatal giving way of the vital powers, were doubtless, hastened, if not positively produced by the injudicious employment of large doses of quinine; and death resulted from its depressing and poisonous influence having been carried beyond the point for healthful reaction.

It is not designed in these remarks more than to allude to the circumstances and conditions of the system, which contra-indicate the free use of quinine, as unsafe and injurious. The great importance and value I have attached to its free exhibition, in diseases and conditions of the system, regarded formerly and even now, by many physicians, as prohibiting its use, might tend to create the belief, that it is designed to extend and widen out the varieties of diseases and conditions of the system, to which its curative effects are peculiarly adapted. This is not so; for careful observations of its effects, given as a tonic in the many varieties and forms of chronic diseases, for which it is recommended in the Books have so invariably led to the conviction of its inutility or great inferiority to other articles, in the accomplishment of the results desired, that its use in that way I have in a great measure abandoned.

To control and arrest paroxysmal diseases of the nervous and vascular systems, and some forms of inflammatory and neuralgic diseases, that are more continued in their character, there is no remedy that can be made to supply its place satisfactorily; but in producing these results, its salutary influence may and ought to be obtained in two or three days, by giving it in the proper quantities; guarding against its exhibition or continuance in this way, so as to prevent hazardous effects.

If the consequences recited result from its improper use in enfeebled constitutions, the propriety or even safety of using it in large doses, as recommended by some physicians, in well marked *congestive fever*, may be questioned, for the symptoms and condition of the system in that form of disease indicate, that the cause has produced a depression of the vital energies, somewhat similar to that resulting from the abuse of quinine.

My own observations have confirmed the opinion, that in well established congestive fever, with the enervation of the system, which causes and accompanies it, in the cases I have seen, in which quinine has been used, it has not been beneficial; but when given in large and repeated doses, it has exerted a decidedly depressing and injurious, if not a fatal influence.

V.—*Anthropo-toxicologia. Cases; with Remarks. Read before the Alabama Medical Society, April 3rd., 1848. By C. E. LAVENDER, M. D., of Selma, Ala.*

Not being able to find, in the works of Nosologists, a name which conveys what I conceive to be the cause and pathological nature of the following cases, I am forced to adopt, or rather to coin from Greek a word somewhat in accordance with my views. If I rightly apprehend the nature of these cases, my limited reading does not enable me to remember any recorded cases similar to them. Nor have I been able to find any allusion to a pathological condition, which I have supposed to exist in these cases. I make these remarks at the risk of being written down a very limited reader. I incur the risk, however, with the hope of being enlightened on the subject; and for the purpose of excusing myself for the apparent pedantry of employing, in this advanced age, a new term in medicine.

The term *Anthropo-toxicologia*, it will be seen, is a Greek derivative, from *Anthropos*, man, and *toxicon*, poison, and it is intended to convey an idea of that form of *poison* generated in, or secreted from, one healthy person capable of producing disease in another human being subject to its influence.

That the human body in a state of disease is capable of sending forth a contagious or infectious poison, is familiar to every one. That the natural and normal secretions of certain animals are poisonous and will, when received in the human system, produce disease and death, is equally true. But that the secretions or exhalations from some human beings in health, are so virulent and noxious as to cause disease in other healthy persons, is a position that will not readily be conceded, and must therefore be examined. For the purpose of throwing light upon this subject, I offer the following cases, which I think will be found interesting, whatever theory may be employed in explaining them.

Case I. Mrs. E. K., æt. 25, good constitution, mixed temperament, black hair and eyes, rosy cheeks, rather full habit, cheerful temper, uninterrupted health, was married to P. K. early in 1845. Soon became pregnant and began to decline; became pale and hydropic; in a few months abortion followed. Bad health continued; some improvement, and in the fall of '45 again became *enciente*. Temperament now leucophlegmatic. Various diuretics and tonics used with but little benefit. Feby. 11th, '46, delivered of a still born child by a premature birth. Nothing extraordinary attended or followed parturition, save the fact that no red blood followed the cutting of the cord or the removal of the

placenta. Quite exhausted, but cheerful. R. Nutritive diet, elder wine, laxats., chalybeates.

15th. Found her cheerful, complaining of little pain, but much oppression, and at times drowsiness; but little derangement of secretions; pulse 110, feeble, compressible and undulating; sighs often and turns from side to side; deadly paleness and slight swelling about the eyes; tongue clean, but pale and colourless; little appetite, no nausea; roaring and uneasy feeling in the head, but no pain; light somewhat unpleasant to the eyes; *light and all white objects appear yellow; indistinctness of vision.* R. Wine, veg. bitters, tr. muriat. ferri, B. mass, pul. dover.

16th. Pulse more frequent, compressible and intermitting; more oppression, vision indistinct; *light looks red, white curtains appear red, dark objects yellow, or dull red*; pupils dilated. Complains of no pain, but is more restless and desires to be turned oftener. R. Op. camph. wine, min. acids, light mercurials and counter-irritants.

17th. Has sunk rapidly since yesterday. Nothing that she has taken has had any visible effect. *Light as red as scarlet*; some wandering of intellect; breathing more laborious. Sunk gradually and without complaint till she expired.

Case II. Mrs. S. K., second wife of this same P. K., æt. circ. 25, sang. nerv. temperament, good health, sound constitution; accustomed to manual labour; married in July, '46, became pregnant and began to decline. Took sundry patent nostrums, which caused further prostration. In about eight months hydropic symptoms supervened.

April 10th. Delivered of a living male child. No attendant circumstance worthy of remark. Directed magnesia, nourishing diet, etc.

17th. Found her debilitated and oppressed; no acute pain, but dull aching in the head; hydropic symptoms; appetite moderate; secretions scanty, otherwise normal; pulse 120, small and deep seated; light unpleasant to the eyes; some darkness and obscurity of vision.

R. Ars. sol., chalybeates, b. mass, wine, nourishing diet.

18th. Frequent and distressing attacks of dyspnœa, approaching syncope; otherwise as before. R. Pill. assafœt. every six hours, alternate with blue pill; wine occasionally.

19th. Attacks of dyspnœa frequent and distressing. Becomes cold occasionally, with feeble pulse, but no reaction; no action on bowels; lochial discharge free; roaring in ears and optic illusions; *can see but one half of an object and that looks green*; light, somewhat painful to the eye; stupid and sometimes comatose. R. nit. acid, b. mass, quin., morph., wine, arrow root, sac. sat.

20th. No improvement in any of the symptoms. Can see but half of a person's face, *which looks green with red spots*; pulse, as before, 120, feeble, intermitting. R. camph., t. op., t. stram., b. mass, wine; dry mustard to spine, blister to neck.

21st. No improvement; rather comatose; no complaint of head; *light and white objects red*; pulse 120; complains of the mouth. R. t. stram. 6 gla., t. op. 4 gla., ter die, wine, panada.

22d. But little alteration; rests well. Continue treatment.

23rd. A. M. Healthy bilious stool; more in her senses; rests well. At 4 P. M. an exceedingly offensive watery purging came on, which

caused great prostration ; moderated in a few hours by the use of *sac. sat. ap.* and *astgt. enemata*. *R.* Sinapisms, burnt brandy, camph., ammonia, *sac. sat.*, *tr. opi.*

24th. Purging moderated, but all the symptoms worse. Comatose, with optic illusions. Secretions from the skin, etc., so offensive as to require purifiers ; pulse 120, threadlike ; cold extremities ; apparently moribund. *R.* Quin. grs. 20 ; *Tr. opi.*, gta. 20 ; to be repeated in six hours.

25th. Has had several cold spells, with threatened spasm ; smell not so offensive, still necessary to keep something burning in the room ; blisters on legs and back filled with yellow water. *R.* Quin. gr. 10, g. camph. gr. 2, morph. gr. $\frac{1}{8}$, every 6 hours.

26th. Symptoms as before ; no improvement. Continue treatment.

27th. Rests quietly ; some light delirium ; offensive smell measurably removed ; takes panada, etc. Continue treatment.

28th. More in her senses ; still cadaverous look and optic illusions ; petechiæ and blisters continue. Complains of quinine affecting her head. No action on bowels for 48 hours. *R.* Nit. acid, A. M., Magnesia, P. M., Morph. at night.

29th. Found her narcotized. Cr. tr. and sulph. had been given last night, without my knowledge, which brought on excessive purging ; a large amount of opiates had been given to check it. The patient gradually sunk and died next morning.

Case III. Mrs. W.—This case, in its rise, progress, diagnostic symptoms and result, entirely similar to the foregoing.

Remarks.—The toxicological symptoms in these cases are the constant and gradual emaciation and waste of vitality, without any apparent fixed disease ; the optic illusions, especially the fact of *light appearing scarlet, and white red* ; the coincidence of the symptoms under similar circumstances ; and the fatal termination.

The anthropological facts are these : P. K., the husband of the two women whose cases are detailed (and the remarks will equally apply to D. W., the husband of the third,) is a man whose cutaneous secretions, especially under excitement, are known to be exceedingly offensive and disgusting. “Essentially an unclean animal, all the civet of the apothecary, mixed with the perfumes of Araby the blessed, cannot sweeten him.” Is it not possible, nay, is it not probable, that the inhalation of these gaseous secretions or the absorption of others, from this living laboratory of malaria, may have produced deleterious impressions on those constantly subjected to their influence, causing a specific and fatal train of morbid associations ?

These two women were in the prime of life, in robust health, of good constitution and of active, healthful habits. So soon as they came in contact with this man, they began to exhibit the effects of morbid causes. They both became leucophlegmatic, and continued constantly and steadily to decline. The same optic illusions, dimness of vision and perversion of colors, marked the progress of both cases to a fatal termination.

The matter of contagion is no more cognizable to our senses than malaria. Infectious disease is, however, usually attended by a certain very sensible factor. Persons whose bodies, in a state of health, are

capable of elaborating infection, as far as my observation extends, are distinguished by the strong and peculiarly offensive character of their pulmonary and cutaneous exhalations. Although this may not always be the case, yet so signally is it true in the cases before us, that I have heard sensible men say that, in their opinion, it would cause disease in any person who would room with them. I therefore set this morbid agent down in the catalogue of animal poisons, as human infection *sui generis*. What its nature, its essence—from what part of the body derived, and upon what tissue it first makes its attack, are, at present, matters of conjecture. Whether all persons are obnoxious to this poison, or whether there is a peculiarity in some, rendering them more susceptible of its action than others, time and observation must decide. In the cases above noted the noxious cause evinced its power by depressing the vital energies, impoverishing the vital fluid, and deranging the sensorium. There are, doubtless, instances in which the infection, not being sufficiently concentrated or virulent to cause fatal consequences, makes known its existence in the continued bad health and low spirits of its victim. And this, it may be, is the secret why so many hysterical cases are so difficult to be controlled, and why in such cases the experience of our profession finds it profitable to prescribe temporary *absque marito*.

I have brought these cases to the notice of the society in order to call attention to this peculiarity in the human structure, and to this form of disease. When the first case presented itself to me, I looked upon it as a case of hydropic affection with *remolissement* of the brain. When the next case occurred, with all the peculiarities of the first, I very naturally suspected a similarity of cause. No other known cause presenting itself I have deemed the one named sufficient to explain the phenomena. The third case, similar in all respects, both as regards cause and disease, strengthened my conviction. Nor is this conclusion, in my judgment, either rash or unreasonable. Personal cleanliness is conducive to health, the reverse predisposes to disease. Healthy persons crowded together in a tight room, or in the hold of a ship, will suffer and contract disease from exposure to their own secretions and exhalations. Writers on hygiene say it is hurtful to the health of the very young to sleep with the very old. It is not therefore more true that “evil communications corrupt good manners,” than it is that evil bodily associations impair the health and superinduce disease.

REMARKS.—The above cases are both curious and interesting, and we are disposed to think Dr. Lavender correct in ascribing the gradual decline and ultimate death of two of his cases, to the foetid and filthy exhalations arising from the body of their husband. The subject deserves consideration, and we hope the publication of these cases will excite the attention of physicians and elicit some further remarks on this curious question.

Whilst on this subject, it may not be out of place to allude to a singular prejudice which exists in Havana among those born of Spanish and native parents. Into the private and public hospitals of that city, nothing can induce a native of that Island to enter as a patient, if it is known that one or more cases of Phthisis are quartered in the institu-

tion; yet, at the same time, they neither fear the yellow fever nor any other form of febrile disease, although liable to contract such diseases. They argue that the exhalations, secretions, etc., etc. from the consumptive, not only predispose those confined in the same room to the same affection, but also deteriorates their general health, impairs the tone and vigor of their constitutions, and thus prevents their speedy convalescence from other diseases. Popular prejudice sometimes has truth for its foundation, and such may be the case in this instance.

We regret that no postmortem examination was made in these cases; as without this test they must be unsatisfactory and incomplete.

EDRS.

VI.—*A short account of the Epidemic which prevailed in Mobile in 1847.* By P. H. LEWIS, M. D., of Mobile, Ala.

As your Journal contains an account of every epidemic which has prevailed in Mobile since 1818, I wish, by way of keeping up its history in this locality, to record a few statistics concerning that which has just passed bye. Yellow fever presents at different times and under varying circumstances so many varieties and modifications, as to lead to conflicting conclusions among observers; to reconcile these and arrive at a correct knowledge of the disease in all its relations, it is necessary that it be observed and noticed for a series of years, as it occurs in some locality where it is endemic.

During the past summer the weather was exceedingly damp and cool; the rains began early in June, increased in July and continued throughout the greater part of August. The first case of yellow fever that was reported to the Board of Health terminated fatally on the 2d of August. In two or three days another case was reported which was followed by several others. As usual, on such occasions, the citizens became very much alarmed, and the Doctors engaged in unprofitable discussions as to the true character of the disease. Amidst the wrangling of the faculty and the hopes and fears of the citizens, the fever gradually increased, and by the 28th August presented such uniformity in its symptoms, that the Board of Health announced that it had become "epidemic, but was of an exceedingly mild and manageable character."

For the last 7 or 8 years slave labor has been taking the place of white;—this circumstance, connected with the fact that the city has been in a condition not to afford work for large masses of laboring people, has prevented any influx of that class of persons who usually constitute the material for malignant epidemics. In view of this condition of things medical men had little apprehension of the prevalence of a fatal epidemic:—nor were they disappointed;—for notwithstanding the alarm of the people, the exaggerated reports that were abroad, and the great number of persons attacked, I will venture the assertion that a more non-malignant, harmless, endemico-epidemic has ever prevailed in any climate or country.

The first well marked case of yellow fever and those immediately succeeding, occurred in persons of intemperate habits, residing in the

Western part of the city, some distance from the bay or river. This section of the city, that is the Western and most remote from the commercial part, was the principal seat of infection. To the certain knowledge of the writer more than one half of the families residing on Franklin, Hamilton, St. Lawrence, Cedar and Warren streets, which are probably the most populous in the city, during the summer months, were visited in the month of September by epidemic fever. These streets run North and South. Before the close of autumn cases had occurred in every part of the city; but if those residing on the streets enumerated be excepted, the disease could not be properly classed among epidemics.

The fever ceased to prevail as an epidemic about the 10th October, some twenty-five days before frost. For about forty days preceding the 10th October there were no rains, the atmosphere was exceedingly dry and dews very light. It is usual for 40 or 50 days of very dry, cloudless weather, to put an end to this character of epidemics;—if the atmosphere continues laden with the same amount of moisture, or any thing approaching it, which invariably exists at the time the disease makes its appearance, there is no cessation of it until after a decided frost.

There was not this summer that marked increase of periodical fever which usually ushers in a yellow fever epidemic—but after the decline of yellow fever (the 10th October) there was a rapid increase in the cases of bilious fever. They were periodical, attended with constant nausea and vomiting of a grass green fluid, and were very difficult to manage. These cases gave me far more trouble than those of yellow fever—quinine, and even opium and calomel, were frequently ejected by the stomach. Blisters to epigastric region and opium and quinine enemata, the quinine in large quantities, was the most salutary practice I could devise.

With the facts before me, I would estimate the number of cases of all varieties, occurring from the 1st of August to the first of November, at 1200. Of this number about 800 may be classed as epidemic or yellow fever. The deaths by yellow fever during this time were 72. Of the 72, 8 or 10 were notoriously intemperate, and 4 others were over 55 years of age. 26 of the 72 that died, were females.

One half or more of the cases of yellow fever proved so mild, as to cause but a very short and imperfect interruption of the organic functions and their relations. Although its access and the febrile stage were attended with symptoms both painful and alarming to the patient and his friends, they faded, after a few hours continuance, with great rapidity. In reporting previous epidemics we have had to note, in cases which had a speedy and favorable termination, the sharpness and intensity of these febrile pains. This autumn this was conspicuously the case. These pains were of an acute, neuralgic character, and invaded the head, back, limbs and entire muscular system. The patient would not unfrequently cry out with intense suffering,—at the same time, the flushed face, bright eye and excited manner gave to him an appearance and expression strongly contrasting with the dejected, haggard and painful physiognomy of those laboring under an attack of bilious fever, where there is usually deep-seated pain in the head and back, with

gastro-enteric disturbance. During this febrile stage the skin was hot, pulse quiet, no positive nausea; occasionally, however, the contents of the stomach, with a little bile, was thrown up. In 10 or 20 hours the skin became moist, pulse became slower and pains rapidly abated. The most that the patient complained of after this, was great soreness of the muscles;—a few days rest, free from annoyance and excitement, and abstinence from indigestible *food* and *medicines*, were all that was necessary to immediate recovery.

If, after this painful feverish paroxysm had passed, there was restlessness, hesitation of manner, decided disinclination for food and drinks, with signs indicating a derangement or suspension of the secretions—all of which may be unnoticed by the hasty and superficial observer—the patient would not recover without passing through those critical stages, ever consequent to an altered and suspended condition of the secretions in this disease. In many cases, I was able to pronounce upon the grade and course of the disease from the state of the secretions of the mouth and bladder and the expectoration. Mild catarrhs were prevalent during the autumn, and if in the second stage of this disease the usual expectoration had ceased, and the mucus and saliva, common to the mouth, were wanting, they afforded evidence that the patient would not recover without passing through a severe ordeal. I attended a clergyman, for instance, who had suffered for some years with a mild form of chronic bronchitis, attended with expectoration of a dark colored mucus. The fever declined in about 20 hours, the pains passed off and skin became soft and natural, as also the pulse. After coughing, I noticed that he did not expectorate as usual—which was the first time it had ceased for years. He continued for five days without fever, pain, nausea or thirst, still, growing weaker, more restless and anxious, with scanty and colorless secretions of urine, and thick, *dove-colored*, putty like discharges, which were forced away by enemas. On the morning of the 6th day the usual expectoration returned, and in a few hours a restoration of the secretions generally became manifest. Sleep and an inclination for food succeeding, put to rest all apprehensions for his safety. Cases of this description, notwithstanding their severity and dangerous tendency, are classed among the milder ones, simply because there is not the mottled yellow skin, peculiar physiognomy, passive hemorrhage or black vomit, which usually distinguish those of undoubted severity.

It is not my purpose in this place to enter into any details of the symptoms and character of yellow fever, but simply to point out some of the differences between the fever of this and preceding epidemics.

The average duration of the disease this autumn exceeded that of 1843 by 48 hours. The duration of the febrile stage in 1843 was 20 hours; in this epidemic it was 40 hours. This stage lasted in many instances three or four days, and in a very few protracted cases seven or eight days. In 1843, black vomit seldom made its appearance before the fourth day and was generally preceded by, or attended with a well marked state of prostration or collapse; in this epidemic this character of vomit has been frequently noticed to occur in 35 or 40 hours and during the febrile stage. When it took place in this first stage, it was scanty, consisting of mucus and a few dark colored flocculent masses,

and soon ceased altogether. Many of these patients, after passing through a well marked perilous collapse stage, recovered—but most commonly the black vomit returned again about the fifth day, and was followed by immediate dissolution. Recoveries after black vomit have been more frequent in this than in former seasons—this, too, after large quantities had been thrown up. Three seamen, in the Marine Hospital, managed by my assistant, Mr. Jenkins, recovered, after vomiting blood and *black vomit*, almost incessantly, for two days. Mrs. Gordon, a lady residing on Dauphin street, vomited the description of matter known as the *wine-lees* vomit, at intervals of three and four hours, for five days, and finally recovered; these patients took no medicines after this symptom made its appearance, but were well nursed and sustained by such stimulants as porter, brandy, etc.; they are now in good health.

In reporting previous epidemics, I have called the reader's attention particularly to cases of an intermittent and remittent character. During this autumn cases of this description were frequently noticed; they were generally quotidian, the paroxysms of chill and fever were very short and did not usually continue after the third day. They were easily distinguished from the ordinary intermittent and remittent fever, by the suspension of the secretions, character of pulse, appearance of skin, nature of any matter that was rejected by the stomach, or the peculiar restlessness and manner of the patient. These could best be discovered during the apyrexia. Quinine was freely administered in these cases; in some, the paroxysms may have been arrested by it, but it certainly had no control in restoring the secretions and checking the dangerous tendencies of the disease.

Persons not familiar with epidemics of yellow fever, and even writers, are under the impression that to distinguish between yellow and bilious fever, such signs as hemorrhage, black vomit or a yellow skin must be present. Such is not the case in an epidemic prevailing among a resident population—at least two thirds of the cases present no such symptoms. Besides the character of pains which I have already alluded to as belonging to mild yellow fever, during the febrile stage, and the general condition of the patient after this has subsided, there are those belonging to bilious fever which cannot be mistaken. In the latter the paroxysm is certain to return, without it is arrested by quinine;—again, bilious fever seldom yields or runs its course without free discharges of bile, etc. No sophistry or authority, on the contrary, could induce a creole or nurse in Mobile to the belief, that a man laboring under yellow fever either vomited or purged bile. Two clergymen of our city, Mr. Balzan and Mr. Dorman, whose active benevolence has won for them much correct information in relation to fevers, have frequently, in my private and Hospital practice, designated each variety and grade of fever with the greatest ease and correctness—separating the grave from ephemeral cases of yellow fever, and distinguishing these again from periodical fever. In their diagnosis they were governed by the character of fever, pains, restlessness, color of skin, physiognomy, paroxysms and nature of the secretions.

I have remarked that there were cases, in which the febrile stage continued for seven or eight days: they occurred towards the close of the epidemic. During this stage, the pulse was variable; temperature of

the skin was changeable, but never cold, or pungently hot; the tongue was not furred, but red, at times dry and inclined to crack. There was great restlessness and some delirium or coma, particularly in the early part of the night. The bowels were constipated for the first five or six days, and even after this the discharges were scanty, consisting of the fluids drank by the patient, with flocculent masses of decomposed blood. They complained of thirst, but really preferred warm drinks to cold; the rebellious condition of the stomach, however, warned the patient against the free use of any fluid. Of this description of fever, we treated twelve cases—4 in hospital, and 8 in private practice. There was great uniformity of symptoms, duration of stages, and the mode of termination was the same in every case. After the seventh or eighth day, there was hemorrhage of a passive character, from the gums and nose, and in three instances from the ears. After this symptom made its appearance, there was constant black sordes about the teeth, and the tongue remained blackened by the constant oozing of blood*—if the hemorrhage was profuse, of course the changes did not take place. After the advent of this symptom, the skin became moist, physiognomy pleasant, mind clear and natural, evacuations bilious, sleep refreshing and appetite good. From this stage, recovery was as rapid as the extreme debility and emaciation of the patient would allow. No case of this description *died* in my practice this season. I gave them little medicines, relying on mustard plasters and stimulants, at the proper time, in such quantities as was necessary to sustain them. The great difficulty in treating these cases, consists in *keeping* up the spirits and patience of the sufferer, and preventing any under interference on the part of friends.

In my published notes of previous epidemics, it is stated, that if hemorrhage from the gums and nose—and vagina in females—took place before there was any appearance of the *bees' wing* or black vomit, the case usually terminated favorably. During this epidemic, hemorrhage from one or more of these orifices occurred in twenty-six of my patients, (hospital and private); no black vomit made its appearance, and with one exception, they all recovered. In females, the flow of blood from the vagina was usually free, and was succeeded by a marked improvement in the condition of the patient. I have never found it necessary or practicable to arrest this kind of hemorrhage. Cases of hemorrhagic fever were occasionally very protracted. I treated one, which excited, from the novelty of some of the phenomena, more than ordinary interest. A gentleman was seized with the disease, which, for five days, wore the livery of the most malignant grades. Upon the sixth day he began to bleed rather profusely from the nose and gums, especially from about the fangs of decayed teeth; in about ten hours after this symptom had made its appearance, the patient became (he had been delirious) rational, the hideous physiognomy vanished, and he became quiet and cheer-

*I have examined particularly, to ascertain whether this was a *dark colored secretion*, or ordinary blood; these examinations leave no doubt, that the blood becomes blackened after it has *oozed through* the mucous surfaces, in the same way that black vomit is formed in the stomach.

ful. On the 8th day, the hemorrhage increased, and he became ^{so} weak and prostrate, that I was constrained to arrest the bleeding by means of *plugs* and caustic:—the blood ceased to flow, and I did not visit him again until the next day, and ninth of his illness. I then ascertained that there had been no further hemorrhage since my last visit, and that the disease had reacted during the night, at least so far as to produce delirium, great restlessness, and uneasiness at the stomach.—On the night of the ninth, the hemorrhage again began, which was soon followed by the same marked improvement which attended the first appearance of this symptom. The bleeding continued for 11 days, ceasing on the 15th of his illness—though so profuse that he would at times grow faint, and his pulse would falter from loss of blood, he was carried safely through, by the free use of stimulants. This gentleman had been strictly temperate, and had taken no mercury. For the 11 days that the bleeding continued, he averaged half a pint of brandy and one pint of port per day—these stimulants were relished by the patient, and were found absolutely necessary to sustain him. He is now in excellent health.

I made five post mortem examinations at the Marine Hospital: in three of these, the coats of the stomach were but slightly altered—the mucuous coat in the other two, (which were protracted,) presented evidences of inflammation, but this was not so conspicuous as to warrant the belief, that this lesion was a very important one in these cases.—Two of these cases were very malignant, and run their course in five days—the liver in these was not strikingly changed—being *dry*, brittle, of a light straw color, and very much lessened in size. The other three cases were of a complicated and protracted character: in two of them, yellow fever with black vomit had supervened upon chronic intermittent. I could discover nothing in the liver of these cases different from that presented in periodical fever. These examinations sustain, as far as they go, the following remarks, appended to the Medical History of Alabama: “Within the tropics, where the disease prevails in its genuine, uncomplicated form, uninfluenced by climate, or the poison of bilious fever, as it frequently is on the gulf coast, this lesion of liver is doubtless far more constant and uniform.”—The internal surface of the intestines was mostly lined with a thick glutinous matter of an ash or *dove* color—this mucus was very difficult to remove—occasionally a patch of dark colored matter, resembling black vomit, was observed.—The small intestines, in those dying of the yellow fever, present no transparency: on the contrary, they are contracted, and present a dull leaden appearance. I have frequently noticed portions of intestines, from three to four inches long, flattened, with the walls glued together, as it were, by the mucus already mentioned. When the alimentary canal presents a condition of this kind, we cannot marvel, that both claret, water, and even calomel, instances of which are very common, should pass through it unchanged.

In relation to acclimation, and immunity from second attacks, the following carefully ascertained facts will be found more satisfactory than any assertions that can be made in reference to this point. I have ascertained the history of two-hundred and forty cases, which stand as follows:

Mild yellow fever, occurring in natives and old citizens, . . .	54
“ “ “ “ in persons who had the disease previously, . . .	27
“ “ “ “ “ “ “ have recently moved to the city . . .	80
Grave “ “ “ “ natives and old citizens, ...	8
“ “ “ “ “ persons who have had the disease before . . .	3
“ “ “ “ “ “ newly arrived, ...	67

 240

Of these cases, nineteen proved fatal ; among these, there were two natives of Mobile, and one who had the disease in 1843. This result is highly favorable to the general belief among medical men, that a long residence or an attack of the disease, secures the subject from, at least, severe visitations of the disorder. We have before stated, that there were few strangers or new comers in the city—of these probably two-thirds were attacked, with a loss of about 20 per cent. Had the disease attacked as *generally and violently the resident and acclimated population, as it did the few that were unacclimated*, there would in our city, consisting of a population of ten thousand, have been some five-thousand cases of yellow fever, and at least one thousand deaths. Instead of this, we have had but eight hundred cases, and seventy-two deaths—a result attributable alone to acclimation. These facts sustain the conclusion to which I had arrived in 1843—viz : “That yellow fever may attack the same individual more than once ; but that second attacks are generally mild and harmless.

The citizens of Mobile, whilst lamenting that their city has ceased to enlarge, and that their population is not increasing, may at least congratulate themselves, that this scourge, the yellow fever, has no further terrors, and in a few years more, it will only be known among them as the *strangers' fever*.

December 20th, 1847.

VII. *Post Mortem Appearances in a recent Dislocation of the Femur and Fracture of the Cranium.* Reported by ERASMUS D. FENNER, M. D., of New Orleans.

As an opportunity to examine the state of the parts in so important a luxation as that of the femur, is of rare occurrence, the following statement may not be uninteresting to the profession. Luxations of the extremities never proving fatal of themselves, the surgeon may meet and contend with them through a long life, without being able to know precisely the nature and amount of injury that is done. This is particularly the case with the *hip joint*, which is so strongly knit, that it can only be displaced by extraordinary force ; and when dislocated, can only be replaced by great surgical skill and adroitness. It is generally believed, that the chief difficulty consists in overcoming the spasmodic contraction of the powerful muscles which lie around the hip joint ; whilst it has been barely surmised, that the *capsular ligament* itself might present no inconsiderable obstacle to the return of the head of the femur to its proper place, after sufficient extension had been made.

Mr. Samuel Cooper, in his *Surgical Dictionary*, quotes as follows, from Mr. Pott :

"The mere bones composing the articulations, or the mere connecting ligaments, would, in general, afford very little opposition ; and the replacing the dislocation, would require very little trouble or force, were it not for the resistance of the muscles and tendons attached to, and connected with them : for, by examining the fresh joints of the human body, we shall find, that they not only are all moved by muscles and tendons, but also, that, although what are called the ligaments of the joints do really connect and hold them together in such manner as could not well be executed without them, yet in many instances they are, when stripped of all connection, so very weak and lax, and so dilatable and distractile, that they do little more than connect the bones and retain the synovia ; and that the strength as well as the motion of the joints depends in a great measure on the muscles and tendons connected with and passing over them ; and this in some articulations which are designed for the greatest quantity, as well as for celerity of motion. Hence, it must follow, that as the figure, mobility, action and strength of the principal joints depend so much more on the muscles and tendons in connection with them, than on their mere ligaments, the former are the parts which require our first regard, these being the parts which will necessarily oppose us in our attempts for reduction, and whose resistance must be either eluded or overcome—terms of very different import, and which every practitioner ought to be well apprized of."

Mr. Cooper says : "That the muscles are the chief cause of resistance, is strongly evidenced by cases in which the dislocation is accompanied with injury of a vital organ ; for then the bone may be reduced by a very slight force. Thus, in a man who had an injury of his jejunum, and dislocation of his hip, the bone was most easily replaced. (*Sir A. Cooper's Surgical Essays*, part 1, p. 20.) In short, anything which produces faintness or weakness, facilitates the reduction—as intoxication, nausea and sickness, paralysis, &c."

Again, "The supposition of the reduction being sometimes prevented by the capsular ligaments, Sir A. Cooper considers erroneous : he assures us, that in dislocations from violence, those ligaments are always extensively lacerated ; and that the idea of the neck of the bone being girt or confined by them, is altogether untrue."

Now let us see what we may learn from our case.

Case.—H. B., an athletic fireman on one of the towboats, (Porpoise,) plying between this city and the mouth of the river, was blown up and terribly injured by the bursting of the boilers, on the morning of the 14th June, 1847. The boat was descending the river, having a ship and brig in tow. All the boilers bursted at once, killing, wounding, and scalding a number of persons. The subject of our case was badly scalded and received a dreadful injury on the head. He was brought to the Charity Hospital on the 16th, and admitted into surgical ward No. 3, under the care of Dr. W. T. Brent. When he entered, he appeared to be laboring under a raving delirium, singing and talking incessantly. His condition was considered altogether hopeless, and scarcely anything was attempted to be done for him. He died on the night of the 17th,

more than three days after the receipt of injury, and the autopsy was made on the morning of the 18th.

Post mortem appearances.—Head.—There was found a fracture extending across the whole frontal portion of the head, involving the frontal, the temporal, the ethmoid, the malar, the superior maxillary, the nasal, and the sphenoid bones. The *crista galli* was broken off and driven through the dura mater, lacerating it considerably. There was considerable depression of the bones across the orbitar region, and the brain was badly lacerated and bruised. The integuments at this point were *not cut or lacerated*, but scalded and swollen. He was in the act of putting a stick of wood into the fire when the boiler bursted, and drove it forcibly against his head. It is worthy of remark, that the brain, where the phrenologists have located the organs of *language and tune*, suffered the greatest injury in this case, and that the man was *talking and singing* as long as he was able. The examination of the brain was not carried any farther, as our special attention was invited to the dislocated hip. As before remarked, no effort had been made to reduce the dislocation, the case being considered utterly hopeless.

State of the dislocated Thigh.—The luxation was upwards on the *dorsum ilii* of the left side; the limb was shortened about an inch and a half, and the toes were turned inwards. On raising the integuments, extensive ecchymosis was discovered around the joint, along the fascia superficialis, and amongst the muscles as low as the knee. On raising the gluteus magnus and medius, the naked head of the femur was exposed, lying on the dorsum of the ilium, with the *ligamentum teres* hanging to it. This ligament had been completely detached from the acetabulum, and nearly half of its attachment to the head of the femur was broken up. The head of the femur, in being forced upwards and outwards, had ruptured and lacerated a portion of the obturator externus, pyriformis, and gemini muscles. After all the external muscles about the joint had been dissected up, we could not bring down the thigh. The iliacus and psoas magnus were now severed. These let the head down somewhat, but it could not be replaced. The triceps adductor was now divided, but without effect. The ileo-femoral ligament was found tensely stretched. All the muscles between the pelvis and thigh being now severed, there was of course no obstacle to complete extension; yet it was found *impossible to replace the head of the femur into its proper position. It could not be forced back through the aperture in the capsular ligament out of which it had passed.* The capsular ligament was found ruptured at least one-half of its extent, yet the opening *had to be enlarged from one half to three-quarters of an inch, before the head of the femur could be put back into the cotyloid cavity.* So, here evidently laid the chief obstacle to the reduction of the dislocation.

The *capsular* is a very strong fibrous ligament, and we may readily conceive of its possessing sufficient *elasticity* to allow the smooth round head of the femur to pass out through a lacerated opening, which might at once contract so as to offer considerable resistance to its return.

In Sir Astley Cooper's *Essays on Dislocations and Fractures of the joints*, may be found a case and examination similar to this, in which

the man died twenty-four hours after the receipt of injury, but the dislocation was reduced before death. With due deference to the high authority of Sir Astley Cooper, I cannot help supposing the capsular ligament may often cause much of the difficulty in reducing dislocations of the hip joint.

VIII.—*Operation of Laryngotomy.* By FRANCIS BARNES, M. D., of New Orleans.

Editors New Orleans Medical and Surgical Journal :

GENTLEMEN :—The following case I deem interesting, as illustrating the real dangers to be dreaded from the operation of Laryngotomy.

It proves to my mind conclusively, that the apprehension with which it is regarded generally by the profession, is unfounded ; and that if cases of a fatal nature have occurred, in which it has been performed, that result cannot be attributed to the operation, but either to its having been performed too late, or in cases in which it could not possibly be attended with success, with the pathological conditions present.

The patient whose case I report, was suffering from a severe attack of typhus. During the progress of this disease, he was seized with erysipelatous inflammation of the throat, involving all the salivary glands and most probably the tonsils and epiglottis. While insensible from suffocation, the swollen œdematous tissues were cut through, his larynx opened, and air admitted into his lungs. He became relieved, and recovered without a bad symptom. Notwithstanding his typhus and the erysipelatous condition of the parts, there was no extension of the inflammation to the larynx, nor the bronchia ; but a copious suppuration of the wound took place, and the swelling gradually disappeared. If such an instance of recovery occurs, we may safely conclude, that the operation is justifiable whenever the pathological conditions are present, which call for its performance.

James Murray, aged 40 years, Irishman, was admitted into ward no. 25 in the Charity Hospital, on the 20th of February ; he was a man about five feet seven inches in height, with full face, broad shoulders, and muscular ; hair and eyes black.

The symptoms he presented indicated that he was suffering from a severe attack of typhus : his prostration was great ; face anxious, and of a dark mahogany hue ; conjunctivæ injected ; numerous petechiæ over the breast, abdomen and elsewhere. Tongue dry and raspy, and much coated. Urine at first scanty and high-colored. Delirious at times. Pulse frequent, yet moderately full. Thirst excessive. Bowels torpid. During the progress of his disease, no symptom of inflammation of any vital organ had been observed.

A tonic and slightly stimulant plan of treatment was adopted : his bowels were moved when necessary, with sulph. magnes. or Seidlitz powders ; infus. cinchonæ and lemonade was ordered for drink ; porter freely, with milk for diet. Tinct. cinchon. compos. was given at times, and when sleepless, sulph. morphia was exhibited. Under this

plan of treatment he improved until, on the 2d of March, at the morning visit, I observed his throat, neck, and lower part of the face enormously swollen; the sub-lingual, sub-maxillary and parotid glands very much enlarged and indurated, and the integuments of the neck very œdematous and of an erysipelatous redness. His mouth could not be opened sufficiently to ascertain the condition of the throat and epiglottis. His breathing was frequent and performed with great difficulty. From the livid appearance of his face and dyspnœa, I was fearful that an operation would be necessary, and determined to visit him again in a few hours. In the meantime I directed a solution of tart. antimony, one grain to the ounce of water, to be given him, a table spoon full every half hour and that his throat and neck be frequently rubbed with mercurial ointment.

At one o'clock I visited him again. I found him apparently dying; his face almost black, and lying insensible and motionless with the exception of his frequent efforts to breathe, which were accompanied with a loud stridulous noise. I immediately procured instruments, and having procured the assistance of Mr. Marshall, resident student of the house, I proceeded to perform the operation of laryngotomy. Owing to the swollen condition of the throat from the chin to the sternum, neither the thyroid cartilage, the hyoid bone or the trachea could be felt with the finger. With a straight bistoury an incision was commenced in the median line about $3\frac{1}{4}$ inches above the sternum, and carried down within a quarter of an inch of that bone; about an inch in depth of the œdematous tissue was cut through before the thyroid cartilage was brought to view. This was depressed by the swelling much below its normal situation, so that when the incision through the crico-thyroid membrane was made, it corresponded with the most inferior part of the wound. Immediately upon the opening having been effected, the air rushed through it with considerable violence, and after five or six respirations made through this artificial air passage, the patient was restored to consciousness and shortly experienced perfect relief. A double silver canula was procured, but the opening not being large enough to admit it, the cricoid cartilage was divided. It was then introduced and held into place with tape passing around the neck. The introduction of the canula caused a paroxysm of coughing, which lasted four or five minutes, but which did not return. His solution of tartar emetic and mercurial frictions were ordered to be continued; he was allowed milk only for diet, and infusion of elm for drink.

March 4th. The canula was withdrawn and the wound closed with the fingers in order to ascertain if he could as yet breathe by the natural passage, but finding that his difficulty of breathing returned, it was again introduced.

The treatment of yesterday was ordered to be continued. In the evening he could breathe with greater freedom upon the wound being closed, but it was deemed safer to let the canula remain in his larynx during the night.

March 5th. The canula was now withdrawn, but directions^lleft to introduce it if necessary. Treatment continued.

March 6th. Swelling considerably diminished; the wound was freely suppurating. Treatment continued.

March 7th. A large abscess formed between the left submaxillary gland and the integument, and opened into the wound. The parotids had diminished to almost their natural size, as were also the sublingual and submaxillary glands. Tartar emetic and mercurial frictions were now discontinued, and his wound dressed with a solution of quinine, which was also injected into the sinus opening into it. The patient's diet was increased and porter and wine freely allowed him. From this time his wound gradually closed and his strength continued to increase until he was discharged, cured, on the 14th of April.

New Orleans, May, 1848.

IX.—*Fever Statistics ; showing the relative proportion of the different forms of Fever admitted into the New-Orleans Charity Hospital during a period of seven years, from 1st January 1841 to 1st January 1848, inclusive ; monthly and annually. Reported by E. D. FENNER, M. D.*

With some trouble I have compiled from the books of the Charity Hospital the following statistical tables, which may prove interesting to the Profession abroad, and especially useful to the student of Fever, the most fatal disease of Southern climes. This institution is probably the most extensive *Fever Hospital* in the world, and affords the greatest facilities for investigating the disease in all its forms and varieties. I shall not stop to discuss the question whether there be "*any such individual disease as fever,*" which has been denied by a recent author, (Prof. Bartlett), but give the statistics of *fever* in the common acceptation of the term, believing I shall be well enough understood. Nor shall I attempt to rectify the *very faulty nomenclature* applied to fevers, particularly in this city. I shall give the names of fevers just as I found them on the books of the hospital. As my sole object is to show the relative prevalence of the different forms of fever, I shall only give the number of *admissions* into the hospital, omitting the *discharges* and *deaths*.

Tables showing the monthly and annual Admissions of the different Fevers.

1841.	Jan'y.	Febr'y.	March	April.	May.	June.	July.	August	Septbr.	Octbr.	Novbr.	Decbr.	Total.
FEVERS.													
Intermittent . . .	3	27	45	39	28	65	187	151	18	66	93	72	794
Typhoid	1										5	6	12
Congestive	4	3	1				7	3		3	1	2	24
Remittent.	3	2		2	9	6	31	31	5	1	7	2	39
Malig. Intermittent							3	3					6
Yellow								174	642	252	37	8	1113
Bilious												3	3
	11	32	46	41	37	71	228	362	665	322	143	93	1991

Total Admissions of all diseases, 4380.

1842.	Jan'y.	Febr'y.	March.	April.	May.	June.	July.	August.	Septbr.	Octbr.	Novbr.	Decbr.	Total.
FEVERS.													
Intermittent . . .	45	29	35	39	45	124	160	169	144	140	110	61	1092
Remittent	4		1	3	4	8	12	34	41	35	11	3	155
Typhoid	9	2				4			2	2	2	1	22
Bilious				2		3	9	3	1	2	2	1	23
Congestive	1		3	3	2		10	5	4	9	2	1	40
Gastric					1	2	6	1				1	11
Catarrhal						1				1		1	3
Yellow								47	247	93	23		410
Nervous										1			1
Adynamic										1			1
	59	31	39	47	52	142	197	259	439	284	150	79	1758

Total Admissions of all diseases, 4404.

1843.	Jan'y.	Febr'y.	March.	April.	May.	June.	July.	August.	Septbr.	Octbr.	Novbr.	Decbr.	Total.
FEVERS.													
Intermittent	31	30	35	31	19	40	70	98	128	136	149	76	843
Typhoid	2										2		4
Remittent	1			1		9	40	75	49	12	8	10	205
Catarrhal	1							1					2
Bilious	1			1	15	3	37	5	2	2	3	2	71
Typhus						3	6						9
Congestive						3	17	4					24
Gastric						1	1	1				4	7
Continued						1							1
Yellow							23	188	365	351	111	15	1053
Cephalic													3
	36	30	35	33	34	60	194	372	544	501	273	107	2222

Total Admissions of all diseases, 5013.

1844.	Jan'y.	Febr'y.	March.	April.	May.	June.	July.	August.	Septbr.	Octbr.	Novbr.	Decbr.	Total.
FEVERS.													
Intermittent	66	49	41	32	44	75	176	258	255	261	216	116	1589
Remittent	2	4	2	1	4	24	30	47	67	55	5	3	244
Yellow	2	2				1	1	1	68	52	25		152
Typhoid	6		1	3		3	10	12	11	8	6	20	80
Simple	3	6	3										12
Gastric	1				2	1			5	2			11
Typhus		4											4
Bilious			2	4	2		1			3		1	13
Inflammatory			1				2		2	2	2	2	11
Congestive				1	7	11	17	14	13	15		3	81
Adynamic							2		2				4
Continued						2				3			5
Eruptive								1					1
	80	65	50	41	59	117	239	333	423	401	254	145	2207

Total Admissions of all diseases, 5846.

1845.	Jan'y.	Febr'y.	March.	April.	May.	June.	July.	August.	Septbr.	Octbr.	Novbr.	Decbr.	Total.
FEVERS.													
Intermittent	7	75	57	44	79	112	145	96	279	196	189	124	1403
Typhoid	7	6	5	2	10	8	11	14	18	20	15	23	139
Remittent	2	1		1	11	17	38	34	33	17			154
Congestive	3		1		1	2	1	4	5	4			21
Inflammatory	2						1	1					4
Yellow		1											1
Continued		1		2	1	1							5
Nervous				1		1	1						3
Bilious				1	2	6	1	1	1	2			14
Simple				1		4	1	1	1	1			9
Pernicious					2		2						4
Ephemeral									1				1
Adynamic									1	1			2
Scarlet											2		2
Catarrhal												1	1
	21	84	63	52	106	151	201	151	339	241	206	148	1763

Total Admissions of all diseases, 6136.

1846.	Jan'y.	Febr'y.	March.	April.	May.	June.	July.	August.	Septbr.	Octbr.	Novbr.	Decbr.	Total.
FEVERS.													
Intermittent.	79	58	75	76	85	138	214	227	359	376	310	81	2078
Typhoid.	30	13	7	5	10	12	14	17	5	7	23	52	195
Remittent	3	3	5		7	6	2	9	22	36	7	3	103
Congestive			1		1	2	4	2	5	9	2	5	31
Yellow									29	83	32	4	148
Bilious				1		1			2				4
Pernicious	2								9				11
Catarrhal.				1									1
Nervous.	1	1								1			3
Scarlet	2		2		1	2				1			8
Malignant							2						2
Larvata									2	2	1	1	6
Pernicious Interm.										6	4	1	11
Gastro Hepatic. . .										1			1
Adynamic.													
Typhus													
Ataxic											1		1
	117	75	90	83	104	161	246	255	433	522	380	147	2603

Total Admissions of all diseases, 8044.

1847.	Jan'y.	Febr'y.	March.	April.	May.	June.	July.	August.	Septbr.	Octbr.	Novbr.	Decbr.	Total.
FEVERS.													
Intermittent	144	117	98	153	140	211	223	74	53	258	380	341	2192
Typhoid	40	21	50	73	66	20	7	2	1	6	60	111	457
Typhus		2	4	107	165	369	57	1		4	100	236	1045
Remittent	4	1	4	9	17	38	69	64	25	12	18	8	269
Congestive.	1	1	1	2	3	12	10	1		1	2	2	36
Yellow						5	148	1611	777	219	49	2	2811
Bilious	2			2		4	7	2	1	9	7	8	42
Pern. Interm.	1				2	1				1			5
Adynamic	1												1
Ephemeral.					3					2	1	6	12
Catarrhal						1					4	18	23
Inflammatory								1					5
Continued											1	2	3
	193	142	157	346	396	661	521	1756	857	512	622	834	6901

Total Admissions of all diseases, 11,890.

From these tables it appears there were admitted of all kinds of fever 19,445 cases—of which 9991 were marked "*Intermittent*." Add to which 26 marked "*Pernicious*" and "*Malignant Intermittent*," and 257 marked "*congestive*," (which is only the American cognomen for the same form of *Intermittent* fever,) and you will have 10,274 Intermittents, or *more than one half of the whole amount*, by 1103.

The following Table will show the relative prevalence of what is marked "*Intermittent fever*" at the different seasons of the same seven years :

	Spring	Summer	Autumn	Winter
1841	112	403	177	92
1842	119	453	394	135
1843	85	208	413	137
1844	117	469	732	231
1845	180	353	664	206
1846	236	569	1045	218
1847	391	508	691	602
Totals . .	1240	2963	4116	1621

Is it not curious to note the gradual increase and decline of intermittent fever at this Hospital; and also the remarkable fact that this form of fever *is never entirely absent; not even at the zenith of the worst epidemics of the yellow fever*. In August 1847, when there were admitted 1611 cases of *yellow fever*, there were also admitted 74 cases of *intermittent*. But examine the statistics carefully, and you will discover other curious facts which I shall not take the time to point out.

Two interesting questions may here be examined, viz., *what proportion of all this intermittent fever really originated in New Orleans? and in what part of the city did most of it occur?*

The city of New Orleans is certainly a *great thoroughfare*, and has a larger transient or floating population perhaps than any other to be found. It is accessible by ships, steamboats and land carriage. It is surrounded by a low, level and very fertile country, having a large number of free white labourers. This class in the country is engaged principally in draining wet land and, to some extent, in the mechanic arts; about the city, it is also extensively engaged in draining the suburbs, but perhaps to a greater extent in street labour, such as paving, draying and loading and unloading ships and steamboats on the Levee. Their residences are chiefly in the newer and more retired parts of the city where rents are cheapest; but they are generally interspersed among those of the wealthier class.

Now, the Charity Hospital is opened *gratuitously* to all indigent persons male and female, white or colored, who may wish to enter, and there is no obstacle to admission. This charity is only offered to the indigent, but pay wards are provided for those who are able to afford a moderate compensation.

I believe it has been established that the poorer classes suffer more than any others from all kinds of fevers. Such is certainly the fact here, where the usual *exciting causes*, such as intemperance in eating and

drinking, and exposure to the hot sun, etc., are very potent. The records of the Charity Hospital do not afford any precise information as to the length of residence in New Orleans. The question is asked and the time stated, but the clerk informed me that he made no distinction between a residence in the city proper and the neighbouring country. The house surgeon and one of the clerks both gave the opinion that most of the cases of intermittent fever occurred amongst the labourers in the suburbs and vicinity of the city. But the truth is, the laborers on the Levee, streets and canals furnish the greatest number of cases of *all kinds of fever* at this place. My own opinion is that those localities within the precincts of the city which afford the greatest amount of intermittent fever, also afford the greatest amount of remittent bilious and yellow fevers; moreover, that they are all *closely allied affections*. The examination of this question may form the subject of a future communication.

If I were asked what sort of *index* these Hospital statistics afford as to the prevalent fevers among the better classes of society in this city, I might be at a loss for a satisfactory reply. I think that people in good circumstances, who live well (as they generally do) and are not imprudent, suffer but little from fever in this city. Those who have but recently settled here are apt to take yellow fever when it prevails, but as they generally have prompt attendance, the mortality amongst them is small, and many escape it entirely. They appear to suffer more from the eruptive fevers than any others. No one aware of the stupid imprudence and negligence of the labouring classes can be surprised at the mortality amongst them. They receive high wages for their labour, and having no idea of economy, it too often causes their ruin.

One more reference to the statistics, and I have done. It appears that the total admissions of all diseases into the *main building* of the Charity Hospital during the above stated period of *seven years*, was 45,713—of which 19,445 were for *fevers*—and of these last, 10,274 were for the different forms of *intermittent fever*.

Part Second.

REVIEWS AND NOTICES OF NEW WORKS.

- I.—*Practical Observations on certain Diseases of the Chest, and on the Principles of Auscultation.* By PEYTON BLAKISTON, M. D., F. R. S., Fellow of the Royal College of Physicians; Physician of the Birmingham General Hospital; and formerly Fellow of Emanuel College, Cambridge. Philadelphia. Lea and Blanchard. 1848. (From J. B. Steel, 14 Camp street, New Orleans.) pp. 348.

If auscultation and percussion can be taught by books, surely no man who reads can plead ignorance on this subject. We rejoice to witness a tendency in those who have written on auscultatory medicine, to condense—to simplify and bring within the comprehension of the most ordinary minds, aided by a little practical application, the principles and real utility of this branch of our science. Since the days of Laennec, few have written as voluminously, and none more elegantly than the founder of auscultation and percussion. It was doubtless necessary for the author and discoverer of this interesting department of medicine to amplify and illustrate by every possible means, a subject, the very principles of which had not as yet been revealed to the medical world.

This necessity, now happily for the profession, no longer exists; hence, the small and compact volumes now before us. We always feel disposed to think favorably of an author's sound judgment and practical good sense, when we find that he has written a small book; he seems to keep in view the old truism,—“*vita brevis.*”

As every book must have a preface, giving usually the reasons, and explaining the motives which induced the author, (frequently much against his will,) to undertake to write a work, Dr. B. has adopted this, the usual method of setting forth the object of the present work.—Believing, he says, “that additional facts are required to establish sound general principles, which may serve to prove the truth or fallacy of those already proposed, he felt it his duty from the opportunities furnished him in civil and hospital practice, for collecting facts, to record and publish them, even should he be compelled for want of leisure or ability, to leave to others the task of eliminating from them general truths by philosophical induction.” He continues: “The principal having been not so much to produce a systematic treatise as to record the results of personal experience, little reference has been made to the works of previous writers.” The principles of auscultation have been

purposely developed in a very elementary manner; for it must have been felt, continues Dr. Blakiston, by all those who are engaged in clinical teaching, that one of the greatest difficulties they have to contend with, arises from the incomplete preliminary education of some of their pupils, and their imperfect acquaintance with the laws of physical science."

Acting upon the conviction that medical students, and even practitioners, are too often ignorant of the elementary principles of some of the physical sciences, and particularly of acoustics or the doctrine of sounds, &c., Dr. B. devotes an entire chapter to an explanation of the "properties of sound." He lays down certain propositions, and then illustrates them in a manner at once so clear and concise that the reader can find no difficulty in understanding the subject. This chapter may be studied with profit by all those that are anxious to have clear and distinct views on the subject of auscultation and percussion.

Having explained to the reader the laws by which sound is governed or transmitted, Dr. B. in his second chapter, treats of the "sounds elicited by percussion." He compares the chest to a drum, the sounds of which when struck, depend on the vibrations of its parchment, varying with its tension and the medium in which it vibrates. If this parchment be made thick by adding to it other materials, or by relaxing it, the sound must be correspondingly modified—the sound will become flat and deadened.

Apply this fact to the chest: let any addition be made either to the thickness of the walls of the chest or to the organs contained therein, and we detect a modification of the sound elicited by percussion—it is no longer clear, sonorous, but dull and flat. The object of the present inquiry is, to ascertain in "what manner such modifying circumstances arise both in *health* and *disease*."

It is confessedly difficult to detect a disease of the chest without some knowledge of auscultation and percussion; and as it is the first duty of the physician to be familiar with the sounds elicited by percussion from healthy lungs, before he can understand them as modified by disease, Dr. Blakiston has given us some sensible remarks on *percussion of the thorax in health*.

He premises, that as the elasticity of the thorax may and is frequently affected by age, produced by ossification of the costal cartilages, and also the absorption of fat, in some cases, it is highly important that this fact should be kept in view in making out our diagnosis, else serious errors may be the consequence.

Of course every sensible auscultator will have regard to the density or tenuity of the thoracic walls; for he has only to recall the laws by which sound is governed, to avoid any mistake on this subject. After laying down some few general rules to guide the young stethoscopist in his explorations of the sound chest, our author proceeds to discuss the mode of percussing the thorax in disease. Any unusual thickening in, or the presence of tumors about, the walls of the thorax, must necessarily interfere with the sounds elicited by percussion; œdema of the subcutaneous cellular tissue about the chest, will also exert a modifying influence over the sonorousness of the lungs. He then describes the sounds

produced by percussing the chest in pneumothorax, emphysema, tubercular cavities of the lungs, and some other morbid conditions, which, in consequence of admitting an abnormal quantity of atmospheric air, give rise to a louder and clearer sound than in health.

If fluid exists in the chest and is confined to the bronchial tubes, the sound of the chest on percussion is not materially affected; because the elasticity of the lungs is still retained, and their sonoriety is but little affected: but should any effusion take place in the cellular tissue of the lungs, the case is of course altered,—the elasticity of the lung being thereby diminished, a corresponding dull sound will be elicited on percussion. Any deposit of encephaloid or other heterologous matter, either in the lungs, upon the mediastinum or pleural surfaces, or enlargement of the heart, must by compressing the lungs and encroaching upon the cavity of the thorax, render the sound in percussion much duller, and embarrass diagnosis. After describing in a very plain and unpretending style, the auscultatory sounds of respiration—with all of which the reader must be quite familiar—Dr. B. makes a few interesting observations on “auscultation of the sounds of the voice.”

In chapter the 5th, he treats of “auscultation of the sounds of the heart.” After giving a very clear description of the sounds of the heart in health, describing the *first* or *systolic* as the longer, and the *second* or *diastolic* as the shorter, sound, he proceeds to lay down the following as the movements of the heart, which may be supposed capable of producing sounds: 1st. The rubbing of the heart against the pericardium. 2nd. The striking of the heart against the ribs. 3rd. The collision of the particles of blood with each other, and with the interior of the heart and large vessels. 4th. The collision between the internal surfaces of the ventricles after the expulsion of the blood. 5th. The motion of the different valves. 6th. The movement of the muscular fibres of the walls of the ventricles during their contraction and dilatation.

In a normal state of the heart and pericardium, no sound whatever can be produced by the rubbing of the heart against this sack. This fact has been verified by removing the pericardium, when it was observed that the contractions and dilatations of the ventricles gave rise to the same sounds; thus proving that auscultation could detect no impairment in this respect.

Again, under ordinary conditions, no increase of sound can be perceived by the apex of the heart striking against the ribs, since on removing the ribs, the effect has been found the same. Neither does the collision of the particles of blood with each other, or against the interior of the heart, prove the principal cause of the systolic sound, as it was heard, though less distinctly, after the heart had been extirpated. Hence, says our author, it follows, “that the diastolic sound is produced by the unfolding and tightening of the arterial valves, and the stroke of the whole column of blood against them. He ascribes the systolic sound of the heart exclusively to the movement of the muscular fibres of the walls of the ventricles during their contraction.”

Having laid down some directions for percussing and auscultating the chest, Dr. B. next examines the stethoscope, and compares the different kinds that have been constructed. He concludes, that theory indicates

the utility of employing a solid stethoscope, and gives as his reasons for his preference for this sort of instrument, that it conveys to the ear more of the pure sound, when laid on the chest, than a hollow stethoscope. The latter, he asserts, conveys sounds of greater intensity, but of a quality which varied with the substance and material of the instruments. We have used stethoscopes of several different constructions, and, after all, we are decidedly in favor of the immediate application of the ear to the chest, though less *fashionable*, and objectionable only in cases of scrupulous or timid females, or when the subject is filthy or laboring under some pestilential disease.

If the physician be desirous to impress his patient with a proper sense of his professional importance, he may approach the bedside of his client with a finely figured instrument, mounted with ivory or even gold, and go through the routine of an examination; but if he be really desirous to hear, to understand, and to interpret the sounds of the lungs or heart, let him disarm himself, lay by his "helmet and shining shield," like Hector of old when he bade adieu to his darling boy, and approach his patient *auribus non armatibus*, and we venture to believe that his diagnosis will be equally correct and satisfactory.

Dr. Blakiston is, if we mistake not, the inventor of the solid stethoscope, and for twelve years he has used this kind of instrument in preference to all others. He tells us, he induced many of his friends to make trial of it, and almost all, including his friend, the distinguished Dr. Watson, have expressed their decided approbation of this instrument.

"It is made of light cedar, the grain of which runs from end to end; the ear-piece is slightly hollowed out, and the stem is tapered off to receive it, in order that it may always fit tightly."

The bare sight of a finely finished stethoscope, so flurries and agitates some persons, especially the nervous and delicate, that it is almost impossible to distinguish one sound from another; hence, confusion may arise in the mind of the young auscultator, and he may confound the signs of disease with the effects of fright.

We made an attempt more than once, to examine with a stethoscope a very delicate and excessively nervous female, whose appearance and symptoms induced her friends to seek advice; and during the examination, the heart, through fear, beat so tumultuously and the respiration became so hurried that it was impossible to distinguish one sound from another.

Contrary to the general belief, Dr. B. maintains, that a physician may become a good stethoscopist without possessing a musical ear: this declaration is encouraging, and no student should despair of acquiring a good practical knowledge of auscultation, though he may be unable to distinguish one note from another. The larger portion of Dr. Blakiston's work is filled with a detail of curious and interesting cases of thoracic disease, aneurisms of the larger arteries seated in the chest, with well directed post mortem inspection.

The book is, then, eminently practical, and full of useful information. The author tells us, the notes of all his reported cases were written down at the bedside and in the dead-house, and he holds himself respon-

sible for their correctness. Such material is always acceptable to the profession, and will be referred to and remembered when the theoretical speculations of some writers shall have been consigned to oblivion.

To show the practical nature of the work before us, we propose to extract from it a few interesting cases, to which we ask the reader's attention. More than one-hundred and twenty interesting (and some of them very curious) cases, accompanied with post mortem examinations, are reported in this work. They relate chiefly to "*thoracic aneurisms, pulmonary and cardiac diseases.*"

The following case, (No. VI,) of *Aneurism of the Arch of the Aorta communicating with the trachea*, illustrates the difficulty of diagnosing such diseases.

"A tradesman, aged 38, had suffered some months from hoarseness and partial loss of voice."

"He was constantly troubled with a cough, and had several attacks of hæmoptysis, in which black coagulated blood was generally brought up, mixed with a little of a bright red color. He constantly felt a constriction of his throat, and his respiration was accompanied with a slight whistle. The trachea seemed to recede backwards from the sternum, but was not turned on either side. No trace of any normal sound could be detected over any part of the chest. In this state he was one day suddenly seized with symptoms of acute laryngitis. Mercury was rapidly administered and preparations were made by Mr. Lawrence, under my advice, to perform tracheotomy, had not relief been obtained. He was however quickly salivated, and recovered, but the cause of these attacks still remained concealed from view. Some months after this, he was seized with symptoms of severe bronchitis; on this occasion he was seen by Mr. T. F. Palmer, who was kind enough to send me the particulars of the attack, and of the post mortem appearances, and to show me the diseased parts. From his account it appears, that previous to death, the patient was expectorating a thin sanious fluid; that whilst conversing, he was suddenly seized with symptoms of suffocation, gasped eagerly for breath, attempted to thrust his fingers down his throat as if to remove some obstacle to respiration, sank back upon his bed and died."

"*Inspection.*—There was much atheromatous, and some calcareous deposit in the aorta. The arch was dilated uniformly into a pouch, at the back of which was an oval opening an inch long, and half an inch wide, with smooth, rounded edges, leading into a sack which would have held a chestnut. This sack was bounded at the sides by the arterial coats, and at the back by the trachea, to which it closely adhered, and with which it communicated by a small aperture, with smooth and rounded edges, capable of admitting a crow quill. It was filled with concentric layers of fibrin, the most external of which were tough and light colored. This sack pressed on the trachea and very much diminished its calibre. The larynx presented evident signs of chronic or repeated inflammation, the submucous tissue being evidently thickened. There were signs of inflammatory action in the lining membrane of the trachea."

Errors in diagnosis are unfortunately too seldom made known by those who commit them; and such candid statements as the above are highly creditable to the parties concerned, and are sure guarantees that everything that emanates from the same source, may be received as truth. The author states in his comments upon this case, that not one of the signs present could be taken to indicate the existence of aneurism.

We shall take another case, and then we have done.

CASE.—*Endocarditis—Tricuspid regurgitation—Dropsy.*

"A porter aged 32, was attacked with acute rheumatism twelve years ago, and again eight years since, when he had very severe pains in the left breast, troublesome cough and dyspnœa. Eighteen weeks previous to my visiting him, his ankles began to swell, then his legs and abdomen. There was much dyspnœa. The urine was not coagulable by heat. The pulse was hard, sharp and vibratory. The jugular veins were turgid and pulsated. There was dullness on percussion at the bottom of each side of the chest. The systolic sound of the heart was accompanied by marked low sounds. In a few days' time intense pain at the precordial region suddenly made its appearance with most distressing dyspnœa. The pulse rose to 120 and was very sharp, hard, and full. Venesection having been freely employed, and having been joined with the administration of digitalis and mercurial frictions, the acute symptoms were removed, the anasarca and the pulsation of the jugular veins disappeared. The pulse remained very hard and vibratory. In a few weeks, pulsation of the neck, rapidly followed by anasarca, reappeared, and he gradually sank.

"*Inspection.*—Much fluid was found in the abdomen and thorax, and four ounces in the pericardium. This membrane was thickened and opaque in patches. The heart was generally hypertrophied and dilated to more than twice its natural size. The tricuspid valves were a little thicker and larger than usual, but could not nearly close their foramen, which would admit four fingers and the thumb nearly up to the knuckles, and which measured nearly seven inches in circumference. The mitral valves were thickened, but acted well. The lining membrane of the left auricle was thickened and opaque, and was studded with several horn-like patches. On one of the aortic valves was a patch of soft, reddish, recently organized, false membrane, of the size of a four-penny piece. The lining membrane of the aorta seemed swollen and puckered unevenly. The other organs were healthy."

Such is a specimen of the cases reported in the work under consideration. The book is practical throughout, and will, we feel assured, be popular with the profession. It may be found at J. B. Steel's book-store, 14 Camp street.

A. H.

II.—*Memoranda on Anatomy, Surgery, and Physiology, forming a Pocket Companion for the Young Surgeon, or for Students preparing for examination.* By MARK NOBLE BOWER, Surgeon; corrected and enlarged by an American Physician. New York. Samuel S. and W. Wood. 1848. pp. 335.

Through the politeness of T. L. White, bookseller, No. 53 Canal street, we have received a small work bearing the above title. We think the time consumed in preparing such works might be appropriated to more advantage in some other way. Some quack it in nostrums, panaceas, linaments, unguents, vermifuges, pills, potions, and a thousand other combinations of medicaments; but others, again, scarcely a whit more honest, but with more pretensions, if possible, deal in another species of quackery—that of *book-making*. The latter class pretend to transport the student with *telegraphic* speed, over the arduous and rugged path of patient study, to the temple of science, with scarcely an effort on the part of the uninitiated. Books like the one under notice, may serve to revive recollections of anatomical and sur-

gical facts, but they cannot take the place of the more comprehensive systems, on the several subjects embraced in medicine.

We have examined this little work, and although it is quite correct in its details, yet it is too incomplete and imperfect to put in the hands of the medical student. It will necessarily lead the mind to take a superficial view of medical science, and thus circumscribe the labors which have, for ages, been deemed requisite to qualify the practitioner for the discharge of his responsible duties.

A. H.

III.—*Lectures on subjects connected with Clinical Medicine.* By P. M.

LATHAM, M. D., Fellow of the Royal College of Physicians, and Physician to St. Bartholemew's Hospital. (Second Edition). Philadelphia. Barrington and Haswell. 1847. pp. 158.

A small book often proves a great favor to the reader, and a blessing for the reputation of the author. In this instance the two assertions hold good; for the small volume before us is a perfect casket of original matter, as rich, as rare, and as interesting to the medical reader, as the finest conceptions of the poet are to the speculative visionary.

The author believes that many of the different branches of medicine, particularly auscultation, may be greatly circumscribed and simplified for practical purposes. Possessed of this idea, he has labored to illustrate its truth, by stripping the subject of much of that spurious and superfluous technological learning, with which some writers have attempted to clothe it. These Lectures are written in that easy, plain, and common-sense style which indicates the practical turn of the author's mind, and is sure to fix the attention of the reader. He deplores the want of preparatory education among students of medicine; and although he admits that some of the brightest luminaries of the profession have been men of limited general education, yet it is fair to suppose, that these very physicians, with an accomplished education, might have occupied a still larger space, and achieved far greater triumphs for science and humanity. The different professions, to adopt the language of Dr. Latham, have one way of glorifying themselves, which is common to all. It is by setting forth a vast array of preparatory studies, and pretending they are indispensable in order to fit a man for the simple exercise of the practical duties that belong to them. I have heard lawyers, says D. L., make such a mighty parade of the things which a man must know before he is called to the bar, that, according to the average of human capacities, not one in fifty has the smallest chance in mastering them.

The above remarks of our author will apply with equal truth to our profession. When our learned professors are called upon to deliver their "introductorys," each one eulogises his particular branch, letting the student understand, that it is the most important and difficult to be acquired of all others. The student is easily persuaded; he listens to the eloquence of another professor, who reiterates the same thing in regard to his department; and so on, until the student begins to question his own abilities, and doubt the assertions of the entire faculty. These thought swere impressed on our mind, on hearing our first introductorys.

It is true, that the different branches of medicine may be considered as forming so many segments of one circle, each being indispensable to complete the whole ; an imperfect acquaintance with one, must throw doubt and uncertainty upon another. Hence, a familiar acquaintance with the principles of one division, will aid essentially in acquiring a knowledge of another.

Dr. Latham, however, regards life as too short to waste our *three score and ten*, in trying to grasp the entire circle of sciences ; he thinks a mind originally well organised, and liberally cultivated, quite sufficient to qualify the student to enter upon the study of theoretical and practical medicine.

He holds up to the young physician such men as Baillie and Babington, as bright examples, worthy of imitation. Although the first was an anatomist, and the second a chemist, yet their knowledge did not end in these two departments : they were men of *fact*, of experience, of observation, and the extensive private practice which they enjoyed, brought them in constant contact with the great world, of which, in consequence, they learned so much.

"My experience," says Dr. Latham, "of human life, has long since convinced me, that the number of truly learned and scientific men in the world, is small. Therefore, real learning and real science must be things of difficult attainment, since so many are engaged in their pursuit. But be their *attainment* ever so difficult, it is not half so *difficult* as *their use*."

He continues : "Knowledge may be an encumbrance as well as a help. Many men know more than they are able to wield. There is a point in the acquisition of knowledge, (and this point varies infinitely in different individuals,) beyond which, if more be acquired, the whole mass becomes useless to the possessor. I am acquainted with men who never have *done*, and never can *do* anything, because they *know* too much ; and I am acquainted with men possessing comparatively small knowledge, so dexterous in its use, that they have ridden over the heads of others, far, very far, their superiors in acquirements. Nothing is more common than to hear it said of some eminent and distinguished person : "eminent as he is, what would he not have been, had he possessed the learning of such a one ?" Whereas, if he had possessed a particle more of learning than he has, he would have been nothing at all ; it would have weighed him down, and he would never have been heard of."

The truth of the above remark, few, very few acquainted with the career of men in this democratic country, will gainsay or deny.

If an individual has a strong predilection for a particular profession or department of science, he will be much more likely to distinguish himself by turning the entire strength of his intellect to a particular study, than by endeavoring to master all the branches of a special department.—Hence, few, very few men have distinguished themselves in every walk of science or literature. Heaven has denied us mortals power or capacity to excel in all things : let us be content.

The following remarks of our author are both apt and true :

"I have always thought that hospitals are not converted to half the good they are calculated to serve as schools of medicine; and I think so still. I have always thought, that in hospitals knowledge is perpetually running to waste for the want of laborers to gather it; and I think so still. I have always thought, that in our schools, every mode of lecturing has been unduly exalted above clinical lecturing; and every place where knowledge is to be had, or is supposed to be had, has been unduly preferred to the bedside: and I continue to think thus."

Clinical medicine is too much neglected in this country: we have numerous large hospitals filled with curious and instructive cases, yet not a single American physician, to our knowledge, has given us a work founded on clinical observation and post mortem inspection.—With the single exception of Dr. W. W. Gerhard, of Philadelphia, we cannot, at present, recall any writer who has given to the profession a work constructed entirely out of the materials so abundantly furnished by our large public hospitals! This is strange, but true; and we ardently hope that some one who has the opportunity, industry and talent, will supply this desideratum to the profession.

In the case of our own great Charity, where every variety and grade of disease, from all parts of the habitable globe, may be seen at a single *coup d'œil*, yet how few facts, clinical facts, have been given to the world, from this immense storehouse of clinical medicine!

Many of our physicians frequent the Charity Hospital of this city, and there study the cases, and lay up valuable practical knowledge for *individual* use; but such information perishes with the individual: the profession generally is none the wiser for his pains or his study.—This should not be; it looks almost selfish; and unless some *American Bouillaud* or *Andral* steps forward and puts in the scythe, and gathers the rich harvest of clinical facts, with which our private and public hospitals abound, they will, with the *Cadaver*, be consigned to the unrefunding tomb. Let it not be understood that we wish to reflect upon the medical men of our city; as a body, they are active, able, and highly intelligent; but, mistrusting perhaps too much their own powers, few seek to be known beyond the immediate sphere of their own usefulness.

Beyond the history of a few isolated cases, reported from time to time in this journal, but little has been communicated to the medical profession from this section of country.

But we are glad to perceive a strong desire on the part of some of our physicians, to cultivate *clinical* medicine, rather than speculate upon the theoretical questions of contagion and infection.

Dr. Latham says:

"Diseases are positive conditions and not "abstractions:" they are modes of acting, different from the natural and healthy modes;—modes of disorganizing—modes of suffering—modes of dying: and there must be a living, moving, sentient body for all this."

"This body must be your study and your continual care—your active, willing, earnest care. Nothing must make you shrink from it. In its weakness and infirmities, in the dishonors of its corruption, you must still value it—still stay by it, to mark its hunger and thirst; its sleeping and waking; its heat and cold; to hear its complaints; to register its groans."

Such are some of the arduous duties of the physician—such, some of the ministrations of his office in a world of affliction and suffering!

The following just observation on surgery and medicine, will be generally admitted :

“One reason why surgery is more popular than medicine, is, that it is easier. Do not, I beseech you, imagine, that I wish to disparage surgery. In a profession like ours, nothing can show such bad feeling or such bad taste, as purposely to let fall expressions, which cast imputation of inferiority upon those who happen to cultivate a different portion of the same field of science and usefulness from our own. And even here I will allow, if you please, that cases occur in surgery beset with difficulties and perplexities which we in the department of medicine do not meet with, and which require information and judgment, and skill of the highest order to surmount.”

“But I am now speaking of the ordinary routine of cases, such as we find them in hospitals; and upon a comparison of such cases, surgery is certainly much easier than medicine; and students take to it the more kindly because it is easier.”

“Surgery, for the most part, requires fewer circumstances to bring you to a knowledge of its object, than medicine does. In surgery there are prominent points of interest, which arrest and command the attention at once; in medicine, the points of interest are to be sought after, and, being found, are to be retained and cherished by much labor of the understanding. External sores, external inflammation, and broken bones, require only to be seen and handled in order to be known. But the same knowledge, which in surgery, is obtained by the use of the senses, in medicine, which is conversant with the internal disease, can only be acquired by a process of reasoning; and reasoning is more difficult than seeing and touching, and its conclusions are more uncertain, and much more liable to error.”

Another reason, and one, as we believe, far more influential, is the fact that surgery, when successful, reflects more *eclat* upon the operator—attracts the attention of the public, because generally it deals with a condition of external things easily appreciated by ordinary minds: hence, young men who are ambitious of distinction in the profession, readily yield to the seductive charms of surgery, by which they hope to attract and fix upon themselves the public eye. To understand the qualifications of a surgeon and physician for their respective departments, compare Andral and Bouillaud with Velpeaux and Roux, as fair examples. The two former are great medical philosophers, sound reasoners, acute observers and capable of making deductions from a number of facts; whilst the latter, unacquainted with the true principles of medical philosophy and rigid induction, deal only with isolated cases that can never be brought within the scope of elementary medicine. Enough however on this subject.

Dr. Latham gives the following admirable method of inspecting a case :

“The patient being placed before me, [be it borne in mind that the Doctor is addressing a large class of medical students] I ask him no question until I have learned everything worthy of remark which my own eyes can inform me of. His physiognomy; his complexion, whether florid, pale or dusky; the general bulk of his body, whether large and full or spare and wasted; the condition of particular regions, whether swelled or attenuated, and of the surface, whether there be any sores or eruptions upon it, and what is their character; and lastly, the power of locomotion, whether he have free use of his limbs or not.—If locomotion be hindered, we look well to the brain and spinal marrow; if there be the livid lip and dusky skin, we scrutinize the heart and

lungs; if the whole body or some of its organs be attenuated, we examine well the organs of nutrition."

Having proceeded thus far, he begins to question the patient, if sensible and rational, in regard to his own sensations and feelings; then questions him as to each particular organ, beginning first with the brain and then the lungs and heart, and so on, taking the organs in anatomical order. In this manner Dr. Latham teaches the student practical medicine—by far the most difficult and irksome to the beginner.

It is of the utmost importance to learn to examine a patient with tact; and this is one of the greatest difficulties with which the young practitioner has to contend in the outset.

A few well timed and searching interrogatories will sometimes throw more light on the nature of the case than may be under examination, than two hours conversation with the patient conducted in a careless and disconnected manner.

Before he enters upon his "doctrine of symptoms," Dr. Latham makes the following apt observations upon inflammation:

"Now the study of morbid processes begins with inflammation, and even popular opinion has learned to associate many portentous things with the notion of inflammation; and justly, because the world finds us perpetually talking about it, and perpetually dreading it. Practically, inflammation is never absent from the minds of medical men. Wherever an organ labors—wherever there is pain, the first practical question which we seek to determine is, whether there be inflammation present."

In his "lectures" on the "doctrine of symptoms," Dr. L. treats of the general notion of symptoms:—how they differ from mere signs;—the relation of symptoms to diseases are not the same in all cases.—Symptoms are direct or indirect,—character of each. Direct symptoms respecting the sensations, the functions and structure of the parts affected:—symptoms which respect sensation—pain—its degrees—its qualities.—Amount of information derived from pain as a symptom,—sources of deception arising from it.—2nd. Symptoms which respect functions,—amount of information derived from them, as compared with that derived from sensation—amount from both taken together.—3rd. Symptoms which respect structure,—the information derived from these, to parts within reach of the sight and touch, until auscultation brought the diseases of certain organs within the scrutiny of the ear.

From the foregoing summary, the reader will perceive the workings of an original, independent and reflecting mind. Nothing can exceed the raciness, the clearness and the conciseness with which Dr. Latham investigates and analyses the several subjects embraced under the above title.

We regret our limited space so restricts us that we cannot do more than commend these particular lectures to the careful perusal of every medical man. They unveil the philosophy of medicine and teach us to think and observe for ourselves.

We thank the publishers for a neat copy of these "lectures."

A. H.

IV.—*Elements of Natural Philosophy, being an Experimental Introduction to the Study of the Natural Sciences.* By GOLDING BIRD, M. D., &c., Professor of Materia Medica in Guy's Hospital.

This work was intended by the author as a text book for medical students "whilst attending lectures on Physics, or as preparatory to their entering upon the study of more elaborate works." He was induced to undertake the compilation principally because of the absence of any system of Physics sufficiently extended to include all those subjects which men of education, especially members of a liberal and important profession like that of medicine, ought and are required to be familiar with; and at the same time, not too diffuse, to disgust and weary the student."

The first part treats of the physics of ponderable matter and contains a concise view of the subjects generally embraced under the head of Natural Philosophy. But by far the most interesting, and, to the medical student particularly, the most useful part, is the second section, which treats of the physics of imponderable matter, embracing Magnetism, Electricity, Optics, Light, Heat, &c. We have read with more pleasure the chapter on Physiological Electricity or Galvanism, than any other portion of the work. Electricity is an agent which just now is attracting almost universal attention, and of the character of which we are still comparatively ignorant. It has already been demonstrated, that "muscular contractions are executed by the developement of a fluid in the animal machine, which is conducted from the nerves to the muscles, without the concurrence or action of metals, and that "currents of electricity are always circulating in the animal frame." It remains to be shown by further investigations, how far human life depends upon the quantity or character of electricity existing and circulating in the body, and whether or not death ensues only when the currents are obstructed in their circulation, or when electricity is not evolved in a sufficient quantity, or of sufficient force and intensity to sustain the vital functions. Every muscle in the human body is proven to be an electrogenic apparatus, evolving electricity by the action of the acid fluids or secretions of the tissues exterior to the capillaries, or the alkaline properties of the blood in the vessels of the muscles. How it would have startled a man two centuries ago, to have told him that his body was a reservoir of electricity and at the same time an electrogenic machine! That same subtle agent which we saw flashing from an angry sky and rending the proud oaks of the forest, gave to the delicate flower at his feet its brilliant hues, painted the cheek of beauty with the glow of health, caused the life's blood to pulsate in the veins, controlled the motion of every muscle in his own body and was capable of transmitting intelligence between the most distant points almost in the twinkling of an eye! And yet some bold adventurer in the field of science will probably ere long announce and demonstrate facts equally as startling to the men of the present day.

The learned have manifested great contempt for "Mesmerism," and have treated the "mesmeric state" as a ridiculous humbug. Is it to be accounted for upon any known fact or theory in relation to physiological electricity, and is that intimate and most mysterious connection existing

between the mind, the soul of the magnetiser and the magnetised, to be attributed to the establishment of a current of electricity between their bodies, and the infusion, or rather the transmission of a portion of the one into the body of the other, so that for the time being, the two have no separate existence, but are animated by one and the same life.

But the writer of this article is neither a physician nor a man of science ; and his limits do not permit him to pursue further such speculations, which indeed, "are far beyond *his* power."

In reference to the merits of the work, it will be found useful and highly interesting, not only to the student, but to every one who desires not to be ignorant of the various phenomena which are constantly developing themselves around us. The period has long since gone by, when knowledge was confined to cells and cloisters, and when the means and sources of information were contained in ponderous and forbidding tomes, accessible only to the wealthy and the man of leisure. In our day, the man who earns his living by his daily toil, can find time to acquire such an amount of useful information as will fit him to occupy respectably almost any station, and the necessary books can be procured at a very trifling cost. Men of science like Dr. Bird, who are constantly bringing knowledge more and more within the reach of all classes, are real benefactors, and are entitled to the lasting gratitude of their fellow men.

The work is for sale at Mr. T. L. White's Bookstore, 53 Canal street.

R. H. M.

V.—*The Young Stethoscopist, or the Student's Aid to Auscultation.*
By HENRY J. BOWDITCH, M. D., one of the Physicians of Mass.
Gen. Hospital. Second Edition.—New York.—Sam. S. and W.
Wood.—1848.—pp. 300.

This is the third book treating of auscultation, &c., which has come to hand since our last issue. It is a neat volume of 300 pages, and it must have some claims to merit, as a second edititon was called for soon after the first was issued. We cannot discover much original matter in the work ; indeed the author, we believe, disclaims having attempted anything more than a compilation from the writings of others.

It is written with commendable simplicity and brevity, and is, we think, very well calculated to "aid" the young stethoscopist in the study of auscultation and percussion. The author does not engage in the discussion of any question relative to chest diseases, which is not considered as settled by the profession—in this he is wise.

But notwithstanding the perfection in our diagnostic means in thoracic disease, have we advanced the treatment thereof in anything like the same ratio ? Has any method been devised by which a cavity can be made to cicatrize and heal ; by which tubercles can be driven from the lungs, or, in a word, by which any *serious organic* disease of the lungs or heart can be permanently cured or even relieved ? With a few exceptions we can answer these questions in the negative. We do not

however, despair that the time will come when many of these diseases, now considered incurable, will be made to yield to a more enlightened and rational system of therapeutics. Newton first recognized the laws of gravitation before he could explain or apply them. So in physic; a correct diagnosis must necessarily precede a rational treatment: the latter must be generally based upon the former.

In the sciences and the arts, discoveries are often made years before they can be reduced to practical usefulness. Instance electricity, galvanism, &c., &c.

Physiological and pathological examinations of the blood, must be the means by which is to be brought about a more satisfactory treatment of the diseases of the chest. It is to organic chemistry, then, that the eye of the medical philosopher must be directed in order to institute a sounder system of therapeutics for these affections.

How impotent, now, appears the stethoscopist, when armed with his explorative appliances, he detects a cavity—an extensive hepatization, or valvular disease of the heart! He writes his diagnosis upon the tomb of his patient, and medical science is vindicated!—*“Quid sit futurum cras, fuge querere.”* For Heaven’s sake do not, if you cannot cure your patient, rob him of hope for to-morrow!

Dr. Bowditch goes a little out of his way, though appropriately enough, to give us a short account of “obstetric auscultation.” He describes the “uterine soufflet,” or placental murmur; as also the sounds of the “foetal heart,” including “funic soufflé.”

To Dr. J. D. Fisher, of Boston, he awards the credit of having been the first to apply auscultation to “cephalic” disease. It has not, however attracted much attention, we believe, either in this country or in Europe.

Our author expresses much surprise that “no more use is made of this method (auscultation) in obscure cases of fracture, particularly in those of the hip-joint.” Dr. B. is persuaded that auscultation might assist the surgeon in detecting calculus in the bladder. It has been proposed to use this means of detecting disease of the abdomen; and, we think, as it is of easy application, it might be put to the test in certain cases.

Dr. Bowditch has been enabled, by auscultation, to recognize a case of diaphragmatic hernia. A man received a severe injury, (fracture of spine,) and Dr. B. anxious to ascertain what would be the effect of such lesion upon the action of the heart, applied his “ear over the cardiac region, and was surprised to hear borborygmi exactly similar to those heard in the abdomen.”

At the autopsy almost total absence of the left side of the diaphragm was observed. The colon, stomach, and part of the small intestines were in the chest.

Having engaged to furnish the publisher a certain number of manuscript pages, and being a little short, Dr. Bowditch, with great *hippo-humanity*, concluded to devote a small portion of his work to “veterinary auscultation.” To those who may wish to inform themselves on the thoracic diseases of animals, and especially that noble animal—the horse—we commend the writings of Youatt, Skinner and Percival.

To illustrate the deplorable effects of ignorance of "auscultation in medico-legal cases," we shall quote from the "Appendix" of the work under notice, the following case:—"Among the most deplorable cases on record, in which innocent human life was deliberately taken, according to legal forms, and by the adjudication of the supreme tribunals of the law, that of the child of Mrs. Spooner, who was executed July 2d, 1778, stands pre-eminent. With our present means of diagnosis," continues the writer, "especially in obstetric auscultation, such a case could hardly happen at the present day.

"Mrs. Spooner was convicted of having been an accomplice in the murder of her husband, and after having been condemned, she begged a *reprieve*, on the ground of being 'several months advanced in pregnancy.' For one month they stayed the execution, whilst the court ordered a jury of 'matrons,' or, in other words, of ignorant old women, to be summoned, to decide on the question, whether or not she was 'quick with child.' These twelve *matrons, wise in medical diagnosis!* with two men midwives, decided that she was *not* 'quick with child.' Mrs. Spooner immediately sent to the Council a most touching petition, in which she says 'although the jury of matrons have not decided in my favor, still I am absolutely certain of being in a pregnant state, and above four months advanced in it, and the infant I bear was lawfully begotten. I am earnestly desirous of being spared till I am delivered of it. I most humbly desire your honors, notwithstanding my great unworthiness, to take my deplorable case into your compassionate consideration. What I bear and clearly perceive to be animated, is innocent of the faults of her who bears it, and hath, I beg leave to say, a right to the existence God has begun to give it. Your honors' humane Christian principles, I am very certain, must lead you to desire to preserve life, even in this miniature state, rather than to destroy it. Suffer me, therefore, with all earnestness, to beseech your honors to grant me such a further length of time, at least, as that there may be the fairest and fullest opportunity to have the matter fully ascertained, and, as in duty bound, shall, during my short continuance, pray.' Notwithstanding this urgent appeal of a mother in behalf of her innocent unborn babe, the honorable Council of Massachusetts ordered her execution, and thus inflicted a stain that cannot be erased from the annals of the State; and they did so, notwithstanding that two men midwives and one of the matrons certified that they believed that Mrs. S. was quick with child, and that they had been mistaken in the verdict they had previously given! It was sufficient that Elizabeth Rice and Molly Tattman certified to the contrary, and that they believed her to be not even quick with child."

The reporter continues:—"With an indecent haste, amid the fury of the elements, and the shouts of the crowd, this legal murder was consummated." On the evening of the day of the execution, "the body was examined, as the prisoner had requested, and a perfect male *foetus* of the growth of five months was taken from her."

Comment upon the legal and medical professions in this case is unnecessary; such blunders, we hope, will never again soil the records of medico-legal science. Indeed it seems impossible, with our present means of detecting the existence of pregnancy, to have enacted a

similar tragedy. It is by turning over the records of the past, and contrasting the present advanced state of the medical profession with its condition half a century back, that we are made to feel proud, conscious that it is indeed assuming the character of a science, and as such must be recognized by the world.

A. H.

———Mr. T. L. White, 53 Canal street, has the work for sale.

VI.—*Principles of Physics and Meteorology*. By J. MULLER, Professor of Physics of the University of Freiburg. First American Edition, revised and illustrated with 538 Engravings on wood, and two colored Plates.

Such is the title of what we are told, in the publisher's advertisement, is the first of a series of works on different branches of science, now passing through the press of Baillière in London. Some idea of the plan and objects of the work may be gathered from the following extracts from the Introduction :

"The grand spectacle that is ever present to our eyes in the vast realm of nature, excites within us so ardent a thirst for knowledge, that we feel ourselves irresistibly impelled to the consideration of the combined causes that have produced these wondrous results. Such subjects fall within the department of Natural Philosophy, whose task it is to trace the connecting link between the different phenomena of nature, and, as far as this is possible, to unravel the causes from which they have originated.

The combined natural sciences treat of *bodies*,—a word which we must not receive in the limited sense in which it is understood by the mathematician, who looks only to the relations of space, disregarding the matter that fills space, while it is to the properties of this very matter that the Natural Philosopher devotes his especial attention.

The process by which the Natural Philosopher arrives at his conclusions is exactly the reverse of that adopted by the mathematician. The former 'traces the connection existing between phenomena brought within the scope of our knowledge by means of the senses, and so arranges them that they may elucidate each other', and thus ascends from the effect which he observes, to the causes which produce it; while the latter, beginning with principles well established or self-evident, carries them out to their most remote consequences. The one deals with nature, and boldly dives into the very depths of her mysterious grandeur, deducing from her own mighty works the laws which she has imposed, and herself obeys; while the other, by a process purely speculative, arrives at results which can only be verified by the strictest reasoning and mathematical demonstration, and which can never be brought home to the physical senses. Natural Philosophy is, therefore, a practical science, the materials for which are derived from experience and observation. But our author tells us that 'it is only the external connection of things that can be discovered by perception'; and that 'we can hazard nothing more than hypotheses as to internal causes of phenomena, or the origin of the forces from which they are adduced;' and he adds, with equal simplicity and truth, 'these hypotheses are like questions which we put to nature, but the answers she gives are not simply 'yes' and 'no', but 'it can be so or it cannot.'"

The first section is devoted to an examination of the properties of bodies, that is "those general properties which exist in all bodies,

whatever other differences they may manifest, such as extension, impenetrability, divisibility, etc." After developing from a consideration of divisibility and extensibility the fundamental idea of the atomic theory, an observation is made of the mode in which different bodies are formed from atoms, and become solid or gaseous, as the force of cohesion or the force of expansion preponderates, or fluid when the forces are in equilibrium.

The most important facts and theories are given in relation to statics, hydrostatics, hydrodynamics, pneumatics, the laws of the motions of waves, sounds, the theory of musical notes, the voice, hearing, optics, magnetism, electricity and atmospheric phenomena, which our limits forbid us to do more than merely allude to, with the remark that the various subjects are treated in the most elementary and practical manner, and that the author has managed to embody within so narrow a compass, as to make it accessible to the general reader, a mass of interesting and valuable information, which has heretofore been almost beyond the reach of any other than the student or the man of science.

R. H. M.

VII.—*Elements of General Pathology: A practical Treatise on the causes, forms, symptoms and results of Disease.* By ALFRED STILLE, M. D., Lecturer on Pathology and the Practice of Medicine; Fellow of the Philadelphia College of Physicians, etc. With a Latin motto. Philadelphia, Lindsay and Blakiston. 1848. pp. 474.

One would readily suppose, from the number of works written on pathology, that the practice of medicine had reached such a degree of perfection, as to prolong life beyond the usual period of "three score and ten" years. Such is not however the fact. With pride we acknowledge, that the improvements and discoveries recently made in medical science, have enabled us to detect and define—to cure or relieve, a large number of diseases formerly imperfectly described, and not even suspected.

Dr. Stillé gives, in the first part of his work, a list of authors whom he has consulted in the preparation of his book. This acknowledgment will disarm, in some measure, the critic; but we believe the reader could learn perhaps quite as much of pathology, by consulting any one of the eighteen works referred to by our author, as from the work under notice. Why the necessity then of a new work on pathology? We leave our author to answer this question, and pass on to notice his "*Introductory essay on Medical truth.*"

This is truly a singular paper, and, to our mind, it seems quite out of place, as if "*medical truth*" differed from any other kind of truth. One fact may differ or vary from another; but the expression—the truth—is the same in both instances. "Truth," says Dr. Stillé, "which is in exact accordance with fact or reality, has no *absolute* existence except in the creation of the universe, because no finite mind can stand in any conscious relation to all created things."—

"But medical truth is neither absolute nor revealed; therefore we

have no medical truth, except such as may be considered relative, as contradistinguished from the absolute."

It is after this manner of reasoning that Dr. S. devotes some forty or fifty pages to "medical truth." He has reviewed all the ancient systems of philosophising on medical and natural science, and we find nothing either in his matter or manner of discussing these obsolete subjects that can either amuse or instruct the student or practitioner.

We must therefore regard this "introductory essay" on medical truth as an episode to the more scientific part of the work. As we have not been able to discover in this essay either any new method of detecting truth, or "royal road" to knowledge, we leave the subject in the hands of those more profoundly versed in abstractions which have more sound than sense, and turn to his chapter on "Etiology," where we hope to find our author more felicitous in the exposition of truth than he has been in his efforts to point out the method by which it may be attained.

He divides Pathology into general and special; he explains these terms, and then proceeds to define the difference between morbid anatomy and pathology. His explanation is quite satisfactory, (to himself) no doubt; the former embraces a description of the "physical lesions attendant upon disease;" and it follows from this that the latter relates to the functional changes!

Dr. Stillé proposes in the present work to

"Sketch for the student the general portraiture of disease; to familiarize him with the language employed to describe it; to mark out for him the road he is to travel; to indicate the objects which chiefly deserve his attention, as well as the dangers to which he will be exposed, and the means and instruments he should employ to become acquainted with the one and avoid the other."

In one word, he gives us the elements and principles of general pathology. He begins by giving a general idea of disease, and then enumerates their causes and the manner in which they are generated. After alluding to some of the difficulties, attendant upon a definition of disease, and enumerating the doctrines and opinions on this subject of a few of the ancient medical writers, he endeavors to define disease. He informs us that Wood and Andral define disease to be *derangement*; Chomel, *alteration*; Hardy and Bahier, *modification*; and Williams, *change*. Dr. S., however, after Van Swieten, defines "disease a deviation from health."

It must be admitted that all the above definitions are in part correct; for do we not find *derangement* of the circulation—*modification* of the nervous system—*change* of action, etc. in disease? But is it not easy to conceive derangement, modification and change without the existence of *positive* disease?

Were we to attempt a definition of disease, we should characterize it as a perversion or *lesion* of *nutritive action*; this embraces the entire subject, and moreover expresses the actual condition of the parts—the seat of disease. It comprehends both the fluids and solids of the body, and refers to that molecular change in the organs which characterizes every departure from a healthy,—a physiological standard. But to waive this subject and come to matters of a more useful and practical nature. In his 3d chapter, on "*Aetiology Proper*," or the causes of

diseases, Dr. Stillé has written with much clearness and good sense, and evinces quite an intimate acquaintance with all that has been said on the subject. We can commend this part of his work to a careful perusal. This part of the book seems to have claimed his particular and earnest attention, as it is long and well written, and may be consulted by the practitioner as well as student of medicine.

Dismissing in a few pages the predisposing and exciting causes of disease, Dr. Stillé, in Part 2d., chapter 1st, treats of the "*general phenomena, theory and classification of disease, general diagnosis and prognosis.*" In section 1st, under this head, he treats of the "*type or form of disease.*"

By this is meant the succession of symptoms, or the features which mark the rise, progress and termination of a particular disease. If diseases differ, so do their types; like plants, they possess certain well known characteristics, and by them they may be recognised and classified by the educated observer. Thus, for instance, we have the *continued* and the *intermittent* type; but we believe with our author, that but few, if any disease, can be strictly called continued; at least, modern writers, in opposition to those of ancient times, declare, with few exceptions that all febrile diseases are remittent or intermittent, to some extent. In this particular, diseases bear a very striking resemblance to many of the physiological actions of the system—since almost all the functions of the different organs have their moments of action and comparative repose. Thus digestion is intermittent; respiration, and even the systole and diastole of the heart itself, is but action followed by repose. Let these examples suffice for illustrations, although others much more striking might be adduced, but the reader will readily recall them with a moment's reflection.

In malarial and palludal regions, we think, we may safely assert that the intermittant type of *all diseases* is better marked than in high and elevated latitudes where miasm does not abound.

Such at least is the fact in regard to New Orleans; and we believe the majority of practitioners of this city would endorse this declaration. Thus yellow fever has been described by some as a disease of one *uninterrupted* paroxysm; but as witnessed in this place, it has been found to be remittent in its character. It was this fact, that first induced the physicians of New Orleans to test the efficacy of quinine in the treatment of this formidable disease.

The success which followed the experiment, both justified and ratified the diagnosis.

But to leave this subject and return to the contents of the work under notice. The 4th chapter is devoted to one of the most interesting and, at the same time, important divisions of pathology—"diagnosis."

Without a correct diagnosis, with a few exceptions, it is almost impossible to apply our therapeutics in the treatment of disease, with any assurance of satisfaction to ourselves or advantage to the patient. Hence, Dr. Stillé remarks that "every case of disease presents to the physician two grand problems to be resolved: 1st, to discover its nature, and 2d, to devise its cure." The first is antecedent to the second, and to be successful in the second case, we must be able to de-

termine the first;—hence, “*cæteris paribus*,” a good diagnostician is a safe practitioner.

As Dr. Stillé lays no claim to any thing original in his book, we do not see the necessity of giving it an extended notice. We have looked over the pages before us, and find but little either to cavil at or to denounce; it is a tolerably well written work for the first attempt, and may be consulted by the young student with advantage.

It forms No. 2 of the “MEDICAL PRACTITIONER’S AND STUDENT’S LIBRARY,” published by Lindsay & Blackiston, for 1848. The work is printed on good paper and in medium sized type.

We do not know to whom we are indebted for a copy.

A. H.

VIII.—*Elements of Pathological Anatomy*, illustrated by colored engravings, and two hundred and fifty wood-cuts. By SAMUEL D. GROSS, M. D., Professor of Surgery in the Medical Institute of Louisville; late Professor of Pathological Anatomy in the Medical Department of the Cincinnati College; Surgeon to the Louisville Marine Hospital, &c., &c. Second edition, thoroughly revised, and greatly enlarged. Philadelphia—Ed. Barrington & Geo. D. Haswell. Louisville, Ky.—Jas. Maxwell, Jr. 1845. pp. 822.

The superstitious veneration with which the ancients guarded their deceased friends and relatives interposed serious obstacles to the progress of both healthy and morbid anatomy. Happily for these two co-ordinate departments of science, such prejudices are rapidly fading before a more enlightened era, and a better estimate of the importance and utility of our science to the human race. To the ancient custom of inspecting the entrails of the victim before offering it as an oblation to the gods, may be ascribed the origin of this department of anatomy; any signs of disease in any of the viscera, of the slain animal, condemned it as an unacceptable sacrifice, and it was deemed insufficient to propitiate the offended powers. Thus, the foundation of pathological anatomy, like that of many other sciences, was based upon a superstition as senseless as it was revolting to common sense. By contrasting its humble origin with its present high state of cultivation, we shall be more forcibly impressed with the superior advantages enjoyed by the profession of modern compared with that of ancient times. The contrast is truly pleasing; “*sed multum restat adhaec cognoscere!*”

But we shall now proceed to lay before our readers some of the doctrines advanced and sustained by Professor Gross, in his second edition of his *Pathological Anatomy*. As the subject of “*Inflammation*” is not only one of the most complex and difficult problems in the science to solve, but likewise one that has elicited the most minute investigation from the first order of intellect, we shall make no other apology for devoting our almost entire attention to this subject in the present review.

The first chapter treats of inflammation, that fruitful source of discussion, which has divided the profession for so long a time, and given birth to as many theories as writers on the subject. Until recently the

subject was but imperfectly understood; now, however, more rational views are entertained in relation to this important question.

Since the attention of the profession has been directed to the use of the microscope in the study of morbid and healthy anatomy, more precise and definite views are gradually supplanting the vague speculations and hypothetical reasoning of authors and writers. To study inflammation is to review the science of pathology; to understand it, requires a thorough knowledge of one of the chief elements of organic disease. Although not always present in lesions of structure, yet we conceive inflammation to be most frequently the starting point—the most fruitful source of pathological mutations. It has destroyed more lives than the sword—it attacks the tissues, and frequently lays the foundation for incurable disease; no age—no condition—no temperament may secure us from its terrible ravages; it is the retributive justice of Heaven for the *Promethian* robbery—it is alike witnessed in the verdant regions of the Tropics, and the cold and icy latitudes of the North—in a word, it is ubiquitous; it snatches the infant from its mother's arms, and puts a period to the affairs of tottering age! Then let us study its complex phenomena, and try to understand its real nature, and some of the causes which produce it.

In his "Preface" to his second edition, Dr. Gross uses the following language:

"Disclaiming to be the follower of any man, or school, in medicine, the sentiments I have avowed are the results of my own conscientious conviction, grounded upon personal observation and reflection; and as such, I do not hesitate to submit them to the scientific scrutiny of my medical brethren.

The above is a very unfortunate declaration, and comes with a bad grace from one who is an uncompromising advocate of the so-called physiological—the Broussaian doctrine of inflammation. To the following broad proposition, laid down by our author, we appeal for the truth of our allegation:—

"So it may be assumed, as a general proposition, liable to few exceptions, that all organic diseases, whatever be their state or extent, are the result of inflammatory action, either of an acute or chronic kind."

And, again, he says:—

"Every inflammation, irritation, or morbid action, is originally of a local nature."

This is all for which the physiological pyretologists contend; they insisted upon the local origin of all fevers; they banished idiopathic diseases from their nosology and appeal to local hyperemiæ as the *causa omnium morborum*.

Microscopic investigations have enabled us to analyze the elements of inflammation; to determine the movements and the alterations of the fluids; to detect now in the phlogosed tissue, a dilatation, and now a contraction of the capillary system of vessels; to trace the mutations of form which the blood-corpuscles experience; no one of which phenomena separately, but conjointly, constitute a real genuine inflammation. If Professor Gross means to assert that one or more of the elements of inflammation is invariably present when organic disease is

developed, we think his position quite tenable ; but if, as his language implies, he contends that all the phenomena of inflammation exist simultaneously, then we have studied the subject in vain and Andral and others have preached a doctrine which is no sooner learned than it must give place to another. Hyperemiæ, or a preternatural accumulation of fluids in a part, is one of the conditions necessary to create an inflammation ; this pre-supposes an afflux of blood to the part ; and this latter again admits a pre-existing irritation at the point to which the fluids now tend with increased velocity. The first appreciable change in the state of the vessels, the seat of irritation, is a contraction of the capillaries followed, consequently by a more rapid circulation ; sooner or later, the tone of the vessels becomes lowered—they relax ; the blood halts *in transitu* ; tumefaction, with redness, heat, pain, and the usual concomitants of inflammatory action, are developed ; the blood, now from its arrest, experiences both physical and perhaps chemical changes ; we have an excess of fibrin and a diminution of the hæmatoglobulin, with the solid portions generally.

The microscope often enables us to detect pus in the blood during the existence of certain forms of inflammatory disease. This fact may be verified by an examination of the serum of the blood, according to Gendrin, which will be found to deposit urinary-like sediment, or be turbid and cloudy. In doubtful cases of disease, we should examine, by all the means at our command, the various secretions, &c., for the information thus elicited may enable us to clear up doubts and to institute a course of treatment founded upon reason and the fixed laws of the economy.

Our author questions the existence of mere “functional disease, or, in other terms, a simple aberration of the physiological state, without some change in its anatomical elements.” Such a doctrine, promulgated *ex-cathedra*, and by a man who must have witnessed a great variety of the neuroses, and the congestive forms of fever, will be startling to those who believe in the existence of functional disturbance, though the exact nature of such an aberration may not be demonstrable by the knife, nor the microscope. Under what head would Dr. Gross class *embarras gastrique*, transient pains, ephemeral fevers, hysteria, neurotic spasms, several forms of effusion, common cephalalgia, and the lumbalgia attending intermittent and yellow fevers ? Many of these affections are of transient duration, too fugitive indeed, to lead us to entertain for a moment the idea of a change in the “anatomical elements” of the parts affected. Organic action is either excessive, deficient, or perverted ; if either of these states persist for a length of time, anatomical changes may take place in the structure of the part ; but to assert that every aberration from the healthy—from the physiological state, is a change of anatomical elements, reduces us to the sad alternative of regarding all diseases as material changes of structure, and therefore difficult to cure.

Sad, indeed, would be the lot of both patient and physician, if the former invariably struggled with an organic affection, and the latter had to combat only such as originated in an anatomical lesion.

To quote again the language of the Professor : “All diseases, I feel confident, will ultimately be found to have a local origin and ‘habita-

tion.'” When this great truth shall have been revealed and been acknowledged, the hundred and one varieties, me thinks, will “cease to have a place in our medical treatises.” Rather say, that before this grand discovery shall be made, fevers will cease to ravage the earth, and mankind put on the immortality of the gods!

Will the Louisville Professor assert after this, that he is not the “follower of any man or school? Does he wish us to understand that he has made any new discoveries in pathology, by which the science is to be remodeled, and the old established doctrine of the essential nature of disease overthrown, and a new one reared upon its ruins? If so, he must inflict another book of “huge dimensions” upon the patient public, or bide the time when the age is ripe enough in pathological lore, to receive his now astounding assertions. After alluding to the influence of age, temperament, season, etc., etc., as predisposing or retarding the development of inflammation, all of which is quite familiar to the profession, and noticing the great liability of certain organs to this affection, and the comparative exemption of others, he proceeds to pass some strictures upon the terms used to express the various degrees of intensity of inflammation; hence, he says, “some writers are in the habit of employing the term *sub-acute*, a word,” he continues, “which seems ill-chosen, and consequently conducive to error.” In lieu of this, he adopts the terms mild, slight, or moderate, and uses them throughout the work. In truth, terms are only used in science to express, in a few words, certain definite ideas. The term *sub-acute* is now familiar to the profession; it is, perhaps, as free from objections as any that might be substituted, and we can see no good reason, therefore, for changing technical terms, without adding something to our stock of knowledge. It is said that innovations mutilate the sciences: certain we are, that they should never be introduced from caprice, or from a desire to seem learned. Dr. Gross, following the older writers, has told us really nothing of the essential nature of inflammation; he has described the *phenomena* which characterize this morbid state; he has told us that in the most vascular structures inflammation is most frequently developed; this is doubtless true, yet this fact is as old as physis; indeed his entire chapter on inflammation is so barren of sound views—so far behind the established doctrines of the day—so disjointed in its reasoning, and so little satisfactory to the inquisitive mind, that we find it an irksome task to wade through it. How any man, with Andral, Miller, Thompson, Williams, Travers, Simon, and a number of others before him, could indict such a chapter, on a subject, too, the very foundation of pathological science, we are really at a loss to conjecture. And yet this work is to go before the medical world as an *American* production—as embodying the doctrines and setting forth the views of the profession, in this country, on pathological science! With the exception of Professor Horner of Philadelphia, Dr. Gross is, we believe, the only American who has written a large work exclusively upon pathological anatomy.* It was, therefore obligatory upon the latter to give a fair

* Since we received the above work, Dr. A. Stillé of Philadelphia, has written a book on pathology, quite on a par with that of Professor Gross.

transcript of the actual state of the science, and to incorporate in his work, at least, a synopsis of the science on this side of the Atlantic. "With respect to acute inflammation, the following terminations may be recognized: 1. resolution; 2. effusion of serum; 3. deposition of lymph; 4. suppuration; 5. hemorrhage; 6. softening; 7. gangrene." These various terminations of inflammation are examined under separate heads, but we have been unable to find anything new on these several subjects, frequently, indeed, omitting to mention some of the soundest views lately advanced on the subject.

It is not our aim to condemn the work *toto cælo*; it contains much useful information for the young student of pathology; but to those who have examined the works of the day, from French and English authors, this book will not be, we believe, very acceptable. It is too diffuse—too copious, as an elementary work, yet not sufficiently specific and minute in its details, for a book claiming to embrace the entire subject of pathological science. The engravings and wood-cuts, although not the best, still they may assist the student in understanding some of the points discussed.

In conclusion, we must apologize to the author for neglecting to notice his work earlier; it was written soon after the book was received from the publishers, but was mislaid; we can assure the Professor, however, that this neglect was unintentional on our part.

Professor Gross enjoys a high reputation in the West as an able lecturer, an industrious student and a successful practitioner in his profession.

A. H.

IX. *Review of Mr. Solly's book on the Brain,* with occasional glances at positions and processes in Medical Science.* By B. DOWLER, M. D., of New Orleans.

It would be alike difficult and unnecessary to attempt a critical examination or an exhaustive analysis of Mr. Solly's entire book—difficult, from the multifarious nature of the work itself, seeing that it "amputates the encephalic extremity," thereby decapitating the tegumentary, serous, vascular, muscular, osseous, and nervous systems, including their tissuological, anatomical, physiological, pathological, comparative, typical and morphological relations,—unnecessary, because this treatise, valuable though it be, is not characterized by any striking originality in its facts, speculations, or practical bearings, and being now published in an attractive form, is accessible to the whole profession; its intrinsic merits will insure it a friendly reception throughout the Republic, and the more so, as in this broad land, British authority in physic, if not in government, is oftener over-valued than otherwise. A

* THE HUMAN BRAIN: its structure, physiology and diseases. With a description of the typical forms of Brain in the animal kingdom. By SAMUEL SOLLY, F. R. S., Senior Assistant Surgeon to St. Thomas's Hospital, and Lecturer on Clinical Surgery, etc., etc. From the second London Edition. With one hundred and eighteen wood engravings. 8vo. pp. 496. PHILADELPHIA: LEA & BLANCHARD. 1848.

member of the British Parliament, at the commencement of the American revolution, spoke of the *Island of Virginia*;* but Mr. Solly never appears to have heard of America at all, though he or his clerk must have ransacked nearly all the libraries and journals of insular and continental Europe for references, cases and quotations. For a wonder, here is a foreign book, without the usual baptismal immersion, or even the sprinkling of American notes! Thanks to the reproofs, if not to the politeness—to the independence, if not to the flattery—to the pride, if not to the vanity of British authors—reproofs uncalled for, it may be. "Silence is great." Commercial publishers have read Bentham; they have skill (?) in utilitarianism, with a *bias* for literature, which does not tax them, and consequently they favor free-trade in all the wares and merchandize manufactured by what Mr. Solly calls the "intelligential neurine;"—finally, as the foreign production costs nothing but the trouble of selecting, it is hoped that they will, as in the present instance, choose the best.

Mr. Solly's title page promises, and the best part of the work fulfills the promise, to "give a description of the *typical* forms of the brain in the animal kingdom." It is now more than half a century since Goethe addressed to a lady a short poem on the Metamorphosis of Plants; at a still earlier period, he wrote to Herder these wizard-words—"I must tell you in confidence, that I am very near the whole secret of the generation and organization of plants, and that it is the simplest thing that can be imagined. The Archetypal Plant will be the strangest in the world, which nature herself shall envy me. With this model, and the key to it, one may invent plants *ad infinitum*, which *must* be consistent; *i. e.*, which if they do not exist, yet might exist, and not mere picturesque shows and shadows, but have inward truth and necessity. *The same law will be applied to all animated bodies.*" Was this the day-dream of a mad poet—the distractive phantom conjured up from the ineffable deeps of Germanic romanticism? No. It was the adumbration of a fruitful principle—of a comprehensive method, now going into successful operation, and which analysis serves more and more to confirm. True was the synthetic generalization, or rather the prediction—"It will be applied to all animated bodies." Mr. Solly happily avails himself of this brilliant conception, so advantageous in the investigation and elimination of truth. Here is the broad platform whereon the naturalist and physiologist, the anatomist and pathologist, meet, in order to advance the biological and medical sciences by a method, new, sure, or at least hopeful. Of these harmonious relations of nature, multiformities in unity, particularly of animated nature, Goethe spoke:

"How all weaves itself to the whole—the one acts and lives in the other! How celestial forces ascend and descend, and hand each other the golden pails! How on wings redolent with bliss, they penetrate the earth from heaven—harmonically all pervade the All! All the forms are similar—none exactly alike. A *group* indicates a *secret law*, a sacred enigma. Every animal is its own purpose, originates perfect in nature's womb, and generates

* This fact is given on the authority of the late Ex-President, John Quincy Adams.

perfect children. All its limbs develop themselves in conformity with eternal laws, and the *rarest* form preserves in secret the *primitive type*. If you wish to appreciate the whole, you must see the whole in the smallest. Permit me to venerate Him, who was so great in the magnificence of His creations, as, after making a thousand-fold plants, to comprehend them all in one, and after a thousand-fold animals, to make that one which comprehends them all—Man."

It was, no doubt, from this poet and profound thinker, that the late eminent St. Hilaire derived this fundamental idea of unity of type, as applying to the so-called deviations and irregularities of animated nature, as may be seen in the profound work of this author on Monstrosities.

Mr. Solly's plan of studying the brain, though not altogether the best that is possible in the present state of science, as will be indicated, is, nevertheless, infinitely better than that which he so justly condemns in the following passage in his Preface :

"According to the plan generally pursued in describing the brain, in systematic works of anatomy, the information conveyed amounts to little more than a vain catalogue of names applied to parts, without reference to their structure, their functions, or even their analogies in the nervous system of the lower orders of animals. Such a barren prospect as a list of names, holds out but little to attract the most zealous among students," and "almost deters him from attempting to learn more than is required to prepare him for examination for the diploma;"—"candidates are still very generally required to describe the appearances presented by the brain dissected, or rather destroyed, by the old method of slicing; a method most unphilosophical in its conception, and totally inadequate to impart any real information in regard to the structure of the organ. And, I do not hesitate to affirm, that this mode of examination has contributed essentially to retard the diffusion of sound knowledge, in regard to the anatomy and physiology of the most important system in the body."

But it will be recollected, that these evils were pointed out by Spurzheim long ago :

"Our physiological views do not, it must be evident, allow us to go on cutting the brain into slices: this procedure, indeed, ought rather to be entitled a destruction, than an anatomical demonstration of the cerebral structure; it is precisely as though one should pretend to dissect a leg or an arm by slicing down those members transversely, or to show the structure of the thoracic or abdominal viscera, by treating the trunk in a similar manner, and giving names to the appearances exposed after each successive slice."—(*Anat. Brain*, 102. Amer. Ed. 1834.)

As science is hastening to get rid of this "vain catalogue of names," and is adopting in their stead typological definitions, that is to say, things, it is to be expected that some new terms will arise, characterizing this transition state, and, although neologies can seldom be justified, yet, sometimes, the exigencies of science may render them necessary, in order to give greater precision, and to avoid circumlocutions, which clog the wheels of language. With these views, typology, a self-defining term, compounded, as it is, of two words thoroughly Anglicised, may be worthy of consideration, and the more so as the theory of science of types is, or will be, introduced into the physical, chemical, biological, and pathological sciences. Chemistry may advance by this route, in grouping mechanical, molecular and elementary phenomena,

though an advance in this direction was not anticipated by Goethe, as he seems to regard the inorganic world as a morphological impracticability: "In the mineralogical world," he remarks, "the simplest—in the organic world, the most complex—is the most excellent. These two worlds have quite different tendencies, and no step-wise progress from one to the other is to be expected."

In Zoology this is at once a method of study, and a fundamental principle of classification. It selects an example or individual form (and this applies to disease, morbid anatomy, etc.) combining in the utmost intensity the essential organization or *model* of an entire family through all its ascending or descending gradations, varieties, and even monstrosities.

Now the word Metamorphosis or Morphology, either as a synonyme or substitute for typology, seems wide of the mark. They differ more, strictly speaking, than genera and species; the former is, so to speak, dynamic, the latter static; the one denotes the great general law of transformation, by which change follows change—shape, shape; the other possesses the idea of individualism, fixity, the essential characteristics of many in one, and of one in many, the representative of the whole in a part, and the point of departure for both synthesis and analysis. Morphology seeks to reduce the multiform laws of organisation to one, or to the fewest possible number of ultimate principles; typology is more intent upon seizing prominent criteria in their material, individual manifestations; the one is more profound, the other more practical; the one seeks to solve the problem of organic nature by the most direct route, and by main force, as it were; the other is content with analogues, and with observing such points of contact as may be more easy of access, and of more immediate use; the one is objective, the other subjective.—One word more about another term: Without any anxiety that it should be accepted, I will sometimes use the term *Tissuology*, in this paper, as a substitute for *general anatomy*, as the latter must fall, or rather is falling into desuetude from its absurdity; *histology*, is too uncongenial and difficult, and, as a proof that it has not been accepted, it cannot be found in the latest medical dictionaries, with one or two exceptions. *Tissue* is thoroughly anglicised; *tissuological anatomy* or the science of tissues, is a very comprehensible term.

There can be no doubt that typology has been greatly abused, that is, too exclusively applied, as well in the natural history of the inanimate creation, as in the natural history of animals. There is not any reason to suppose that man was once an atom of carbon, a polypus, an earthworm, or an oyster, from which he was pushed forward by an inherent morphological principle, or by external circumstances, through all the now existing types, from the lowest forms to the top of the biological pyramid, where he now stands in all the sublimity of a rational and immortal being, with a halo of glory about his head, though darkness is before as well as behind him—an enigma to himself, perhaps a wonder to the sacramental hosts of heaven. The history of types and morphological development, gives no assurance that the monkey will ever become the man, or even lay aside his *queue*, or elevate his social or moral condition by an original law of indefinite perfectibility.

Types are but helps, doors, to the temple of human anatomy, physi-

ology and pathology—not only doors, but stair-ways, each analogue is but a step from the lower to the higher, yet a different one from the last ; nor is any step identical with the ultimate object sought after : for the orang-outang is still vastly removed from man in structure, diseases, psychology,—not to mention frogs and the lowest types of animal life upon which so much has been written, and miscalled the medical science of man. There is, too, unfortunately, a *hiatus* at every step.

There is a simplicity in *typology* that contrasts most advantageously with the philological, nosological, encyclopædic and theoretical methods of study—methods often arbitrary, fanciful, tedious. Typology conforms to nature,—has a fundamental principle which tends towards system, unlike the encyclopædic, which works in a retrograde direction, distributing science according to its fortuitous orthography. Thus, if the student wish to study the anatomical or surgical history of the temporal bone, his investigations must vibrate from one extreme of the alphabet to the other—to A for the auditory passage—to Z for the zygomatic process—an evil that even monographs are often not exempt from, when tissues, types, organography, and functions, might afford useful and natural connections, analogies and principles : “nature knows the way.”

Mr. Solly sullies part II., and, indeed, almost every part of his work with Dr. Hall's dogmas concerning the excito-motory system, reflex action, &c., all of which he pronounces “important discoveries”—not as conjectural or hypothetical, but axiomatic philosophy.

Mr. Solly is not simply enamored with this so called discovery, which unfortunately labors under the double disadvantage of being old and unproved, he elevates it into a separate and distinct *system*, and resorts to it in all his difficulties for explanations. He divides the nervous system into three “presiding systems: 1st. the cerebral; 2nd. the true spinal or excito-motory system of Dr. M. Hall; 3rd. the sympathetic,” p. 57.—That relentless destiny which dooms these reflex discoveries to oblivion, is characterized in these wailing words : “*these important discoveries have been passed by unnoticed and unknown by that scientific society, whose object is to promote science and reward real merit. These researches of Dr. Hall have been rendered still more valuable by those of Mr. Grainger, Dr. Carpenter, Mr. Newport, and others, who have shown that one of the most important functions of a nervous system, as it regards the vital existence of an animal, is to receive impressions and react on such impressions, independent of the consciousness or the will of the individual. This fact will be found universal in its application.*”

Whenever the wants of an animal require a consent in the action of its organs, such actions are called into play, and superintended and controlled by a nervous system.” 51.—This is a curious passage. Curious is it that the Royal Society, whether from peccability or impeccability, will neither promote, reward, notice, nor know one of the triune “presiding systems,” nay “an important discovery of universal application;”—more curious is it how “a *function* is to receive impressions and react on such impressions.” An organ, as the optic nerve, may receive impressions. Such reception, or seeing, *is in itself the function*. But the *act* does not become an *entity*, or organ, that is an eye with a retina to receive in its turn other impressions, acting and reacting, impressing and being impressed. But the most curious of all is the claim to a dis-

covery set forth in the closing sentence, the honor pertaining to which is generously divided between Hall, Grainger, Carpenter, and Newport, though it is really due to another, as all must admit upon the historical method of settling questions of priority in discovery whether real or apocryphal. Lamarck* has stated this doctrine with a clearness, argued it with an acuteness, illustrated it with a force, and ornamented it with a splendor of diction not yet equalled, and, at a period long anterior to that indicated in the researches alluded to, as may be seen in his natural history of the invertebrated animals, in eleven volumes octavo, published in 1816. The neurological optimism, not to mention the omnipotency, with which Mr. Solly endows the "neurine, the *true* spinal cord, the reflex action and the excito-motory system," so that all the wants of animals are supplied forthwith, "new actions being called into play, superintended and controled," comes up to the Lamarckian standard. But, oh the stolidity, peccability, and contumacy of the Royal Society† in not "promoting, rewarding, noticing, and even knowing a discovery of a sort of entity, which has a positive power, wisdom, foresight, and contrivance but little short of the Infinite. The glory of discovering this "presiding nervous system," dazzling as it is illusory, must in this instance be given, if not to ages long past, at least to the illustrious, and, it is believed, mistaken Frenchman. He attempts to show, that in virtue of a pre-existing nervous fluid or principle with which all animal organizations are endowed,—a principle allknowing, allcontrolling, allprogressive, allperfecting, and never receding, there is, and must be an inherent and unceasing tendency in animals, not only to go on to perfection in developing existing organs, but to create new wants, (*besoins*) nay *unexpected* wants, and with them organs altogether new!‡ Hopeful philosophy! Progressive age! to feel—*sentir* is the word—to feel unexpectedly a want' (*besoin*), is all that is necessary! The nervous fluid, the neurine, will do the rest—will create, perfect and perpetuate the same, *in sæcula sæculorum*. There was a time, it may be supposed, agreeably to this theory, when crocodilians felt a want of webbing to their fingers and toes, in order to swim with increased facility, and, lo! the web responded to this new *besoin*—not however in all its strength and perfection, until well exercised in its new vocation. But it is grievous to think how tardy these animal perfectibilities are. The crocodilians of the Mississippi are to this day bad swimmers, so poorly are they webbed in their feet. Their wishes, wants, and "*penchants*" have not in any degree advanced or modified this webbing or structure, when compared to the Nilotic crocodile, specimens of which I have examined,—one of which Mr. Gliddon, the learned Egyptologist, presented to me, with the cerements of fifty centuries upon it. Its pedal organs are pre-

* Histoire Naturelle des Animaux sans vertèbres; last Edit. Paris, 1835—1845.

† Mr. Solly takes his "addition" from that society! He puts F. R. S. immediately after his name! If that society be, what Mr. S. represents it to be, why does he not resign? Is it the *prestige* of the word *Royal* in which Mr. Solly glories at the expense of true philosophy? The letters F. R. S. F. S. K. would be better: Fellow of the Royal Society For Suppressing Knowledge.

‡ Hist. Nat. i. 152 *et seq.*

cisely similar to those of the crocodilians of Louisiana. It is doubtful whether other animals (whatever may have been their wants) have done better. Even poets as yet cannot fly.

Lamarck maintains that all animals have from their primordial structure, a progressive principle which develops even the *sentiment*, *besoin* or *penchant*, and this in its turn invariably develops the organic form desiderated whether to fly, swim, or, it might be added, to live forever. Thus says Lamarck, *nature* possesses in herself, not only all the necessary means to form the living body, but to endow it with special organs, developing, varying, multiplying and perfecting the same in an indefinite progression. This is not all,—for the organs and faculties are not only gradually increased in number and elevated in character, but all that is gained and retained for a single lifetime, is transmitted unimpaired to all succeeding generations, being, in fact, wholly physical and imperishable in their nature;* in a word, his fundamental doctrine is, that whenever animated beings, however suddenly and unexpectedly are made, from any cause, to feel a new want, (*sentiment interior*, *sentiment d'existence*, *conscience*) a new movement to that end immediately takes place, the new want is sustained independently of thought, judgment or will, until the new organ is developed and permanently added to the pre-existing organic capital. All the *penchants* of living beings are so many phenomena of organization produced by the internal feelings of existence *itself*.† All of which is done by a hypothetical nervous fluid, or as Mr. Solly would say, by a secretion from the hemispherical ganglion or cortical neurine,—which latter is thus referred to in the tabulated contents: “Source of power—Intelligential agency—Gray neurine the seat of judgment—Reflex functions, gray neurine, volition—Inflammation of the cortical substance,” with other special diseases of this part of the neurine, while the medullary is not less unfortunate and not less mysterious in its motions, sensations, and maladies.

There is a striking analogy, nay many points of contact and identity, between the logic of the Lamarckian and that of the excito-motory schools, though the points of departure are different; the former originating in an assumption—the latter in inconclusive experiments.

It is not a predilection to controversy that prompts these animadversions concerning the reflex physiology which forms the warp and woof of Mr. Solly's work on the Brain. Had the learned gentleman advocated the excito-motory hypothesis, *as a hypothesis*, and not as a perfect discovery, forming a complete system in anatomy, physiology and pathology, no harm would result to tyros, and the most practical utilitarian could not object; for speculation is the life of science, and woe is the day for medicine when it shall be interdicted. There is not a pebble, a flower, or an infusorium, but what weaves for itself, so to speak, a

* He lays down the law thus: La production d'un nouvel organe dans un corps animal, résulte d'un nouveau besoin survenu qui continue de se faire sentir, et d'un nouveau mouvement que ce besoin fait naître et entretient. (*Histoire Naturelle*. T. i, 152 and 176.)

† *Ib.*, i, 214.

web of theory—is related to other forms of existence—is in communication with the universe. The metaphysician is the best reader of physics; the logician best knows how to touch the tangibilities of a fact—to mark its points of contact with the known laws of nature, and to solve that which appears to others unrelated, enigmatical. Speculation is the frequent forerunner to demonstration. But, it may be repeated, that, when hypotheses are pronounced discoveries and systems, inertia, antagonism, resistance, are necessary to keep the doubtful, doubtful. Even the transcendental philosophy is of great value in medical investigation. For the transcendental within the limits of possible experience, the ideal which might be real, the synthetical or *à priori* conception, though altogether beyond the actual, tend to the evolution of the truth. The history of the universe is endless, the curtain never falls, the phenomenal personages of the drama regularly appear, disappear, reappear, but all in vain until the spiritualists become spectators. Transcendentalism is natural to man, and, as the line which divides the possible from the impossible is not known, he is ever attempting to conquer that which is apparently impossible, and sometimes he is unexpectedly successful. He has not explained the causes, origin; end, aim, and essence of matter and mind, time and space, attraction, and repulsion, health and disease, life and death; but he has done a thousand things once deemed impossible. He has the lightning disarmed, and made it the messenger of his thoughts. Hippocrates would have smiled at the mention of painless surgery. Israel, after forty years' journeying from Egypt to Palestine, dreamed not that rail-road cars could in a few minutes traverse an equal space; nor Æneas after seven years' voyaging from Sicily to the Tiber, how much time he lost for want of a steamship. The phenomenal world always existed, but the spiritual comprehended it not. If a sect of physiologists were to arise, professing the indefinite perfectibility of man—for example, the indefinite prolongation of life, they would fail to accomplish their object, but they would not fail to do much good. They would select a salubrious climate, establish hygienic regulations, interdict marriages among the feeble, sickly, deformed, and such as are affected with hereditary maladies. The probable consequence would be an increased degree of general health, and longevity, which might be transmissible from father to son, and even augmented from age to age. For want of the animating spirit of speculation, science often stands still upon a pyramid of facts, every stone of which may be covered with untranslated hieroglyphics rich in meaning.

Dissenting altogether from Mr. Solly's exposition of the secretion, circulation, conduction and phenomenal history of the "power" of the neurine as being "an established point in physiology," I will only examine, in a cursory manner, whether anterior to 1836, the period when the first edition of the work under consideration was published, anything more than mere "glimmerings" had transpired in relation to these opinions.

In his preface, Mr. Solly says, that he has not arrogated to himself the credit of discovering a new system—VIII,—and yet he has by indirection set up the claim for having "established a point in physiology," the height and depth, the length and breadth of which has no

parallel in biological science. If this be true, there is no arrogance, but rather honesty in distinctly setting forth his claim, and defining the nature of his discovery; and every conscientious man is bound to honor the one, and adopt the other now, even now, before interment.

Mr. Solly's book affords incontestible evidence invalidating his gratuitous assertion, that "our predecessors had (only) some glimmering light upon the subject, but their opinions were various and unsettled." He quotes Willis* as saying the cortical substance "*pro-creates*," (or secretes,) while the medullary "*circulates*" the animal spirits (or forces);—the Wenzels, as asserting "that the office of the medullary is not simply that of a conductor," &c.; and thereupon he adds, "I rejoice to derive support to my opinions from such accurate observers"—35—a retrogressive, anachronistic, retractive, stultifying sort of rationalization for establishing a new opinion. Rather let the shades of the mighty dead rejoice in the support which *their* opinions receive, ages after promulgation, from Mr. Solly's "intelligential neurine."

The principal point on which Mr. Solly rests his claim to originality is this: "the cineritious neurine" (pulpy matter) "is the source of power, and the medullary the conductor of it"—positions, as to the animal forces, purely hypothetical, wholly unproved by physiological anatomy, unsupported by direct experiment, and contradicted by all physical analogy, as might be shown at length, were the occasion a proper one for an experimental inquiry. Gall and Spurzheim who were far from being timid in speculating or in assuming principles concerning the brain and its phrenological organography, felt, very rationally, some dubiety exactly on that point wherein Mr. Solly is most credulous, and the manner in which they contest it, shows that it was in their day no novelty. In discoursing on "the use of the nervous pulpy substance," Spurzheim, doubtfully asks, "who will say that the locomotive capacities of inferior tribes, in whose brains the gray substance predominates, are less remarkable than their intellectual endowments?"

M. Foville long ago maintained, according to Spurzheim, "that the superficial substance of the brain presides over the intellectual functions, and the white and deep-seated masses over locomotion."

The reader who will take the trouble to compare the opening pages of Spurzheim on the "Anatomy of the Brain," with Mr. Solly's "Human Brain," will be startled with the similarity; an example will show this:—

SPURZHEIM.

"The nervous system comprises two distinct substances; the one gelatinous, or pulpy, and usually of a grayish or brownish hue; the other fibrous, and of a more or less perfect white color. Dr. Gall and I suppose that each nervous apparatus is composed of two peculiar substances, the

SOLLY.

"There are two kinds," (of neurine or nervous substance,)—"the one is of a gray or ash color, and pulpy texture; the other pearly white and fibrous—the medullary neurine." "Neurine is the substance in which the peculiar powers of the brain and nerves reside."—33, 34.

pulpy and the fibrous, and that both are necessary to produce an instrument adequate to the performance of a particular function.”—17.

“Gall and Spurzheim did not see clearly the relation of the cineritious neurine and the medullary.”—35.

Mr. Solly’s conclusion as to the state of opinion in times past, in relation to the theory he advocates, is, therefore, gratuitous. He will only allow that “our predecessors had *some glimmering* of light on the subject, but their opinions were varied and unsettled.”—34.

It is now nearly half a century since the doctrines of Gall were published. He called the gray substance of the brain, in which the fibrous originates, the *matrix* of the nerves, and regarded the increase of the medullary matter in the adult, compared to infantile period, as giving a corresponding increase of intellectual power, &c.—(Brain II., 273.)

“The pulpy, non-fibrous, gray cortical substance, is anterior to the white, fibrous substance which takes its origin from the former.”—(Ib. VI., 278)

Now, whether this be true or not,—and I know it is not with crocodilian brains—it is very like Mr. Solly’s hypothesis.

Now the truth of the matter probably is this: Mr. Solly borrowed something more than “glimmerings” from our predecessors, particularly from Gall and Spurzheim. Gall and Spurzheim were in the same borrowing predicament with respect to Haller, whose lectures in Göttingen on these topics preceded Mr. Solly’s first edition exactly one century. Let the occidental reader direct his physiological horoscope orientally, to the star that “glimmers” upon the mountains of Switzerland: “We must inquire,” says Haller, “into the nature of this medulla. It is disposed into *fibres*, or parallel threads.”—(Physiol. cccclxxiv.) Mr. Solly calls it medullary fibres or neurine. Haller, (cccclxxvii.) says the medullary fibres are *hollow*—Solly, *tubular*. Haller adds, “it seems to be *certain*, that, from the vessels of the cortex, a *liquor is secreted into the hollow tubes of the medulla*, which, being continued into the small tubes of the nerves, and *propelled to their extremities*, is the cause both of sense and motion.”—(cccclxxxiii. Mr. Solly’s fundamental points, as already quoted, are the same, namely, the cortical *secretes*, the medullary conducts or propels the power so secreted.

It is easy to see that Gall, the most original of the modern writers upon the brain, was indebted to Haller. What others called medullary matter, the Father of Phrenology called the *fibrous* substance of the brain.* Mr. Solly calls it *fibrous neurine*—a neologism of the day, which is repeatedly urged with a zeal bordering on the risible, and with an imprecision not warranted in cerebral technology, already deluged with meaningless names, though some of the former names, as cineritious, gray and pulpy—white, medullary, fibrous—are more definite than “*neurine*,” especially as the latter is used to designate a sort of genus—the brain as a whole, as well as the different kinds of brain, as the gray and white. Besides, these terms are not used without, in most cases, all the other synonymou words that ever were used,

* Brain, II., 84.

and these, too, in juxtaposition ; and yet Mr. Solly says, "it appears to me wrong to use *two* words where *one* expresses the thing."

Mr. Solly calls the hemispheres of the brain, or at least their cortical portions, *hemispherical ganglions*—a better term (possibly) than the old, comprehending, like it, the unimportant idea of figure with the hypothetically important one of function, or ganglionic analogy.

Mr. Solly refers with great self-complacency to his originality in naming the cerebral hemispheres, *ganglia*: "I think," says he, "that we are justified in extending the term ganglion. I am quite sure that it would enable us very considerably to simplify the anatomy of the human brain, &c.; I should denominate the hemispherical ganglia, &c. I gave it that of hemispherical, because it appeared to me the best—I think so still—but *if any other* anatomist had re-named it well, I would have followed him in this edition, sooner than leave it without a name!" —55.

Mr. Solly's book furnishes the most solid evidence that its author has studied Spurzheim, if possible more than Dr. Hall. It is a fact known in America, that *Spurzheim*, on the Brain was republished several years anterior to *Solly* on the Brain in 1836. But lest Mr. Solly should be unacquainted with that which relates to the Western Hemispherical Ganglion of the globe, it may be proper to state that Spurzheim's *English* edition of the Anatomy of the Brain, was published in London, in the county of Middlesex, on the river Thames, in 1826. In that work it is laid down as a law, that "in reptiles, birds, and the *mammalia*, the prolongation of the *two great cerebral ganglions* compose the hemispheres, as they are styled, of the brain."—154. This is not a solitary specimen of Mr. Solly's self-appropriating propensity.

The comparative anatomist sees as he descends morphologically, through the analytic series of inferior organizations, a tendency in the cerebral mass to distribute or disperse itself into simpler forms than that of the human subject, assuming configurations like little brains or ganglions. The *crocodilus Mississippiensis* has, for example, a large olfactory ganglion thrown out between, or rather in advance of the orbits.

It must be observed, however, that this law, so generally received by comparative anatomists, has some marked exceptions : for example, the circulation of the alligator (which I do not now intend to describe) is far more complex than that of man ; perhaps nothing in all physiology is more curious—not obscure—for the adaptations are clearly understood from the habits of the animal. I will not now attempt to describe the great aortal sack with its three sinuses—the right ventricle with two chambers, the left with one—nor the transverse canal connecting the great gastric artery arising from the right ventricle, with the abdominal artery arising from the left,—but, simply ask the believers in the law of an uniform simplicity, as belonging to inferior animals, what do you say of a circulation in which the right side of the heart receives *all* the blood, all the venous blood, sends only *half* to the lungs to be arterialized, the other half being still *venous*, goes out in the stomachic artery to the viscera—a circulation, by which the left side of the heart receives and circulates *only half* of the blood, the right side receiving *all*, without any tendency to inequilibrium? I may here remark, that Mr. Solly's account of the circulation in this animal, is

altogether erroneous—246 : nor is Cuvier's correct. (Anat. Compar. Tome., VI, 204.)

Mr. Solly asserts that "in crocodiles the hemispheres are *wide posteriorly* and narrow in front, something of a heart shape. The cerebellum is *very small* and very simple."—95. These statements are incorrect. The hemispheres are not "wide posteriorly," but *compressed*. Their inferior and anterior portions are, to a small extent, compressed into a conical shape. Their vertical depth, next the falx, is very great. Their transverse horizontal diameter is much less than the vertical, being greatest, not "posteriorly," but through the centre, just behind the orbits. The cerebellum is not "very small," but is, probably in the same proportion as in man, at least in the young animal, though the aggregate quantum of the brain bears a diminished ratio to the increased size and age of the animal. Age, not only alters the relative proportion, but the configuration of the brain, if I may judge from subjects measuring from one to five feet long. This is particularly true of the *hemispheres*, which Mr. Solly regards as the measure of psychological power—a doctrine very favorable to infantile ability, inasmuch as their hemispheres are relatively larger than those of adults. All the evidence which Mr. Solly has borrowed, in Part IV., on the "*weight of the brain*," is opposed to this phrenological opinion of his and of others. For instance, by table III., the weight of the brain, at from one to five years, is as 1 to $8\frac{1}{2}$ of the entire body, while at the ages of from 40 to 50, it is as one to 38 only. Have infants, therefore, four times more intelligence or intellectual power than adults?

Mr. Solly regards each cerebral hemisphere as great overshadowing ganglion—a term, as already mentioned, highly suggestive, but one that is not without some inconvenience, inasmuch as it tends to confound two systems, generally regarded since the days of Bichat as distinct, having but little in common, namely, the cerebro-spinal system, with one centre; and the sympathetic or gaglinary system, having as many centres as ganglions. But, even here, our positive knowledge is little, our ignorance great.* The heart of a decapitated crocodile acts regularly for many hours, whether the spinal marrow and the sympathetic be removed or not.

Mr. Solly calls the cerebellum not only a "ganglion, but a regulator, and co-ordinator of muscular action," having its nucleated dynamic vesicle," &c.!—261. Much of what Mr. Solly and others teach, as the well established physiology of the brain and cord, as the seats of intelligence, volition, sensation, motion, reflex, action and other specialties, require further investigation.

A decapitated crocodile, sometimes watches you, shuts his eyes when he sees any thing approaching them, his pupils contract naturally, and he opens his mouth to bite you. His headless trunk loses the power of linear locomotion, but it perceives, wills, feels, and acts intelligently for many hours. The slightest touch of fire, &c., as in the un mutilated animal, (and I have tied both,) will cause him intense pain; he will remove the fire with his feet, or, perhaps, scratch the knife from your hand, unless tied. Thus, if the headless trunk perceives that an ignited lucifer match or bit of paper is in contact with the body, the animal directs the proper limb of the proper side, to the proper place, forwards, backwards,

or upwards, according to circumstances, in the same manner, as I have seen the unmutilated animal apply its foot to remove an insect from its body. If the headless animal finds that the foot cannot reach that portion of the tail affected, it will bend its body so as to bring it within range, or it will bend the whole body so as to recede as far as possible from the offending cause, and will sometimes roll quite over to the right or left, always in the appropriate direction, and if necessary, it will sometimes direct its hands and feet to places difficult of access, as on approximating the regions between the shoulders or hips—a difficult action, I repeat, for this animal, from its structure, and one contrary to its usual motions—more difficult, perhaps, than it is for the reader to put his hand between his own shoulders. The animal cannot run, either for want of his head, or, perhaps, because the great lateral muscles are divided; but he manœuvres skilfully by means of the numerous short muscles on the side, so as to avoid his enemy. These “intelligential” actions, cannot, according to Mr. Solly’s supposition, proceed from the cerebral neurine, nor yet from the cerebellum, which he calls “a regulator and co-ordinator of muscular motion.”* Now these motions cannot be reflex according to Dr. Hall’s doctrine, endorsed though it be by Mr. Solly: for if the fire be placed so as to be more directly within the reach of the hind legs, though the “spinal arcs” of the forelegs be the seat of the pain, anatomically speaking, it will use the former, that is, the motion will be in the limbs, the muscles and joints of which afford the greatest facility for accomplishing the object, though that portion of the spinal cord and the special *arcs* thereunto belonging, may not be stimulated—a fact expressly contrary to the reflex theory, which assumes that the excito-motory power resides in that portion of the cord whence the nerves arise for the limb that contracts, and there alone.

In speaking of a decapitated frog, Dr. Hall says, “it has not a trace of spontaneous motion; it is without sensibility—without volition.”—(Nerv. Syst. 29.) Mr. Solly everywhere regards this as established physiology!

Is it not a very latitudinarian and unsatisfactory method to define phenomena occurring anterior to decapitation by one rule, and the same phenomena occurring after this operation by another, altogether of a contrary import—the one set to the brain, the other to something else?

* My talented friend, Dr. Young, to whose politeness I am greatly indebted for crocodilians, witnessed, and indeed performed a number of these experiments. It is proper to state that one experiment here recorded, as to the eyes of the separated head winking on seeing an approaching body nigh the cornea, is not confirmed by Dr. Young’s recollections, now, some time after the event. But the Doctor was not present all the time, and made no notes at the moment as I did. It is a matter of little importance compared with the general phenomena, so frequently noticed in both the head and trunk.

It is remarkable that many human subjects possess simple functional contractibility after death, or the extinction of life, in its popular, utilitarian sense, in much greater intensity than crocodilians, while the latter retain under the most extensive mutilations indubitable manifestations of perception, volition, sensation, with appropriate, combined and definite muscular motions.

Will it be said that these phenomena are merely *automatic*? What is the import of this word? It is only necessary to define the term to repudiate this philosophy. *Αυτοματον*—*automaton*, *an engine that goes with a spring*; *any piece of art that seems to move of itself, as a clock*—(Ainsworth);—*a machine that hath the power of motion within itself*. (Walker.) Less mechanical, more ingenious, but equally fallacious are the explanations derived from the excito-motory system. Without going into experiments made not wholly on man, but sometimes upon alligators, (after the complete destruction of the spinal cord,) showing clearly that Dr. Hall's fundamental doctrine is not what Mr. Solly calls it, a fully established system, it may be sufficient to state, again, that the alligator sees after decapitation; for example, if you point a stick at his eye, and cause it to approach the same, before it comes in contact with the cornea, he will close his eye-lids. These facts clearly prove that the *true* "spinal system" has been mis-called. Dr. Hall says:

"The excitomotory motions are never spontaneous; they are *always excited*. As the designation of this system implies, there is always the application of an appropriate stimulus; this is followed by the contraction of *peculiar sets of muscles*. It is clearly proved that the influence of the stimulus is carried along an excitor and incident nerve to the medulla oblongata or the medulla spinalis, and that it is reflected thence along other reflex or motor nerves. The incident excitor nerves, the medulla and the reflex motor nerves, constitute the system. The presence of the medulla oblongata and spinalis is necessary to the contractile functions of the eye-lids."

Now let it be observed that Dr. Hall himself says, that the excitor and motor nerves of the eye-lid and of the eye-ball, the fifth, fourth and sixth, belong to the true spinal or excito-motory system. They constitute one of the main pillars of the reflex edifice. Mr Solly—60—quotes Dr. Hall thus:

"The eye-lids close when the eye-lash is touched, through the same agency of *excitor* and *motor* nerves and of the spinal marrow."

Dr. Hall vehemently asserts the necessity of the contact of a stimulus, mechanical or galvanic, before the eyelids of a decapitated animal can be excited into contraction. "I touched," says he, "the eye or eye-lid [of a decapitated turtle] with a probe. It was immediately closed." Doubtless it was; but the saurian of Louisiana is more "intelligential," as he does not always wait, and, what is more, he repeatedly opens his eyes voluntarily to notice what is going on—the pupil responding to the intensity of the light, while the trunk thinks, feels, wills, and acts. Dr. Hall relates, that having knocked the greater portion of sensation and volition out of a horse's head, by a blow upon the occiput with an axe, he "touched the *eye-lash* with a *straw*—the eye-lid was forcibly closed by the action of the *orbicularis*;" but the destruction of the medulla brought on convulsions, &c., "and the eye-lid and eye-ball remained motionless on the application of stimuli."* It is a fact which the executioner ought to know, that decapitation, and more than all, the destruction of the spi-

* Lect. Nerv. Sys., 23-4; 25-6; 41-2; 28. Amer. Ed.

nal marrow is a quick and sure method of killing. Cutting off the supply of the blood, dividing the muscles or nerves, the loss of respiration, the inhalation of carbonic acid gas, as well as a blow on the head, will arrest the animal forces, but of these coincident facts, not one in itself shows the special tissue in which the muscular force essentially inheres. There was an oriental logician who undertook to prove by facts that he caused the sun to arise at his command, which command, however, he never gave until the proper moment, that is, sun-rise.

In comparative anatomy, (a relation in which Mr. Solly's work deserves great commendation,) organology continues to be the point of departure or fundamental doctrine, and, is, indeed, the most obvious, useful, and successful route for the naturalist, though the pathologist must not stop here. He must know more than the figure, shape, and number of teeth or claws by which one animal may be distinguished from another. Cuvier, who, with all the advantages of Bichat's labors before his eyes, is biased but too strongly in favor of organography, as all who read his latest, most elaborate, and most voluminous work, namely, his *Comparative Anatomy*,* must allow. Cuvier travels from animal to animal, from apparatus to apparatus, in pursuit of analogies in shape, size &c., among the individual organs. But the osteological conformation of the sternum, or the figure of the foot, gives but a limited view of the natural history of the science of tissues. In fact, he has sought in the organography of the alimentary canal, the basis of his classification—a most defective method, except for the study of the vegetative, nutritive or organic life; because, it leaves out the animal life, the functions of relation, that is to say, the greatest and most characteristic criterion for distinguishing the higher from the lower forms of animated existence. The ablest of the French critics do not hesitate to blame Cuvier for this error, particularly M. Comte, in his recent and voluminous work on Philosophy, (*Cours de Philosophie Positive*.) Gall viewed almost the whole circle of biological sciences through his favorite department, that is, cerebral organography: "The faculties of animals are multiplied," he remarks, "in proportion to the complexity of the brain. The existence of particular qualities, whether moral or intellectual, depends solely upon the presence of certain cerebral parts. Place man before you, and contemplate his high and prominent forehead; compare that majestic forehead with the forehead of other creatures, and you will learn why man is man. Place your hand on the front and top of his head, and you will there find the sign of the alliance which his Creator has concluded with him.—*God and the brain! Nothing but God and the brain.*"†

The venerable Past did not place the brain next to God in dignity: "Hippocrates and some later authorities considered it as a *sponge* to absorb humidity; Praxagoras and many others, an *insensible excrecence* of the spinal marrow; Misticelli, an *inorganic mass*; Malpighi, a *confused mass of bowels*; Sabatier and Boyer, an *excretory organ*!" Indecent!

* *Leçon's d'Anat. Comp.*, Second Ed., 9 Vols. Paris. 1835.

† *Func. Brain*, II., 233-4; VI., 291-2.

The degradation of the muscular tissue—the functions of which are illustrated by its structure with a clearness unknown in most other tissues—the ascription of the animal forces, particularly the muscular, to the nerves alone, not to mention the kind of experiments relied on to establish these dogmas, go to prove that the prejudices of past ages still continue. Animal perfectibility, according to Gall and others, is limited only by the nervous matter, and must be measured by the developments of that tissue alone!

That the muscles are incompetent to the performance of voluntary motion after the destruction of their nerves, does not prove that the muscular force is directly the gift of the nervous system. Mr. Solly admits that “the effects of arresting the cerebral circulation shows most clearly that *all* the mental operations are dependent on the flow of blood through the brain for their production. The experiments of Sir Astley Cooper on the rabbit are most interesting and satisfactory.”—44. “Again,” says Mr. Solly, “Sir Astley Cooper, by his experiments with ligatures on the carotid and vertebral arteries of dogs, showed the *dependence of the brain* on its supply of *blood* for the performance of its *functions*. By pressing on the vertebrals as well as the carotids, life was very soon extinguished.”—273. The arrestation of the imaginary secretion of the imaginary fluid by the cortical neurine, the obstruction of the imaginary circulation of the same, in the medullary neurine, could do no more with all its plenary powers. And yet Gall cries “God and the brain! nothing but God and the brain!” Mr. Solly might cry God and the blood! not neurine!

Lamarck invokes the nervous fluid, the existence of which is wholly unknown; he endows it with all the creative power. It makes new organs, and promises indefinite perfectibility to all animated existences. Mr. Solly is scarcely less transcendental in his announcements; he not only ascribes to this fluid a vast amount of functional work, but he gratuitously asserts that the gray neurine secretes it, as the liver does the bile, while the white neurine circulates it, as the heart does the blood. Are these fluids, secretions, circulations and structural adaptations, facts? or are they the incoherent dreams of neurological myths? It is remarkable that while Mr. Solly is so anxious to establish a marked difference between the neurines, he admits that this difference “depends more on the arrangement of the *supporting membrane* than upon any *physical difference* between their elementary particles.”—34.

Of the literary character of Mr. Solly’s work, nothing need be said. The less an author be criticised on account of his peculiar style, the better, provided always that it be accurate, perspicuous, and decent. In these respects, this book is unexceptionable; at least, its inaccuracies are few. The first sentence of the work lays down a proposition exactly contrary to the one Mr. Solly intended to express, namely:

“In the first edition of this work, I remarked, that the structure and functions of the human brain are objects of comparatively slight interest to the medical student!”

He sometimes expresses an opinion as if it were a fact; for instance, he says, “we must not omit the *poison* of scarlatina,” but “determine the *poison* to the skin.”—327. Is this poison discovered? its course

ascertained? Is it visible like arsenic? or does Mr. Solly call the eruption or effect of an assumed *poison*, the material phenomena, the poison itself?

Again, Mr. Solly borrows from Dr. Conolly the history of a "very corpulent woman, subject to hysteria, who described herself as *feeling so well and lively* just before her *worst* attacks that she could not always refrain from *singing, showing*," (adds Mr Solly,) "that the capillary system of the hemispherical ganglion *was at that time in a state of hyperæmia*,"—369,—a mere opinion, and a very improbable one this is. Inflammation of the brain is not ushered in with *songs* and "*feelings so well and lively*." The saddest, the most distractive, the most dolorous thing this hyperæmia is.

The practical part of Mr. Solly's work is really the most theoretical and unsatisfactory. He contributes little, quotes largely, if not wisely. Biased against the positive, he shows a predilection to the dubious. Dr. Bennett's microscopic researches,* and his fourteen conclusions therefrom, concerning softening of the brain,—295, 298—and his own—304, concerning its inflammation, are quite unsatisfactory, particularly the latter, which will be given entire, as follows :

"I believe, 1st. That the inflammation of the *hemispherical ganglion* may be distinguished from the inflammation of the *rest* of the encephalon. 2ndly. That inflammation of the *upper* portion of this ganglion may be distinguished from that inflammation which is at the *base* of the brain. 3dly. That inflammation of the *medullary or tubular substance*, though seldom occurring alone, may be distinguished from that of the *ganglia*. 4thly. That inflammation of the cerebellum has its characteristics;"—

Three dubieties, and one truism. The North Pole, the centre of the moon, every thing "has its characteristics." The top, as well as the base of the brain, the cortical ganglion, as well as the medullary matter, may possibly have diseases distinct from, and independent of, each other. It may be, as Mr. Solly says—304—

"That the hemispherical ganglion is the most important portion of the brain. From its exposed position, this ganglion is that portion of the brain which is most frequently disordered and diseased;"

But his divisions and sub-divisions establish nothing; his symptomatic histories, and his pathological distinctions, are but the disrupted dreams of a chirurgico-theoretical pathologist, not the axioms of a practical physician. The unprejudiced anatomist who shall examine, from fifteen minutes to six hours after death, two or three hundred brains, comparing the morbid appearances with the previous diagnostications made

* The muscular tissue of the alligator affords a good, probably the very best myological type for microscopical observation, being highly organized, and at the same time less absorptive of light than in the human subject, transmitting light sufficient to reveal its structure. I have examined with the microscope several morbid alterations in this animal, and can say without hesitancy, from the transparency of all the soft tissues, that the student will find the crocodile infinitely superior to the human subject for studying several morbid alterations, especially hyperæmia and ulcerations of the serous and mucous tissues; morbid vascularity is transcendently beautiful, &c.

either by himself or others, the most skillful, will find that the maladies of each membrane, and of the different portions of the "hemispherical ganglion, of the cortical, and of the medullary neurine," "inflammations from within, inflammations from without," are not to be distinguished so easily as this transcendental surgeon seems to think.

Mr. Surgeon Solly's fears concerning *Doctor Solly, Pathologist*, expressed in the opening sentence to the latter half of his work on the "Diseases of the Brain," are well founded: "In undertaking," says he, "this division of my subject, I am afraid that by some I shall be considered as stepping beyond the bounds of surgical practice." He gives from all British medicine but a single case illustrative of his favorite position relating to the diagnostics of inflammation of the "hemispherical ganglion"—a case borrowed from Dr. Begley, who borrowed it from Dr. Conolly's institution, and, probably, a more inconclusive and worthless one never appeared in twelve lines of print.—305

"Inflammation of the hemispherical ganglion from within," occupies about one-thirtieth of the entire work—328 to 343. "The cause" (of this "inflammation from within,") "is metaphysical!"—328. Next in order come the maladies of "the *tubular neurine*!" The rest of the book, or rather the whole of the Part XI, comprehending 215 pages, nearly half, is mere mosaic, stuccoed together with little skill.

Probably the best article in the latter half of Mr. Solly's book, is that which treats of apoplexy, and faulty enough it is. He says—

"I think apoplexy is applied too generally to the *effects* of disease, instead of the *cause*. The *classification* of diseases of the brain which I have adopted is founded on *pathology*, not on *symptomatology*.* I propose using it to designate *pressure on the brain*."—363.

Is not that *pressure* an *effect*? Did any tyro ever before, in print, venture on founding a system of diseases "on pathology," and yet exclude, as antithetic and antagonistic, "*symptomatology*," its essentially distinctive feature, seeing that literally,* medically, and popularly, it is the *sine quâ non* of the former?

Mr. Solly describes apoplexy in its most awful form thus:

"A man, apparently in good health, suddenly falls down *deprived of all his senses*, wholly unconscious of surrounding objects. The countenance livid, the vessels of the face and head turgid with blood, the breathing stertorous, slow and laboring: the *limbs lie powerless*; the pulse is full, slow and intermittent: from this state he never rallies, sinks without any change, and dies in the course of forty-eight hours. His brain, when examined after death, is

* From *παθος*, disease, and *λογος*, discourse. *Pathologie*—Partie de la médecine qui traite de la nature, des causes et des *symptomes* des maladies—(Dict. L'Acad.) Richardson adopts Cogan's definition, namely: "it is the history of the sufferings incident to the human frame." *Pathology*,—the doctrine of diseases. It comprehends nosology, ætiology, *symptomatology*, *semeiotics* (signs,) and *therapeia*—(Hooper's Med. Dict.) Disease is not known as a positive entity or substance, but in its symptomatic phenomena; and, therefore, Mr. Solly's "classification of diseases, founded on pathology, not on symptomatology," is as comprehensible as would be a classification of the members of the Royal Society by the weight, number, and shape of their dreams.

found to have been more or less torn and destroyed by extravasated blood.”
—364.

Children the most vigorous and healthy are carried off in half the time named, with cerebral symptoms equally strong, from eating a chesnut, an apple or a pickle, and yet without cerebral *extravasation*.

In sun-stroke, or solar asphyxia, in the first degree, the patient does not live forty-eight hours, nor even as many minutes, and yet no *extravasation* will be found in the *brain*, but always, so far as I have examined, in the *lungs*. That part of the description marked in italics, is not altogether correct, though it is strictly so in cases of solar asphyxia or pulmonary apoplexy. Apoplectics almost invariably feel the lancet, mustard, ice; they often open their eyes, speak, swallow, &c.; but in sun-stroke, never. In fact, Mr. Solly will be sorely puzzled to classify cerebral diseases, even with the aid of symptomatology; and, when he asserts, that “the above phenomena *always* attend the apopleptic seizure,” and that “we must allow that they are *peculiar* to the disease,” it is supposed that all *practical* pathologists and morbid anatomists will dissent from the author; for *they* know full well the difficulty of establishing, on a secure foundation, the connection between cerebral *pathology* and cerebral *lesions*, in many cases, at least.

If Mr. Solly has disavowed the *terra incognita* of cerebral pathology, without the aid of symptoms, he must be exactly the man that medicine has long been seeking for, but as yet, in vain. A treasure he is. It is unfortunate that his book on the Brain does not impart any of the wished for information.

It is distressing to hear the unbelievers in pathological anatomy maintaining that this science is valueless in practice, while, on the other hand, some take for granted that it has already accomplished its mission, not hingmore remaining for it to achieve. And yet, who has even attempted to establish standards for healthy and morbid structure? What can be more absurd than the attempt of the picture-loving pathologists, to *pourtray consistence, ramollissement, cohesion, etc.*? Who has measured, estimated, or even named that variety of vascularity *without injection*, in which the vessels of the cerebral envelopes are *empty, flattened, dilated*? Who has estimated, or even named, a great, a fundamental, and very frequent influence, which modifies very materially after death, cerebral hyperæmia, namely, post-mortem capillary circulation? Who has adopted, as might easily be done, a standard for determining the *tenacity or cohesion* of the arachnoid and pia mater, by detaching a band of a certain width, to ascertain whether it will or will not sustain the weight of the hemisphere, or of the whole brain, or of so many ounces? Who has fixed any standard for *softening*, or as Mr. Solly generally calls it, *ramollissement*? With European dissectors *softening* ought almost always to be found, solely from *post-mortem* changes, owing to the late period of the examination. With respect to real pathological softening, on which Mr. Solly quotes so much, it may be said, that among a great majority of dissectors of little experience and much theory, honest though they be, the chances are about equal in making a post-mortem examination, that the brain will be considered natural, or softened, whatever may be its state, unless in extreme or

very palpable cases, because a standard or test is wanting by which to judge. To say a diamond is as large as a piece of chalk, the brain as soft as a hasty pudding, or to say that the pia mater is vascular, is possibly less vague than Ossian's dimmest ghost, clothed with the mists of Lano, with stars twinkling through his airy form.

I have found, (in the presence of a jury of inquest,) 76 hours after death, on the disinterment of a man who had been in good health until fifteen minutes before death, that the brain had become as soft as pus, or castor oil. I have averaged the published periods after death, at which dissections have been (and continue to be) made by some of the principal pathologists of Europe; the result is about thirty hours! Now what can be learned of softening, hyperæmia, &c., one and a half and often two or three days after dissolution? Mr. Solly does not appear to regard this matter at all, as he often fails to mention the time of the dissection! In his account of the "inflammation of the *tubular neurine*," he gives two French cases, (not mentioning the periods,) and one British case, that of a young lady whose brain in *eighty-four hours after death*, "in the summer of 1844, *was slightly softened, the convolutions of the upper part of the brain were flattened*," etc. It is not good to print such cases in Philadelphia. In New Orleans, where examinations have generally been made in one or two hours after death, a physician would be regarded as having a *very soft brain*, if he were to publish such cases, founding on them important distinctions, and transcendental notions about maladies in different portions of the neurine. Nor is it less a folly in Mr. Solly.

Once more on this topic: Mr. Solly gives two French cases and one British case of post mortem examination, to illustrate meningeal apoplexy; in the latter only is the time given, namely, forty-four hours after death!—of apoplexy of the cerebellum, three French cases—time omitted!—of convulsive affections, one, *idem*.

To sum up the whole :—

Mr. Solly has given thirty-nine post-mortem examinations, and, although many of them are taken from the works of eminent men, there is not, perhaps, one which does not contain internal evidences of imperfection of a fundamental character. Every pathological anatomist, relying on things more than on names—on common sense more than on dogmas, must admit the importance of what may be called *external morbid anatomy*. In this relation, to say nothing of the *internal history*, Mr. Solly's cases will be found defective in nearly all of the following particulars: muscularity, emaciation, venous distention, rigidity, flexibility before or after rigidity, temperature, color of the eyes, skin, &c., abdominal convexity or concavity, color, consistency of the adipose, muscular tissues, &c.; gaseous distention of the cellular membrane, petechiæ, appearances of the blood, cadaveric injection, contractility of the muscles, &c. Other essential matters, often serving as criteria by which to judge of the consistence, &c., or physical history &c., of the internal organs, are not given. For example, if cadaveric injection prevail externally, the same may be expected to happen internally—if the muscles be dark and soft, so will the neurine and other tissues, all, it may be, from *post-mortem alterations*. Of the thirty-nine cases given, the time is omitted in twenty-four, and of the residue, 15, it ranges

from 16 to 84 hours, averaging over 33 hours! one was 48, another 37, another 36, &c. In mid-summer, in a humid climate, "*ramollissement* of the hemispherical ganglion," not to mention the "nucleated dynamic vesicles," ought to happen in less than $3\frac{1}{2}$ days. Pathology gives a *habeas corpus* to all such cadavera, delivering them up to the high court of chemistry.

While Mr. Solly appears to regard Dr. Hall as his Hippocrates, he trenches, nevertheless, upon "the true spinal system." The boundary lines clash. The "Human Brain" claims convulsive diseases, for example, epilepsy, which "the true spinal also claims, and claims exclusively. Pathology, like Poland, is dismembered by the three allied powers, namely, the brain, the sympathetic, and the true spinal. The phrenologist only has a *carte blanche* to practise neurological homicide by decapitation. He is satisfied with your head. The friends can have the body, spinal marrow, sympathetic, organs, all. The pathologist is less local in his plans. The ideas of Bichat—the tissuological resolution of anatomical aggregations, constituting the head, trunk and limbs, and the organs grouped together in the same, or dispersed in remote regions, must ever be kept in view. It is not so much the contiguity, as the tissuological identity of organs that constitutes the true path to philosophical pathology. No tissue will, pathologically speaking, bear decapitation. Decollation takes the parotid gland, but leaves in remote regions, its fellow sufferers in mumps, namely, the mammæ and testes—it divides Mr. Solly's part VIII,—"*cerebral circulation*," from the general circulation—from the functions and diseases of the heart; "*the protective apparatus*," part III, as, bones, skin, &c., from their kindred tissues distributed elsewhere—"the cerebral nerves," part VII, from the sciatic nerves—cerebral convulsions from the sympathetic and spinal.

Are small pox, erysipelas, anasarca, and syphilitic affections of the bones and skin "of the protective apparatus," essentially different from those affections in the pedal apparatus? Can "diseases of the brain," (part XI,) divided into four classes, namely, anæmic, hyperæmic, convulsive and organic, be severed from the main trunk of pathology? Can the *neuroses*—legion is their name—affecting the mind, the sensations, the muscles, &c., be thus mechanically disintegrated, as in the polypus tribe, regardless of the identity and oneness of tissues?

The bones, membranes, and arteries of the brain will bear mutilation far better than the neurological skeleton. Not only the science of tissues, but organology suffers detriment by removing the brain from the residue of the nervous system. Mr. Solly may be less blameable than most other writers in this respect. His title-page relates to the "Brain," and yet much of his work is related, or rather devoted to other portions of the nervous system. The next edition, it is hoped, will deserve a new title, namely, *Anatomy, Physiology and Diseases of the Nervous System*. And may a similar reform be extended, as far as practicable, to every other biological and pathological book! The topographical method is the main pillar of operative surgery. But medicine must advance by monographs founded chiefly on types, morphology, tissuology and organography, not isolatedly, but constantly in reference to the whole, in order to obtain the ultimate end, namely, the establishment of a theory like that of gravitation, or at least a few.

principles which shall harmonise all the phenomena of life, health, disease, &c., worthy of the divine science of medicine, which even now transcends all others in practical good.

An example may be acceptable as illustrative of the influence of *tissue upon organology* in pathological anatomy. While writing these pages, I was summoned to testify in the case of H. M., mate of the British ship Charlotte, charged with the murder of Capt. Smith, commander of that vessel, who, after lingering nine days, died from a penetrating wound of the abdomen, inflicted by the mate aforesaid. The post-mortem examination which I made before Drs. Farrell and Tompkins, (consulting physicians after the first day,) and before the Coroner and jury of inquest, showed an universal alteration of color, consistence and structure of the peritoneum. As this membrane not only lines the abdominal cavity, but forms the outer coat of the organs contained within it, there can be little doubt that the peritoneum equals in extent the external skin covering the entire trunk from the neck to the lower limbs. This extension of inflammation from a focal point is *one* disease, principally confined to *one* tissue, and becomes explicable only by *tissuology*. Now as all the viscera were inflamed on their outer surfaces, it will follow, according to the nosological organographers, that this patient had, besides peritonitis, nearly a dozen of diseases—one for *each organ*, as omentitis, mesenteritis, gastritis, enteritis, hepatitis, splenitis, nephritis, cystitis, etc. The first malady in this enumeration, *omentitis*, was as strongly marked in its morbid characters as can be conceived; for besides its blackness and loss of consistence, it was hypertrophied, that is to say, it was four or five times thicker than when I returned it into the abdomen, a few minutes after its protrusion through the wound. Again, a gastritis could readily be made out by a symptomatologist, or by an organographer. A quenchless thirst, inappetency, and innumerable vomitings for nearly nine days, together with a profound alteration of the serous, and congestion, thickening, and redness of the mucous tissue of the stomach, would leave no doubt in that behalf upon the mind of the veriest skeptic in the gastric pathology.

Again, in purpura, attended with copious and fatal hæmorrhage, I have found in the subserous tissue of every cavity, and of almost every organ, patches or plates of effused blood, much dispersed, but pursuing similar structures.

Since the above article went to press, I have obtained, through the politeness of Dr. Fenner, the very interesting paper of Professor Le Conte, (in the New York Journal of Medicine for November, 1845,) namely, "Experiments illustrating the Seat of Volition in the Alligator." These experiments (which I never read with any attention until now,) refute the universally received doctrine which localizes sensation, intelligence, volition, &c., exclusively in the brain. The French academy thus defines the *sensorium*: "Le point, la partie du cerveau que l'on suppose être le centre commun de toutes les sensations." The brain, according to Dr. Hall, "embraces sensation, perception, judgment, volition and voluntary motion. The motions which result from sensation *always imply volition*."—(Lect. 30.) Such is the general exposition of writers. The nervous system of the alligator, I may remark, resembles that of man much more than could be anticipated from the general

configuration and habits of the animal. Its ganglia and plexuses are less developed; the brain is less convoluted. Let the reader bear these facts in mind while perusing the following extracts; the first from Mr. Solly's "Summary of Part X, on the Physiology of the Brain":

"The mass of evidence in favor of the belief that the great hemispherical ganglion or cortical substance of the brain is the immediate *agent in all mental operations*, is, in my own opinion, so conclusive that it becomes next a most interesting question, whether different portions of this ganglion play different parts."—258. "If there is one point in the physiology of the brain more unequivocally demonstrated than another, it is that the *hemispherical ganglia are the instruments of the mind*—the portion of the brain in which *sensations are converted into perceptions*"—263; "in them resides the power of directing the mind and its organs."—264.

Now instead of these diluted waters of opinion, let the reader look at Dr. Le Conte's experiments, massive as a mountain of granite:

"With the assistance of my friends, Drs. J. F. Posey, J. S. Morel, J. B. Tufts and two non-professional gentlemen, I performed the following experiments on a young alligator (*Crocodylus Lucius*, Cuv.) 2 feet 8 inches long. The animal being well secured, decollation was effected at the articulation of the atlas with the occiput. Not more than two ounces of blood flowed from the wound. Experiments were made upon the head—the jaws snapped at anything which touched the teeth, tongue, or lining membrane of the mouth. After the violent convulsive motions produced by decapitation had subsided, the trunk of the animal remained in a state of torpor resembling profound sleep. But, when pricked or pinched on the sides, he would scratch the spot, sometimes with the anterior and sometimes with the posterior extremity, according to the situation of the injury inflicted. These movements of the limbs were promptly and determinately performed, and were always confined to the members on the side of the irritating cause. If touched below the posterior extremity, on the thick portion of the tail, he would slowly and deliberately draw up the hind foot and scratch the part, and would use considerable force in pushing aside the offending object. These experiments were repeatedly performed, and always with the same results—the animal invariably manifesting slow and determinate movements, accurately directed to some particular spot for a definite purpose.

"Conjecturing that a portion of the *Medulla oblongata* remaining in the upper part of the vertebral canal, might give rise to these exhibitions of apparent sensation and volition, I introduced an iron wire 0-10 of an inch in diameter, with the point reduced to a cutting edge, for the purpose of avoiding compression of the medullary matter in forcing it downwards, into the cervical extremity of the spinal marrow. As soon as the instrument penetrated the canal, violent spasmodic motions supervened—the anterior extremities were forcibly pressed against the table, while the posterior were extended backwards, downwards and outwards, as in ordinary locomotion. These subsided in a few moments, and the animal relapsed into its former state of dreamy torpor. The experiments in pricking and scratching were now repeated, with precisely the same results, excepting that the torpor appeared to be more profound, requiring a longer application of the exciting cause to produce the movements. These motions were never spasmodic or convulsive; they were slowly and deliberately performed. The wire was cautiously forced down the spine at intervals, giving time to repeat the experiments in sensation and volition, at every stage of its progress downwards. Any movement of the instrument produced convulsive motions of the extremities, lasting, however, only a few seconds after its progress was arrested—the limbs which before were flexed, were, at this stage of the experiments, suddenly and forcibly ex-

tended—they remained for some time fixed parallel to one another, and to the axis of the trunk, and then gradually relaxed. These movements were not convulsive or sudden, and were only manifested by the foot on the side of the irritant. The experiments occupied about one hour.”

Prof. Le Conte concludes that this decapitated animal’s “motions appear to have been performed with a perfect knowledge of the end in view; they were directed peculiarly to that end—were volitional; they varied according as the conditions in which they were elicited, altered, and the alteration was always so contrived as to suit the variation in the circumstances; the animal seemed to know, to intend and to accomplish its definite object.”

New Orleans, June, 1848.

Part Third.

EXCERPTA.

1.— *Quarantines. Are measures of seclusion and Isolation useful in the Case of Yellow Fever?*

It is natural to suppose that in all ages, when men have witnessed a pestilential disease spreading by contagion, they have adopted some mode of arresting intercourse between the infected and the sound. But it is only since the doctrine of a specific virus being the cause of each contagious disease was promulgated by Fracastoro, that the practice has been based upon a recognised and philosophical principle. This principle is, as we understand it, that every germ of morbid poison emitted from a diseased person has its term of development, its point of highest vigour, and its period of decline; its life and its extension are alike limited, and although capable of great modification from the nature of the surrounding media, yet the one can under no circumstances be prolonged beyond a certain time, nor the other be extended beyond a certain space. The modern quarantine laws, therefore, fixes a certain arbitrary time as a period of purification, within which the vitality of the poison must be exhausted, and a certain arbitrary space, beyond which the germs, weakened by diffusion, do not spread, or do so harmlessly.

The great objection which has been pleaded against this practice is, as we have said, that its rules are *arbitrary*; they are not founded on observation, which has not yet fixed the length of life of a poisonous germ, nor the distance of its spread; one poison may live only a few hours, and spread only a few feet; another more tenacious, will live many days, and spread for hundreds of yards; the quarantine laws regard each alike, and with manifest injustice and detriment to all concerned, submit the ephemeral and the durable poison to a like treatment.

Another and more important objection is that the principle of these laws is partial; it regards only the poison; it forgets that the poison requires conditions, without which it dies, and becomes innocuous; it leaves to time and natural decay the extinction of a disease which science can, or at any rate soon will, effect at once.

Slowly, and step by step, we are acquiring some knowledge of these mysterious poisons; we recognise that each requires a peculiar condition; one demands a certain heat, another a certain degree of moisture; the typhus poison dies in the tropics, the yellow-fever poison in the arctic regions; the variolous poison will not live in a dry air, the choleric virus in an atmosphere loaded with moisture. And if these distinctions are as yet too doubtful to be applied to practice, and too liable to fallacy to be quoted as immutable truths, we are yet certain of the grand fact that emanations and effluvia, both from animals and vegetables, are, as it were, the matrix or nidus of these poisons, and that the absence of one necessitates the death of the other.

Although with our present knowledge we regard quarantines as necessary safeguards, particularly as applied to poisons which are multiplied rapidly by the human body, or which develop themselves with difficulty apart from this organism, as small-pox or oriental plague, we conceive that the time must soon arrive when their use will be in a great measure abandoned, except in certain countries, and during certain seasons.

Leaving, however, this general view of the subject, and keeping to the question as to the use of quarantines in the case of yellow fever only, we are to consider whether in our West India colonies or in America, the entrance of this disease can by such means be arrested. Quarantines profess to keep out of a place a contagious disease. In the case of yellow fever, what is it they profess to keep out? Those who believe in a specific contagious yellow fever will find no difficulty in answering the inquiry? we, who consider that an accidental combination of influences can generate this disease, find it no such easy matter. Yellow fever is not produced by a single and unique cause; its contagious form we have concluded to be only a variety. Are we to extend a rule applicable to a variety to the fever in all its forms? If the contagious variety is to be excluded by quarantines, can we always tell when the prevalent fever is of this type, or are all vessels which have lost men from yellow fever to be submitted to the same rules? The question is a simple one for this country; we need entertain no dread of yellow fever, unless, as in the case of the *Eclair*, the disease is actually prevalent at the date of arrival here; but in the instance of the West Indies and America the case is not so simple.

Even admitting that the contagious yellow fever could be always diagnosed, it remains to be seen whether the virus can be excluded by preventive measures; whether it be not too diffusible for the allotted space, or too durable for the allotted time. These questions can only be answered by experience, and it so happens that opinions as to the efficacy of quarantines are as much divided as those on the subject of the contagion of the disease. The Report presented by a select committee* to the House of Assembly of the State of New York, is intended to prove the efficacy and the necessity of quarantines. The committee arrived at very decided conclusions. (p. 43.) They state that the yellow fever has been brought to the city of New York either by foul vessels, or by sick persons, or by damaged cargoes, or by the clothes of persons who had died of the yellow fever. Frost destroys the fever, and violent atmospheric commotions check it. The fever is not infectious, contagious, or epidemic in a perfectly pure atmosphere, unless it has been pent up in clothes.† The code of regulations founded upon these conclusions is sufficiently stringent. All vessels direct from any place where "yellow, bilious, malignant, or other pestilential or infectious fever existed at the time of their departure, and arriving between the last day of May and the first of October," are to remain in quarantine for at least thirty days after arrival. No vessel infected with yellow fever is to approach within 300 yards of the city of New York. Passengers arriving in vessels subject to quarantine are not permitted to leave quarantine, "until 15 days after the sailing of their vessel from her port of departure, 15 days after the last case of pestilential or infectious fever which shall have occurred on board, and 10 days after her arrival at quarantine." The committee have therefore fixed 30 days as the maximum period during which a ship or cargo can

* Messrs. Wheeler, Comstock, and Hine; these Gentlemen do not appear to be medical men, but to have taken considerable pains to make themselves acquainted with the subject.

† We do not quite understand this. To it is meant that the virus, *because* it has been pent up in clothes, acquires the power of becoming epidemic in a perfectly pure atmosphere?"

retain the germs of the yellow fever virus, 10 or 15 days as the period in which those germs become manifested in the system, and 300 yards as the distance beyond which the virus becomes innocuous.

Yet, strange to say, the facts brought forward in the Report do not warrant these conclusions, and the period of 30 days in particular appears, from the statements of several believers in contagion, to be very doubtful, and we may remark in this place that the evidence for contagion is not so unequivocal as Dr. Copland's remark at the conclusion of his article on "*hæmagastric pestilence*" would have led us to suspect.

The report consists of three parts. The facts and conclusions arrived at by the committee; the act founded upon these, and various letters and affidavits from medical men and others, in answer to certain queries addressed to them by the committee.

The several epidemics which appeared in New York previous to the year 1795 are, the committee informs us, barely reported; and the little history we have of them is very unsatisfactory. Since 1795, yellow fever has occurred epidemically five times, viz. in 1798, 1803, 1805, 1819, and 1822. Since the latter date the city has been free from epidemic attacks, but sporadic cases are supposed to occur every year. (Report, p. 14.) In all these epidemics the origin of the disease was a matter of doubt and controversy; as far as we can judge from the statements of the committee, their facts are strongly opposed to the importation and even to the contagion of the disease. For proofs of this statement we refer to the letters of Dr. Bayley (p. 7), the Health Commissioners of 1798 (pp. 8, 9), Dr. Rogers, (p. 10), &c. The committee have indeed arrived at a contrary opinion, but on what grounds we cannot understand.

The most valuable portion of the Report consists in the answers to interrogations by ten medical men. Of these ten, five give unequivocal opinions against contagion, viz. Messrs. Harris (p. 112), Manley (p. 165), Hort (p. 173), Reese (p. 211), and Roschenberger (pp. 219 23). One, Dr. Sweetster, considers it to be very slightly contagious, but "by no means like smallpox," and of domestic origin (pp. 155 8); and another, Dr. Wallace, considers it to be imported, "but not communicable by contact." Three decide the disease to be contagious; of these three, one, Dr. Vaché, actually furnishes evidence against the time which the committee considers sufficient for purification (see from page 79 to 94); another, Dr. Francis, states his opinion without a single confirmatory fact; and the third Dr. Townsend, a well-known name in the yellow fever controversy, is a follower of Chisholm and Pym.

If the facts detailed in the report do not bear out the positive conclusions professed to be founded upon them, still less does the history of quarantines in New York indicate that any benefit has been derived from them.

"The state of New York, as early as 1784, enacted quarantine laws, and the examination of them will show that they are scarcely behind those now existing in their practical operation; and yet in 1798, when the city contained only 58,000 inhabitants, between 2400 and 2500 died with the disease the law was passed to prevent. At subsequent periods laws have been enacted and precautionary measures taken; still, in the years 1805 and 1822, the city found itself in the midst of pestilence, and in some other years cases have occurred, but the number has been so small, that the disease could hardly be regarded as epidemic." (p. 41.)

It appears that the cleanliness of the city of New York has greatly increased during the last twenty years; yet it seems that there are still abundant sources of effluvia and nuisances of different kinds, and that although the authorities have taken "*incipient measures*" to establish sanitary regulations, "yet what has been actually done has been far behind the absolute wants, the absolute demands of humanity." (p. 43.)

But we must now quit this Report, and state our general conclusions as to the efficacy of quarantines in the case of yellow fever.

1. As we deem contagion to be only an accidental property impressed on a fever by peculiar circumstances, and as of course this property can only be maintained by a continuance of the same causes which produced it, quarantines may be abandoned as useless in all cases where we feel certain that the poison will not meet with its conditions of existence. These conditions are a certain heat, a certain moisture, and certain effluvia from animals or vegetables. Thus we consider that in this climate, the yellow fever poison would be incapable of development during the greater part of the year, in consequence of the cold, and perhaps through the whole year, in consequence of our habits of cleanliness and ventilation, which if not yet as perfect as they will be, have yet so greatly improved as to have kept at bay, all pestilences during the last century. We consider that the proposition of receiving the sick of the *Eclair* into Haslar Hospital was perfectly justifiable, and we entertain no fear but that, from the ventilation and cleanliness of the hospital the disease would have lost its contagious property, or, in other words, that its self-reproductive property would have become inappreciable.

2. But in the case of the yellow fever districts, where the conditions of existence cannot be so readily removed, the question has a different aspect.* It is true that quarantines have very often failed. They were rigidly enforced at New York in 1822, and yet the disease appeared; in 1797 they were in full vigour at Philadelphia, and yet the disease was not arrested. Dr. Ferguson states that at Martinique, where they established a strict quarantine, particularly against Guadaloupe, "they have been consumed with yellow fevers; but at Dominique, Tobago, St. Vincent's, where they established none at all, they have not had, as far as I could learn, a single case."

In 1810, on Sir W. Pym's arrival at Gibraltar, measures of seclusion and separation were adopted, and soon afterwards the disease declined. By one party this is attributed to the influence of the quarantine measures; by the other to a sudden change of weather, accompanied by a cold north wind, which checked the disease. (See *Cyclop. of Pract. Med.* vol. ii, p. 291.) In 1813, at Gibraltar, the dockyard labourers were strictly separated from the rest of the garrison, and were stated by two officers of the quarantine department to have escaped altogether the disease. But Dr. O'Halloran, who was in Gibraltar at the time, proved satisfactorily that many cases occurred among these men. Dr. Gilchrest remarks on this point:

"During a residence at Gibraltar we had ample means, by referring to the declarations of the official authorities at the dockyards, of confirming the assertion of Dr. O'Halloran as to several cases in 1813 having occurred there as well as some deaths, indeed the names of twenty-three could be given, were it necessary; so that, regarding the original statements, no impressions favourable to the accuracy or candour of the quarantine officers, who made them can be entertained, and in the justly severe remarks of Dr. O'Halloran on the subject, to which no reply has been made, future observers of circumstances connected with the public interest have received a salutary warning." (Op. cit. p. 202.)

Nevertheless, however strong the evidence may be against quarantines, there is no doubt that, as in the case of the *Eclair* and Boà Vista, the Bann and Ascension, the yellow fever, when it becomes contagious, and is imported into a place in the yellow fever region where it meets with its conditions of existence,—conditions which cannot at present be removed,—can only be combatted by such rude contrivances as measures of isolation and seclusion. It remains

* Dr. Hort, in the New York Report, surmises that the gradual improvements in New York and Charleston, and the adjacent countries, have removed these places from the yellow fever region. The northern limit of the yellow fever district is stated by a committee of physicians of New Orleans to be twelve degrees south of what it was a hundred years ago; "and this great result has been accomplished, not by quarantine laws, but by judicious police regulations." (Report, p. 202.)

therefore to be determined by actual observation how these measures may be best and practically applied; that is, we require some certain knowledge of the power of diffusion of the virus, of its durability, and of its rapidity of propagation.

3. On all these points, however, there is great uncertainty. It is essential for the interests of commerce that the quarantine period should be only the time required for the natural death of the poison; but is this 10 days, as some assert, or will it extend occasionally to 50 or 60 days, as Dr. Vaché makes it appear? We have little doubt that the virus will occasionally develop itself gradually in a fit atmosphere, even without acting on human systems, thus at Boá Vista, a soldier of the 5th guard at the Fort on the island was attacked in the Fort, and without having had intercourse with sick people certainly more than 15 days after the departure of the *Eclair*. (Report, case of Jose Sancha, p. 25.) The dates are not very clear, but it must have been as much as 15 days, as the 4th guard, who came on duty when the *Eclair* left, were 12 or 13 days there, and Jose Sancha, who relieved them, was 3 days on duty before he was taken ill. But we do not put much faith in Dr. Vaché's statements, and we have been able to find little additional information on this point from authors.

The incubative period is generally allowed to be from 2 to 12 days; it is possible, however, that it may occasionally be much more than this. Marsh fevers have become developed weeks and even months after exposure to the exciting cause. The incubative period of the African fever appears to have been in many cases 12, 14, 15, even 22 days. (Climate and Diseases of Africa, pp. 151-2-3 and 233.) It appears that the disease did not seem to be of milder type after this long incubative period, and therefore the mere severity of the "hæmagastric pestilence" does not argue a short period of latency.

The quarantine space also has yet to be decided. Five hundred yards has been stated by some writers as the maximum diffusion distance of the poison. The American Commissioners fix 300 yards. A much shorter distance is, however, assigned generally to the febrile poisons of this country, and we doubt whether any accurate rule can be yet laid down for the yellow fever virus when it becomes contagious. From the ready way in which it confessedly loses its contagious property, we should believe the distance to be very small. The degree of adherence of the poison to clothes, blankets, and other articles in daily use, is another point requiring more data for elucidation. We are disposed to give little credence to the usually quoted instances of this occurrence.

It is not our part, however, to go into details on this subject, we have to deal only with principles, and although we believe that quarantines cannot in all cases of yellow fever be abolished,* yet we think they should be considered only as temporary expedients which must some time become obsolete.

The attention of government as well as of individuals should be fixed on the means of acquiring that knowledge which alone can warrant the abolition of quarantines. The poison or poisons of tropical fevers must be more carefully studied. We have supposed that the same virus evolved from certain soils, or productions of the soil undergoing chemical decomposition, produces both the common remittents, and the severe remittents, or yellow fever, the highest or epidemic degree of which is sometimes, though not always, contagious. But it must be remembered that this opinion does not exclude, or rather that it includes, the supposition that there must be many modifications in point of chemical composition produced in this virus by external circumstances of soil, temperature, humidity, occult conditions of the atmosphere, &c. We have seen

* For example, it must be acknowledged that they would have been useful at Boá Vista, or at Ascension.

Humboldt referring to such differences as the only mode of explaining the insusceptibility of the inhabitant of a yellow fever country to the poison of his own district, and his susceptibility to the poison of another district. It is probable that infinite modifications may exist in this poison even in epidemics of yellow fever; for how else are we to reconcile the varying effects ascribed to it, and the difference in symptoms in different years? And although it is not in this generation, nor in the present state of science, that we can look for a solution of these abstruse and profound questions, it is not less true that, without a greater knowledge than we now possess, the practical details of quarantines must remain subjects of doubt and controversy.

This increased knowledge is to be obtained, not by a search after these poisons as they exist in the atmosphere, but by an inquiry into the conditions antecedent to their production, and when they are thrown off into the states necessary to their existence, the means whereby they live, and the sources whence they gain increase and development. In this way only will the question of quarantines receive a complete and satisfactory elucidation. Then only can we expect to reconcile the interests of commerce and the facilities of intercourse with the duties which a protecting government owes to its citizens, and the care which a state professes for the health of its people. It would be well if the medical profession, whose opinion guides the state, would reconsider the grounds of their approval or dislike of quarantines. It would be wise if all parties would agree that in the present state of our knowledge extreme opinions either way are inadmissible;—if they would allow that quarantines must sometimes be admitted to be necessary, but that they must be considered hurtful only when so exclusively considered as to draw off his attention from those internal measures of prevention, which are still more powerful than quarantines in annihilating the pestilential poisons, and in thus arresting the stealthy inroads and the deadly march of these destroyers of mankind.

Since the above article has been in type, we have received a 2d edition of Dr. Bartlett's 'History of the Fevers of the United States.*' This work is divided into four parts; 1. Typhoid fever. 2. Typhus fever. 3. Periodical fever. 4. Yellow fever. The last part is the only one which concerns us at this time, and its consideration will not detain us long, as it is not written from personal observation. Dr. Bartlett informs us, that he "has never met yellow fever at the bedside, and has had no opportunities of direct and clinical study of the disease.

He considers, however, that yellow fever differs altogether from periodical fever. His chief ground for this opinion seems to be that remissions do not occur in yellow fever. "Yellow fever does not belong to the class or family of periodical diseases; it is not properly remittent nor intermittent in its type; *it is not marked by any obvious nor regular series of recurrent phenomena.*" (p. 486.)

The occurrence of remissions is to be attributed to the "periodical element" being engrafted on the true yellow fever. In our former article we referred to this point, and have necessarily anticipated the arguments which refute this statement. This will save us from the necessity of further considering it at this time.

With regard to contagion, Dr. Bartlett does not seem to have arrived at any certain conclusions. He details the three chief opinions, viz. that the yellow fever is always contagious, that it is never contagious, and that it is sometimes or contingently contagious, but he expresses no personal opinion on the subject.

* The History, Diagnosis, and Treatment of the fevers of the United States. By Elisha Bartlett, M. D. Philadelphia, 1847. p.p. 534.

We cannot avoid remarking that this chapter bears evidence, as does indeed the whole section on yellow fever, of a defective acquaintance with the English writers on this subject. Thus Dr. Bartlett quotes the case of the Hussar from Blane's 'Diseases of Seamen,' and criticises the loose and inaccurate account given in this work by Sir Gilbert Blane; but he does not seem to be aware that in 'Select Dissertations,' the account has been amplified and rendered infinitely more conclusive and stringent than it was before. The cases of the Bann, the Kent, the Scout, and others, are left unnoticed; and the only remark made respecting the fever on board the Eclair is, that "it was yellow fever arising during the passage from the coast of Africa, and it occurred at Boà Vista for the *first time*, nearly a month after the departure of the Eclair." (p. 404.) The fever of the Hankey Dr. Bartlett considers to have been "remittent and not yellow fever." (p. 526.)

Dr. Bartlett devotes only two pages to the subject of exemption from subsequent attacks; he details the inquiries made at Gibraltar, to which we have already referred, and then continues,

"Dr. Lewis has investigated this question with some care, and the result of his inquiries differed somewhat from that which I have just given. Five respectable citizens of Mobile, he says, have had the disease as many as three times, according to the testimony of competent judges. As many as 20 of his own patients who were mildly attacked in 1843, stated, that according to their physicians they had already had yellow fever during the epidemics of 1817 or 1839. Dr. Lewis concludes that in 1843, about one fifth of the patients who had mild yellow fever, had been subjects of the disease during previous epidemics." (p. 464.)

The origin of yellow fever from marsh miasmata, is denied on two grounds; 1st. That in countries devastated by periodical fevers, true yellow fever is not seen; 2nd. that in some places subject to yellow fever there are no possible sources from which malaria could be evolved. (p. 466.) The first proposition *per se* is imperfect, as it is not stated whether the malarious districts referred to are in temperate or in tropical climates; a very essential point, as the firmest supporters of the miasmatic origin of yellow fever have always contended, that a high temperature and perhaps occult atmospheric conditions are necessary to produce that modification of miasma, which gives rise to yellow fever rather than to any of the numerous and variable forms of periodical fever. The second proposition also is not yet sufficiently proved. Dr. Bartlett refers to Gibraltar as not containing any of the usual sources of malaria, but to this objection we have already replied. The other examples to which Dr. Bartlett refers, viz. Woodville, Barbadoes, Brimstone Hill, and Stoney Hill in Jamaica, are doubtful cases. It must be remembered, that any place, if it be situated in certain geographical limits, and possess certain other conditions necessary for the propagation of the poison, may be visited by epidemics of yellow fever during the prevalence of those mysterious influences which give so singular a law of periodicity to the origin and recurrence of this peculiar poison; although such place may not present any of the more obvious and universally recognised sources of marsh miasmata.

We observe also, that Dr. Bartlett believes the yellow fever poison to be of "terrestrial origin."

"In regard to the essential poison," he writes, "the application of which to the system, gives rise to yellow fever, I can do but little more than to repeat the remarks which have already been made in connection with the essential etiological poisons of other fevers: the nature and composition of the former, like those of the latter, are entirely unknown to us. It would seem to be clearly enough of *terrestrial origin*." (p. 480.)

This opinion coupled with his uncertainty respecting the contagion of the disease, narrows the question, as far as Dr. Bartlett is concerned, to this point. He has certainly not proved that yellow and periodical fevers are distinguishable by symptoms; he expressly declares the post-mortem appearances insuffi-

cient as diagnostic marks, as he doubts whether the "yellow colour" of the liver described by Louis, is a constant appearance after death, "there is good reason to believe that the change in the colour of the liver is not a constant occurrence." (p. 501.) Therefore we would ask him whether it is not *most probable* that the yellow fever, traced up, as it has been by numerous transition cases, from well-marked and defined periodical fevers, may not be simply a modification of these latter arising from unusual epidemic influences not yet recognised by our imperfect science; or, that if the terrestrial poison be really specific, and give rise to a peculiar disease which will hereafter be recognised by pathognomonic symptoms or by distinctive post-mortem appearances, is it not *most likely* that the source of this poison is to be sought in those same fruitful generative "foyers" from which issue the several allied poisons which, according to inherent differences in themselves, or in the bodies on which they act, produce the multiform varieties of malarious fevers? The symptoms and post-mortem appearances are detailed with clearness and conciseness by Dr. Bartlett. There are several points, however, which would bear discussion did our space permit. We are sorry to find Dr. Bartlett speaking in a disparaging tone of the great mass of English writings, with which, however, he appears imperfectly acquainted, and a deeper study of which would have led to a more complete history of yellow fever. We observe that Dr. Bartlett confounds Dr. John Hunter, the author of the 'Diseases of Jamaica,' with the great John Hunter, (p. 520.) it is an unimportant mistake; we merely point it out as worthy of correction in a future edition.

2.—*Influence of Mind on Disease.* (Address delivered before the Medical Society of the County of Columbia.) By JOSEPH BATES, M. D.

[Communicated by the Society.]

Every period of human life, from cradled infancy to the infirmities that pave the path of second childhood, is surrounded by numberless objects, circumstances and influences, that make deep and abiding impression upon the mind. The multiplicity of subjects in the different kingdoms of nature, and the ever varying interests and objects, by which man is surrounded, and with which he is daily and hourly in contact, invigorate his imagination, challenge his attention, direct his associations, invite to contemplation, and give rise to a variety of moral emotions, that may be more or less intense, and leave traces corresponding upon his physical organization. The balmy days of childhood and youth treasure the mind with ideas, whether false or true, that float down the current of time, causing happiness or misery in all the subsequent stages and conditions of life. The child, taught to believe in the existence of witches and ghosts, in the reality of dreams and signs, &c., is bolstering up his mind with a tissue of absurdities, which all the reason and strength of manhood can never perfectly eradicate. He acquires early in life, the habit of believing in the existence and reality of things contrary to the dictates of reason and the plain truths of philosophy, and thus lays a broad and firm foundation for the fabric of duplicity and credulity, that strengthens with the powers of manhood, and to which he ever after resorts to shield himself from the force and influence of truth and reason. Such are the preliminary elements of his education, that he has a peculiar relish for novelties, and for whatever is mysterious and inexplicable. Ghosts and demons hang upon his imagination by night. Fearful omens absorb his contemplation by day, by which fancy threads the fearful labyrinths of the future. The zephyrs of evening, his imagination converts into the whispers of demons. The fracture of a mirror or the howling of a dog are the premonitions of sickness and death; and his mental emotions may be so intense as to prove the truth of his delusion. With such prelim-

inary elements of education, what can be the mental acquisition and moral fruits of manhood? Let the history of ages break upon your ears in reply. Let the duplicity of the nineteenth century be regarded as its legitimate offspring. Much alarm and anxiety are frequently created by the phenomena of the heavenly bodies; such as the appearance of comets and falling meteors; and so intense as to prove, at least, the exciting cause of various diseases. Many publications have been issued upon these phenomena calculated to heighten delusion and enkindle intense moral emotion. Wars, famine, pestilence and death, they are regarded as being the legitimate harbingers. Within eighteen years a work has been published in England, the author of which traced so direct a connection between the motion of the comet of 1811 and the military movements of Napoleon, that he denounced all persons that denied to comets the character of special messengers from Heaven, as insulters of divine wisdom.

Who can measure the extent of such mental delusion and limit its influence upon the human system, in originating diseases, functional and organic? The primary elements of education, thus briefly alluded to, are conspicuous in all grades of society. They give character to the great mass of communities in all countries. Monarchies, empires and republics, are matured from this nursery. Hence the slow and arduous advancement of philosophy, and the concentrated influence arrayed against the truths and progress of science. Hence the speedy growth of error, and credulity, and superstition. Hence the dizzy heights that mark the soaring flights of quackery. Hence the many morbid manifestations of mind, and the multiplicity of diseases that exist only in fancy. Hence the remarkable cures, wrought through the medium of imagination, and the success that frequently inspires hope and confidence in charms and incantations. The most fatal diseases, and those that frequently bid defiance to rational treatment are disarmed of their ravages, and arrested in their progress, simply through the powerful influence of faith and imagination. If these two emotions can be so controlled as to save large communities from the desolation of the most formidable disease, what important auxiliaries may they not be made in the treatment of disease that more readily responds to medication? In 1743 the plague raged at Messina, and we are informed that the tutelary deity, holy Mary, was taken down and carried in procession through the city. The result of this simple performance was most glorious. The plague stopped immediately. Here, where medicine failed, faith and imagination triumphed. In this instance, the citizens imagined themselves secure from subsequent ravages of that disease, after such an imposing performance, and their faith operated as a powerful mental stimulant, in consequence of which they became less susceptible to morbid influences, and were saved from further alarming mortality. The history of the 17th and 18th centuries is replete with mental delusion. No grade of society was exempt. From crowned heads to the meanest peasant, and the various functionaries of popery, it deluged in blood the greater part of Europe, and left its track of desolation stained with the innocent blood of thousands and tens of thousands of hapless victims. In the small place of Wurtzburg, in one century 8000 individuals were executed for witchcraft. In the same period of time, it is estimated, that not less than 100,000 were sacrificed in Germany. England, France and Spain were in the same deplorable condition. The light of science, the strength of reason and philosophy, were all too feeble to guaranty the least security. The best cultivated intellects and the strongest mental powers were prostrated before its influence, like the forest before a sweeping tornado. Members of parliament and all legislative bodies with the judiciary and clergy, and legal professions of England, Scotland, France, Spain, Germany, &c., were as zealously engaged in the fanaticism of witchcraft, as the same functionaries of the 19th century are in that of quackery. Both are the spontaneous effusions of credulity and superstition. If the judiciary and clergy were the foremost in the delusions of witchcraft, it may with emphasis be said that they remain foremost in the pre-

sent delusion of empiricism. Such men as Sir Matthew Hale, Chief Justice Holt, &c., could as conscientiously condemn witches, as some of our judges do at present commend and encourage the system of Homœopathy, Hydro-pathy, or any other species of quackery. No longer ago than 1716, in England, a Mrs. Hicks and her little daughter, aged two years, were hanged for selling their souls to the devil and raising a storm, by taking off their stockings and making a lather of soap!! This may appear ridiculous to the extreme, but when contrasted with the imaginary shadow of nothing, divided and subdivided until numbers fail to express it, and then a thousandth part to be given at a dose, we must acknowledge that the ridiculous in the extreme was held in reservation for the present century to demonstrate. All mental delusions, when sufficient to convulse nations, and spread alarm throughout kingdoms, exert a powerful influence upon the diseases of such countries.

The light of science and philosophy concentrated, can aid us but a few steps in the investigation of matter, before all is darkness, uncertainty and conjecture. If, in the analysis of ponderable bodies, we soon find a point beyond which they fail to conduct us, we also find that as guides they direct us but a short distance in the examination of mind, and where they fail to afford further assistance, we drop the inquiry. They instruct us, however, in many of the laws to which material bodies yield implicit obedience. They light up our path in the investigation of the laws that govern organization, and in some of the laws that govern mind, but of the essence of mind they teach us nothing. How that mind operates upon matter, and influences physical organization, we know not. Many ingenious theories and hypotheses have been advanced, but they fall far short of rational demonstration; nevertheless, that mental influences do have a powerful effect upon vital organizations we see almost daily verified.

The remarkable influence of faith in the cure and prevention of disease, was doubtless well understood by the sages of antiquity. It is stated that the Romans, in times of pestilence, elected a dictator, with great solemnity, for the express purpose of driving a nail into the wall of the temple of Jupiter. The effect was generally instantaneous; and while they thus imagined that they propitiated an offended deity, they in truth did but diminish the susceptibility to disease by appeasing their own fears. Fear, also, is adequate to cure many diseases. Sometimes the return of a paroxysm of hysteria is prevented by the fear of the remedy resorted to. The emotion has been known to cure an attack of neuralgia. I have witnessed a violent case of bilious colic cured by sudden fright. In the year 1733, General Proxin led a Russian army to the banks of the Rhine. At this remote distance from their native country, his army was likely to be destroyed by home-sickness. Several were daily becoming unfit for duty from this source. The general issued an order to bury alive all who were thus affected. This punishment was inflicted in two or three instances, in consequence of which the disease instantly disappeared from the army. Here was a disease, originating in moral emotions, that threatened to desolate an army, cured by mental influence. The fear of being buried alive was paramount to every other emotion, and probably banished from the mind all recollections of native country and friends, and the numberless associations that cluster around the thoughts of home. The effect of moral emotion upon the genital organs, and through them the whole system, is a subject demanding serious attention. The effects resulting from this source are variable and complicated, and frequently hurry on patients to death's door, while their friends and physician little think of the cause. Fear, and many other emotions, have been known to check suddenly the catamenial secretions. We all admit that mental influences may alleviate or augment many diseases for which we prescribe, and also that they influence the operation of remedial agents, and yet it is to be feared that we pay too little regard to the subject in the medication of our patients.

A modern author remarks that he has seen tumors, ulcers and eruptive diseases increased and perpetuated solely by mental effort directed to the disordered part. If mental emotions are sufficient to change the color of the hair in a few hours, and even effect a change, in the absorbents and secernents of the system; if they are capable of arresting the ravages of the most malignant epidemic, and influencing the growth of tumors, and even causing death, how indispensably requisite that the physician study and labor to control those influences and direct them for the safety and welfare of his patients. The converse of the subject must also be understood, that while these agencies are capable of rolling back the tide of desolation and death, under certain circumstances they may be the cause of perpetuating disease and accelerating death. The criminal has laid his head upon the block for decapitation, his pardon has arrived ere the fatal blow was given, but it came too late to save him; his own mental emotions have been his executioner. Fear has such an influence upon physical organization, and produces such very variable symptoms, that volumes might be filled with its details. It is said that a dream caused the death of Lord Lyttleton; that he expired at the very stroke of the clock; which in a dream he had been forewarned would be the signal of his departure. Had he been put to sleep with opium or some other narcotic, and remained in that condition till the hour of his departure had passed, his life, doubtless, might have been prolonged. "Many of the deaths which take place upon the field of battle, without the slightest traces of a wound, and which have fancifully been attributed to the wind of a flying ball, are beyond doubt, the effects of intense fear." All the symptoms of hydrophobia have been witnessed from fear, as well as the symptoms caused by the bite of the most venomous serpents. In Lesinky's voyages round the world is an account, the truth of which is attested by other navigators, of a religious sect in the Sandwich Islands, who arrogated to themselves the power of praying people to death. (This was previous to the introduction of Christian missionaries upon those islands.) The author remarks, that whoever incurs the displeasure of this sect, receives a communication that a prayer is about to be offered for his death, and the very announcement is sufficient to produce the effect.

Fear has been known to affect the bowels like an over-dose of cathartic medicine. Abortions innumerable have followed in its train of consequences. Mental emotions are regarded by many as having an influence upon the fœtus, during uterogestation, and to be the fruitful source of all the various malformations. This is a subject upon which the medical profession entertain various opinions, and can only be satisfactorily settled by a careful observation of facts. If true, it is wonderful that we have so few cases to register.

The arguments in favor of this theory are plausible, and very ingeniously sustained, but far from being conclusive in all cases. This is a subject that has had its advocates in all periods of the world's history. It is frequently a source of much useless anxiety and alarm among pregnant females. The Old Testament refers to it in the history of Laban and Jacob. Sheep and cattle, during conception, were influenced simply by the sight of sticks partially divested of bark, etc. History, in relating this transaction, refers it to natural causes, rather than any divine interposition to reward the faithful labors of Jacob. Moral effects as well as physical are related by modern authors as the result of mental emotions during utero-gestation. The mother, influenced during this period with a propensity for theft, is said to transfer the same moral obliquity to her unborn offspring. If with a keen relish for intoxicating liquors and her desires not gratified, the child is born to inherit a drunkard's miseries and disgrace. If her mind is absorbed in mathematical investigations, she will give birth to a natural mathematician. If upon the study of the planetary system, to an astronomer, &c. In many

places you will find communities arguing upon these points, as though they might be settled to a mathematical certainty. This theory of mental influence, carried out to its fullest extent, gives an unbounded influence to the mother in forming and controlling the intellectual capacities, habits, sensual gratifications, moral character and future destiny of the world. Were its refutation called for, I think it could readily be proved fallacious. Here is a train of mental influences, that not unfrequently give rise to much useless anxiety and suffering, and many times so intense as to occasion great constitutional disturbance, and morbid function of various organs. The *methodus medendi*, in this class of patients, should not consist in drugs, but in proper moral treatment. Many nervous diseases are caused by the influence of moral emotion. Epilepsy has resulted from sudden fright; and it has been suspended, and even cured by the same emotion. A gentleman travelling in Europe discovered a man sitting in a state of most woful despair, and apparently near the last agonies, by the side of one of the mountain lakes of Switzerland. With great anxiety, he inquired the cause of his suffering. "O!" said the man, "I was very hot and thirsty, and took a large draught of the clear water of the lake, and then sat down on this stone and consulted my guide-book—(this guide-book was written in French, of which language he was ignorant.) To my astonishment," said he, "I found there, that the water of this lake is very poisonous! O! I am a gone man—I feel it running all over me. I have only a few minutes to live! Remember me to ——" "Let me see the guide-book," said the gentleman. Turning to the passage, he found: "*L'eau du lac est bien poisonneuse.*"—the water of the lake abounds in fish. "Is that the meaning of it?" "Certainly." "I never was better," said the dying man, and immediately leaping up, his imaginary pain left him. "What would have become of you," said the gentleman, "if I had not met you?" "I should have died of imperfect knowledge of the French language." Instances are recorded of patients laboring under the impression that they had been bitten by a venomous serpent or a mad dog, and the symptoms caused by such bites have supervened, and only been arrested by convincing the patient of his mistake. Political excitement, no doubt, frequently occasions such intense mental emotions as to produce much functional derangement, and becomes the exciting cause of many diseases. May not persevering and long continued moral emotion, from this cause, light up cerebral affections, which may result in sudden death from apoplexy, &c. Have we not repeated instances of this, in the sudden death of many of our most able statesmen? No doubt that many who have been promoted to high stations of trust and honor, by concentrating their intellectual powers and mental influence, for the attainment of still higher stations, have fallen sudden victims to intense mental emotions. I need not refer to any particular characters, the very announcement calls up a series of associations that cluster around our national Congress. Is it not probable that many a sudden death which has occurred among its members, might be traced to such a cause? In our own State, during the last twenty years, many who have filled the highest offices of trust in the gift of an enlightened and happy people, have fallen from the same cause. Might not timely moral treatment avert such fearful calamities? Mental emotions have left their track of desolation in all grades of society. John Hunter being contradicted by one of his colleagues at the hospital, was thrown into a passion, and struggling to suppress it hurried into another room, when, with a deep groan, he fell lifeless. If moral emotions are sufficient to extinguish the vital spark, in a healthy man, how indispensably necessary, under certain circumstances, that we guard those emotions when the patient is suffering, for instance, with functional or organic diseases of the heart. Dr. Thomas Brown, the founder of the celebrated Brunonian system, had a thorny road to fame, and during many years of his professional career, met

with persecution and disappointments. His mental emotions and exercises were intense, to which he finally fell a victim; having been able to lecture on the day of his death.

Dr. E. H. Bishop, in an address before the Connecticut Medical Society, remarks that disappointment, broken hopes, and protracted gloom, may develop disorganization of the liver, or softened heart. That the strifes of ambition, speculation, or gambling, with their anxieties and vicissitudes, may, after years have elapsed and oblivion shaded the causes, show their effects in the diseased heart and perhaps a sudden death.

He remarks that, grief long continued and indulged, or crushed affections, may leave their impress on the lungs, in tubercles, &c. Grief is said also to bring on sudden death by inflammation of the heart, and rupture of its auricles and ventricles. The cases of sudden death occurring at the time of uttering imprecations or blasphemy, he remarks, result, undoubtedly, from the operations of fear and anger. Where persons have called for curses upon themselves, and fallen as examples of the righteous judgment of God, they have feared in their rage, lest their imprecations should indeed be fulfilled. A physician should be careful not to give his patients unnecessary alarm relative to the issue of their disease. The effects of mental emotion are many times more difficult to control than those resulting from the most fatal poisons. A case is related, and well authenticated, of a girl in a pest house, who, with her companions, [having recently been inoculated for the small pox, so recently that none of the symptoms had appeared,] seeing a drunken physician pass, determined to quiz him; he was called in to see her as very sick; upon examining her pulse, he declared that she would not live twenty-four hours. That she was, indeed, very sick. His prognosis was verified, as she immediately ran down in spite of all means that could be used to prevent it, notwithstanding the attending physician contradicted this declaration of danger, and the one that gave the fright being recalled reassured her that he saw no such symptoms; she died a victim to terror. This case shows, that death may hang upon the lips of a physician. An author remarks, "that quacks sometimes succeed, when the regular medical man fails. An honest practitioner will not hold out to a patient sinking under the influence of a mortal malady, delusive hopes of recovery; but the empiric says, I can cure you,—you shall be cured,—put faith in me and I will cure you. The same author remarks "unless the patient has faith in his physician, but little good can be effected. This is often the secret of success. A patient who had been pronounced incurable, as a dernier resort employed a quack, whose promises were large and gratifying. He was told that he must not expect any change until the expiration of six months. A friend who saw the daily fee and daily deceit, expostulated with the sick man. For God's sake, he exclaimed, destroy not the hopes which that man holds out to me; upon them I live; without them I die. Give a patient the benefit of encouragement, even in doubtful cases, and he is much more likely to recover. Last winter, I was called one evening to ride nine miles, to visit a patient in consultation, who had been sick ten or twelve days with inflammation of the lungs. The messenger was solicitous that I should make all possible speed, as the patient was given over to die, and it was thought he could not survive till morning.

On my arrival I found the house filled with the villagers, who were expecting him soon to breathe his last. His mother had bade him her last farewell, as she supposed, and his friends were weeping around him. He was almost pulseless — pupils much dilated — and, to all appearances, death had marked him for his victim. I thought that possibly the feelings and expectations of his friends around him, which were visible upon every countenance, and then the frequent inquiry if he were not dying, might have a powerful effect upon him. I spoke sufficiently loud for him to hear, that his room must be immediately cleared of visitors, and none allowed to enter, except his

necessary attendants; that they were doing all in their power to hasten his dissolution, and if he did not recover, it would be because they would not give him an opportunity. I closed the door, remained with him alone a short time, examined him rather critically, auscultated his chest, &c., and began giving him the benefits of mental stimulus, encouraged him, told him I never saw a patient with such a tongue unless he got well.

I did not wish his friends to know the amount of assurance I gave him, on account of the uncertainty there appeared to be hanging over the case, knowing that the prognosis I gave him would never come to light unless he recovered. I pursued this plan of encouragement, because I thought it necessary to his recovery, and yet I had many fears lest in the event he might be disappointed. And here I would add, that I deprecate the practice of holding up delusive hopes, to a patient on the verge of the grave. I advised his attendants to change their gloomy countenances, and be more cheerful while in his presence, that they might be the means of contributing towards his recovery. I did not think best to order much medicine; directed a table-spoonfull or two of chicken tea every hour, and two tea-spoonsfull of wine every two hours. His friends and attending physician requested me to call again, unless I should receive notice of his death. I agreed to see him again in thirty-six hours, told him I expected to see him much improved on my next visit. This assurance, no doubt, did him, at that period, far more good than any other treatment he could have received. On my next visit I found a great change, the greatest I ever witnessed in the same space of time. The patient was sitting up in the bed, feeding himself with chicken tea. His recovery was perfect, and I attribute it mainly to the influence of a mental stimulus. Without this, I think the patient must have died under any treatment whatever. I think if I had even represented his case as doubtful, he would have sunk very soon.

Here I wish to relate a rather singular anecdote, in the words of the author by whom it was published. It came out in the *Boston Medical and Surgical Journal*, Dec. 21, 1842.

"In the early part of my practice," says the author, "I was called into a neighboring town to visit a patient. It being about the middle of the day, the old gentleman of the house invited me to stop and dine. While at dinner, he says: 'I don't know as you like my dinner?' 'Why, yes,' said I, 'I do; I like it very well. It is very good.' 'I guess,' said he, 'you don't know what you are eating?' 'Why, yes,' said I, 'I do. It is some new corned beef.' 'Ah!' said the old gentleman, 'it is horse beef.' I replied, 'I don't believe it.' 'It is,' said he, 'I declare it is some of my old mare.' I was not much acquainted with him at that time. I looked at him, supposing him to be joking, but could not discover a muscle of the face to alter or change. I had just taken another piece on my plate, and a mouthfull of the second slice in my mouth, and in fact it was horse meat, sure enough; I could taste it as plainly as my olfactory nerves would discover the scent of an old horse. The more I chewed it, the more disagreeable it tasted. I continued picking and tasting a little sauce, which I could swallow; but the meat, as the negro said, would no go. I at last gave a swallow, as I do with a dose of physic. I thought that I should have thrown the whole contents of my stomach up at table. I afterwards tasted a little sauce, but took care not to put any more meat in my mouth, and kept time with the family. Glad was I when dinner was over. It being cool weather, the old gentleman went to smoking and telling stories. At last he says, 'I won't leave you in the dark about your dinner. I told you we had horse meat for dinner, and so it was. I told you it was some of my old mare, and so it was, for I swopt her away for a steer, and that was some of the beef.' I have ever since been glad that the gentleman put the joke upon me, for I never should otherwise have known how far imagination would have carried me.

"Not long after this I attended a patient, a young man about 18 or 19

years old, in another town, sick with the scarlet fever and throat distemper (*scarlatina anginosa*). I visited him on Sunday morning. I told him he was better, his disorder had turned, he was doing very well, and that I saw nothing but he would recover. I had business further along, and on my return, about sunset, I called again, and behold the family and neighbors were standing around in a large room, seeing him die. I turned to his mother and asked her what was the matter "O!" she said, "Joel has been growing worse ever since you left in the morning." She added, that the minister called soon after I left, and said that he might possibly live till night, but that he could not live till to-morrow morning, and she thought it her duty to let her son know the near approach of death. I went to the bedside, and I verily thought him to be dying; he had a deathly pulse, subsultus tendinum, spasmodic affection of the muscles of the jaws and face, indeed, the whole system was generally convulsed. I thought of the horse beef, and I says to him, "Joel, I guess I can give you something that will help you." I conceived he had his reason, but I believe he could not speak. I immediately got a cordial, and with much difficulty he swallowed a very little. I walked the room, and I saw his eye followed me. I went to him again, got a little more medicine down, felt his pulse, told him his medicine was doing him good, and I guessed he would do by-and-by. I left him again, but took care he did not catch my eye again. I paid attention to him in this way for several hours, until he was really better. The next morning I found him altogether better. He has told me since, I can't tell how many times, that he certainly should have died that night if I had not called as I did."

I have known individuals faint, on looking at a bad wound, also by seeing a person bled. I knew a man before whom it would not do to even speak of bleeding, while he was at his dinner. I have more than once known him so affected by an allusion to bleeding, at the table, that he would hasten to the door to vomit. I recollect an individual whose appetite would fail him instantly on looking into the street and seeing a lame cow pass his door. The sight of a large amount of pus, or an ill-conditioned ulcer, will vomit some as efficiently as a dose of ipecacuanha. The idea of taking an emetic will vomit some, and others will sicken on seeing a patient taking his medicine. An emetic was left for a patient to take; the nurse gave him a tumbler of water, and went into another room to prepare the emetic. The patient supposing the tumbler of water contained his emetic, replied to the nurse, as the medicine was brought in, that he was fearful he had taken too powerful an emetic, and began vomiting. Some have great aversion to swallowing pills; no matter what composed of, the stomach rejects them.

It is a common saying, that sickness and death follow after protracted meetings for religious worship, and this calamity is regarded as a special judgment from Heaven; and I have heard it made use of to heighten the excitement, and to stimulate communities to increased faithfulness in attendance upon the means of grace. I have known this excitement kept up almost every evening for three months. From such continued intensity of mental emotion, crowded houses and exposure from heated rooms to night air, and all the various alternations of atmospherical vicissitudes, one might, without the gift of prophecy, argue *a priori* relative to the results upon the physical organization and the health of communities. I make these remarks, not for the purpose of casting the slightest reflection upon the subject of religion, but to call your attention to the results of such protracted mental exercise and collateral causes in the development of disease. On the contrary, I believe that much good may result from such combined effort, if properly conducted, and that none of the ills is a necessary consequence.

I recollect one of the most severe and alarming endemics, following upon a religious excitement, which had injudiciously been fanned into a flame for a series of months. During the early stage of this disease, the meetings were continued, and many a sincere petition was offered to the Throne of

Grace, that its desolations might be stayed; but nevertheless, if its mortality must continue, that it might fall upon the members of the church, who were prepared for an exchange of worlds, and that sinners might be spared to repent and believe in the Lord Jesus Christ. I have heard it remarked, that this endemic was most fatal in the church. This was doubtless owing to the fact that such became easy preys to disease, in consequence of such protracted mental exertion.

Ought not the physician frequently to attend such meetings, and watch closely the current of mental influence thus occasioned, and if he discover a morbid state of mental emotion, that is likely to result in disease, and the health of the community compromised, is it not then his duty to confer with the clergy upon the subject? We are all familiar with the influence of Mr. Miller's delusion to some extent; but when history shall have carefully treasured up its details and spread out its results, it will be plainly discovered that the fatality of epidemics then raging, was vastly greater in those places where this moral mania spread over the social horizon its baneful and withering influences, filling the inhabitants with consternation by the daily expectation of beholding the Saviour of the world, and the conflagration of the universe. A malignant erysipelas, it is well known, then left its track of desolation far and wide, and many were at death's door within a few hours after its attack. Newspapers and medical journals frequently alluded to that disease, as something new and peculiar. Many times it was spoken of as the black tongue, and its appearance was regarded as the harbinger of death, and added fright to the excitement and terrors of the advent doctrine, and thousands fell ready victims. There might well be expected something peculiar in the progress and termination of any disease, when it makes its appearance during such intense mental emotions.

The history of many asylums will record as a cause of insanity in many inmates at that time,—the advent delusion. Suicide and insanity that marked its progress, was but a drop in the ocean, contrasted with the general results of that belief. Fear and anxiety, blasted hopes and ruined fortunes, and despair, agitated the social elements and prepared victims, who fell an easy prey to that peculiar, so called, erysipelas epidemic, that overran almost every section of our republic. Is it not a fact that in those places where Millerism most extensively prevailed, there sickness and death left their deepest and widest track of ruin? Would not a history of the former lead to the destruction of the latter? I do not intend to convey the idea that this disease was an effect in all cases of this belief; neither that every patient was a proselyte to this delusion; but that this erysipelas was far more malignant and fatal where this exciting subject most generally prevailed. Diseases are said to change, and even the same disease to require very different medication one year from what it does another. I know this is referred sometimes to one cause and sometimes to another. Often said to depend upon some peculiar epidemic influence in the atmosphere, &c.; and yet, little or nothing is said of different public sentiments and moral emotions that agitate most intensely communities and convulse nations, as being the cause.

During the existence of malignant epidemics, we should fall far short of doing our duty faithfully, if we neglect altogether moral causes and treatment. A patient who has unshaken confidence in his physician, (other things being equal,) is much more likely to recover than one who has lost all reliance in the skill of his medical attendant. He who labors to destroy this confidence, is diminishing the chances for the patient's recovery, and is morally accountable for such influence. There may be instances in which you might as well resort to physical means for the destruction of the patient as to ruin his reliance upon his physician. Hope and confidence to the sick are, under certain circumstances, the most salutary in their results, and operate as stimulants or tonics to keep the patient from

sinking. It has been said, and I might add, proved, that while visiting his patients, life and death hang upon the lips of the physician. And there are times when a fearful issue may be too plainly indicated in his countenance. Sorrow and sadness should never be allowed to mantle his visage, so long as the least ray of hope or rational expectation can be cherished for his patient. No matter what may be the disease, where located, or what particular function may be in danger, when the patient is balancing, as it were, between life and death, he should be allowed all the benefits that can be imparted from judicious moral treatment. Deprive him of this, and let despair displace hope and confidence, and you have but little to expect, simply from drug medication. So long as the patient retains perfectly his reason and mental powers, as a general rule, I would explain to him, in a few words what I expected to accomplish by his medicine, and state it with confidence.

A patient laboring under the impression that he has taken a powerful cathartic, when he has taken nothing but a Dover's powder, has been known to have been as effectually purged, as though he had taken a dose of Croton oil. Sometimes, however, you find instances where you had better say but little about what you administer, and but little relative to its results. Some patients will take opium to advantage, provided they do not know that they have taken it; but just inform them you have given this article, and they are worse than though you had given them nothing. There can no general rules be given in relation to informing the patient upon this subject, that does not admit of many exceptions. Did time permit, I might go on to instance the different varieties of mental emotions, and their various results, according to their intensity, upon individuals, during the ravages of disease, and in times of health. I might cite you to the multiplicity of cures wrought by Mesmerism, as the result of mental influence, &c.

The physician has it in his power to a very great degree, to convert mental emotions into agents of life or death. It is, no doubt, owing to this ability that some possess, during wide spreading epidemics, that makes such a striking contrast in the success attending different physicians. One relies solely upon the efficiency of drug treatment, while the other, having no less confidence in that, combines with it no little share of moral treatment. No one who has neglected the influence of the latter, can appreciate its worth or fathom its results. There may be cases, and doubtless have been many, where the sad and gloomy countenance of the physician has sealed the fatal issue of his patient.—Cases in which a smile upon his countenance might have enkindled hope, and confidence, and encouragement, and have been the cause of triumphant success.

If such be the results of mental emotion, that life and death are their issues, how unspeakably important that the physician, in the treatment of his patients, should feel their efficiency, control their tendencies, and scrupulously guard his communications and deportment; for upon them frequently depend, not only his success and reputation, as a medical man, but the happiness of friends—the health, safety and life of his patients.

Part Fourth.

MEDICAL INTELLIGENCE.

FOREIGN.

- 1—*The Modern Chemical Philosophy.* By T. S. HUNT, Chemist to the Geological Survey of Canada.

The sagacious Locke has well observed, that "the right use and conduct of the understanding, whose business is purely truth and nothing else, is that the mind should be kept in a state of perfect indifferency, not inclining to either side any further than evidence settles it by knowledge, or the over-balance of probability gives it the turn of assent and belief." The soundness of this principle will be disputed by no one, and yet in all our searching after truth, whether in the records of the past or the experience of the present, we are too apt to regulate the conduct of our understanding rather by the bias of some previous prejudices. This is indeed so natural to man, that it is the highest triumph of the philosopher to enable us to discard all pre-conceived notions, and weigh calmly and disinterestedly the various facts presented to us.

We have been led to these remarks by a consideration of the existing system of Chemical Philosophy, in which, perhaps, more than any other science we see illustrated this tendency. It stands before the world a loose, disjointed, and unwieldy fabric, built upon an unsound foundation, whose weakness is each day made more apparent. At the time when it was framed and made the basis of a new system of nomenclature by the French philosophers, it was, although certainly not based upon a rigid induction, one that accorded well with then known facts, and seemed to its originators sound. The science of Chemistry has, however, made great and rapidly increasing progress since that time, and an immense number of facts have been developed which seemed incompatible with the system of philosophy that had been adopted as the infallible standard and the test to which every discovery must be brought. And here comes in the perversion of the only true principles of judgment; without any shadow of probability, except so far as appeared from their adaptation to the demands of the received theory, an immense number of hypotheses were advanced and hypothetical bodies described, which not only were unknown, but often from the very nature of things impossible. Hence has been not unaptly said, "the chemistry of the day consists in the description of unknown combinations," not to say impossible ones. This corrupt and false system has been supported and perpetuated by the nomenclature of the science, which inculcated the idea of dualism in all combinations, and was indeed based upon the hypothesis that all compounds are binary in their structure.

The falsity of this idea, and the inadequacy of the present system to the wants of the science have been felt for several years, and a gradual preparation for a new and more natural philosophy has been making in France among some of the *elite* of its philosophers. It is only within three or four years, however, that Laurent and Gerhardt have dared, in their published memoirs, to renounce their adhesion to the popular creed, and boldly to take the lead in defence of a new and purely inductive system. This has been done through the pages of the *Annales de Chimie et de Physique*, and especially by M. Gerhardt's *Précis de Chémié Organique*, a book which by the elegance of its style, the clearness and force of its reasoning, and its admirable systematization, will reflect lasting honor upon its illustrious author, and place the name of Gerhardt with those of Priestley, Lavoisier, and Davy. Like all other great advances in truth, this has met with most determined opposition from the Swedish and German Schools, and indeed from the chemists of Europe generally; such innovations upon a philosophy and nomenclature were not to be at once admitted, and the prejudices of more than half a century were to be vanquished, but it is slowly progressing, and already numbers among its converts not a few names illustrious in science.

In tracing briefly the history of this new order of things, M. Gerhardt mentions M. Daudriment as having in a work published in 1838, attempted to show some of the fallacies of the popular system, and this he regards as the first publication upon the subject. Chance, however, has thrown in my way a work by Mr. J. J. Griffin, of Glasgow, entitled *Chemical Recreations*,* in which he speaks of some of the prevalent notions under the name of the "Romance of Chemistry," and endeavours at the same time to frame a new system of Chemical Philosophy and a new nomenclature. He has seen the fallacies in reasoning, and the imperfect analogies by which the popular system is supported, and has at the same time anticipated the views of Gerhardt in many important particulars. From the unpretending character of the book, and his curious, not very attractive manner of writing, it is probable that Mr. Griffin's ideas have scarcely attracted the notice of any scientific man, and the founders of the French School are doubtless profoundly ignorant of his claims as being the originator of many ideas which justly entitle him to great credit as an original and philosophical investigator. Indeed, it would seem as if Mr. Griffin himself had no proper conception of the importance of his own work, for I am not aware that he has pursued his peculiar ideas any farther, or applied them to the investigation of any of the more recent discoveries in the science; at least he has not, I believe, published anything further. As an act of justice to the talented author, I shall notice some of the peculiarities of his views, and compare them at the same time, with the popular ideas of the day, and those of the new French School.

The fundamental error in the School of Lavoisier and the anti-phlogistic nomenclature is, that it attempts to define the ultimate constitution of bodies, and show the precise manner in which the different elements are grouped. The idea of a binary arrangement is also carried through the whole system. Thus potassium combines with oxygen to form potash, zinc with oxygen to form ox'd of zinc, and sulphur with oxygen to form the so-called anhydrous sulphuric acid. These are facts capable of demonstration, and it would be well if theory would stop here, but instead of this we are told that sulphuric acid and potash unite to form sulphate of potash, the same acid and oxyd of zinc to form sulphate of zinc: and, finally, the two sulphates themselves combine to form a double sulphate of potash and ox'd of zinc. The formula according to this will then be $(\text{KO} + \text{SO}_3) + (\text{ZnO} + \text{SO}_3)$; all these how-

* *Chemical Recreations*, &c., to which is added the *Romance of Chemistry*; an inquiry into the fallacies of the prevailing theories of Chemistry, with a new theory and nomenclature, by John Joseph Griffin. Seventh Edition, Glasgow, 1834.

ever, are assertions which want truth for their basis; analysis shows that this salt contains $S_2 O_3 K Zn$, and this formula represents its composition. But say the advocates of the dualistic theory, "we form these sulphates by directly uniting $S O_3$ with $K O$," and $Zn O$, and from this infer that their ultimate composition is what we have represented by the formulas. It is true sulphate of zinc can be made by dissolving zinc or its oxyd in dilute sulphuric acid, but here water is decomposed in the one case and displaced in the other. This, although precisely their mode of reasoning, is begging the point at issue to prove their proposition, for there is no more evidence that sulphuric acid contains $S O_3 + H O$, than the sulphate of zinc contains $SO_3 + Zn O$. All that we are warranted in concluding from analysis is, that sulphuric acid contains $SO_4 H$, while it is equally obvious that sulphate of zinc contains $S O_4 Zn$. We might with as much reason assert that the true composition of the salt is $ZnS + O_4$, for the sulphuret of zinc by heat will combine directly with four equivalents of oxygen and form sulphate of zinc.

As a step towards a reform in the science, the new French School have rejected entirely the idea of dualism, and the attempt to represent the constitution of bodies by means of the so-called rational formulas; and seek to express by them only the actual proportions of each element in what is regarded as the equivalent of the compound; at the same time ascertaining which of these elements are essential to the conservation of the type, and which are capable of being replaced by other elements.

As an example, the sulphate of potash is found to contain $SO_4 K$, and that of sulphuric acid, which is designated as the normal or typical sulphate, $SO_4 H$; but as we find sulphates which contain two bases, we conclude that the real equivalent is double this, and hence write the formula of sulphuric acid $S_2 H_2 O_8$. In this compound one or both of the equivalents of hydrogen are capable of being replaced by any metal, giving rise to a class of salts which will be either neutral or acid, as the whole or a portion of the hydrogen is replaced. The series is then as follows:—

Sulphuric acid,	$S_2 (H_2) O_8$
Acid Sulphate of potash,	$S_2 (H K) O_8$
Neutral sulphate of do.	$S_2 (K_2) O_8$
Sulphate of potash and zinc,	$S_2 (K Zn) O_8$

The same view is extended to the hydrated alkalies and alkaline earths. We are generally told that hydrate of soda contains $NaO + HO$ and hydrate of lime $Ca + HO$, and this, too, notwithstanding it is impossible to expel any water by the strongest heat of our furnaces. This fact should certainly lead us to infer that the hydrogen in the compound is not really united to a portion of the oxygen forming water, but the contrary of this has been gravely asserted, and admitted, too, without the shadow of a proof; well may the existence of such baseless fancies as these, lead Mr. Griffin to describe the present theory as a part of the "Romance of Chemistry."

The constitution of the hydrated alkalies as deduced from analysis is really $(KH)O_2$, or adopting the double equivalent of hydrogen, and dividing the equivalents of the metals to correspond to it, $(KH) O$, water being represented by $H_2 O$. We can now understand why we cannot form oxyd of potassium, $K_2 O$, by the action of potassium upon water; one half of the hydrogen is replaced, and then the affinities are satisfied, or a stable compound is formed; but if hydrate of potash really contains water, it is difficult to understand why the water does not act upon the potassium. Many hydrated oxides by heat lose the elements of water, it is true, and are converted into simple oxides; and this is brought forward by the old school as an argument in favour of their system, but in truth, $2(CaH)O$ become $Ca_2 O + H_2 O$.

Such are the views at present adopted by the French school; let us compare Mr. Griffin, and we shall see a complete correspondence between the two. He says—

" Shall I state in one word wherein the rottenness of the prevailing theory lieth? It is in the *missapplication of the doctrine of proximate constitution*, a doctrine . . . of by far too unsteady a nature to be the lode-star of a chemical system, or a chemical nomenclature. Who knows the ultimate constitution of alum? Who is ignorant of its proximate constitution? With the latter we are on safe ground; with the former we slide at every step. p. 194.

Again—

The difference between an *acid*, viz., a *hydrated acid*, and a *salt*, is this:—the former contains one combining proportion or 0.25 parts of *hydrogen* while the latter *instead* of this hydrogen, contains exactly one combining proportion of a metal; *all other constituents*, both of the acid and salt *remaining the same*.

EXAMPLES.

4 Sulphur 8 Oxygen 0.25 Hydrogen=Sulphuric Acid.

4 Sulphur 8 Oxygen 10.00 Potassium=Sulphate of Potash.

Numberless instances of this sort could be given, but these suffice to confirm the above proposition; viz., that the hydrated acids are simply salts of hydrogen." . .

" The difference between a *hydrate of a Protoxide of a metal* (such as slacked lime) and an *anhydrous protoxide of a metal* (such as quick lime) is this:— the former contains a combining proportion of hydrogen, where the latter contains a combining proportion of a metal. The difference between a hydrated protoxide and an anhydrous protoxide is therefore the same as the difference between an acid and a salt. In both cases one of the compounds contained a combining proportion of hydrogen, which is replaced in the other by a combining proportion of a metal. As an example.—

5 Calcium 4 Oxygen, 0.25 Hydrogen=Hydrate of Lime.

10 Calcium 4 Oxygen, 0.00 Hydrogen=Anhydrous Lime,

These hydrates of protoxide contain no water, for they contain but one proportion of hydrogen, whereas our water contains two proportions." pp. 93, 94.

In accordance with these ideas Mr. Griffin writes the formula of carbonate of potash KCO_3 , of oxalic acid HCO_2 , oxalate of potash KCO_2 ; the sulphate of potash KSO_2 , sulphuric acid being HSO_2 , while the bisulphate is $\text{KSO}_2 + \text{HSO}_2$, or KHS_2O_4 . He does not appear to have considered that the same reason which led him to write the formula of the protoxyd of potassium K_2O , and the hydrate K_2O , should make the formula of sulphuric acid $\text{H}_2\text{S}_2\text{O}_4$. With the oxalic and carbonic acids it is truly bibasic, like water which is really the normal type of a great class of *oxyds*, as they are called, but which are as truly *salts* as the sulphates or nitrates.

In explanation of our author's formulas, it must be observed that he divides the equivalents of the metals and hydrogen by four, and those of oxygen, sulphur, and carbon by two. As this is merely a question of ratios, it is a matter of no importance so long as the necessary relations are observed. The modern French chemists generally divide the Berzelian equivalents of the metals by two.

The solution of iron in dilute sulphuric acid has been a source of great perplexity to chemical philosophers, and in reviewing all the idle works that have been wasted in its discussion, we have a striking instance of the dangerous consequences of pre-conceived theories in the investigation of a new class of re-actions. Sulphuric acid was considered as a compound of water with the so-called anhydrous acid SO_3 , and this last was conceived to unite directly with oxyd of iron to form the proto-sulphate. The oxyde must be formed by the decomposition of the water, which at the same time evolved the hydrogen gas. But iron was not found to be capable of decomposing water alone, and in order to explain this, a new power was imagined, which was named the *force of presence* or *disposing affinity*; according to this notion the presence of the acid disposed the iron to decompose the water, or, as might be inferred from the explanations of some authors, it was the affinity of sulphuric acid for an oxyd not yet formed, which caused its productions.

It is only within a very few years that more rational views of the constitution of acids have enabled chemists to see that their theories are quite unnecessary, and that the whole process is one of the most simple imaginable. The light in which Mr. Griffin had been led to regard the salts, had long before this, prepared him for a proper understanding of this reaction, and accordingly we find him stating that—

“When salts are produced by the combination of the above acids, [sulphuric, phosphoric, hydrochloric, &c.] with electro-positive metals, one atom of hydrogen always gives place to one atom of metal. Thus $\text{HCl} + \text{K}$ produces $\text{KCl} + \text{H}$, and $\text{H}_2\text{SO}_4 + \text{K}$ produces $\text{K}_2\text{SO}_4 + \text{H}$.

The statement that *water is decomposed* when iron is dissolved in diluted oil of vitriol, is a mere assumption without a shadow of evidence in support of it, and made without necessity or utility.” (pp. 259, 260).

The classification of the natural silicates, has long been a subject of perplexity, and the formulas calculated to explain their composition have presented a most delightful confusion. It is only within the last year that M. Laurent has succeeded in unravelling their perplexity, and has shown that they are in reality referrible to a few very simple types. It is curious to observe how very near Mr. Griffin had arrived to the solution of this question. He assumes a hydrate of silica SiHO_2 (anhydrous silica being in his notation SiO) in which any metal can replace the hydrogen. The other silicates he regards as compounds of this form with anhydrous silica, or as being respectively Si_2MO_3 and $\text{Si}_4\text{M}_2\text{O}_2$. These, he observes, will be found to include the principal natural silicates, although there are probably some that are referable to other types; and he very justly adds that, “if the views given in this book respecting the composition of the silicates and other salts, were applied to mineralogy, they would have a great effect upon the systematic arrangements of that science.” see pp. 109–111.

But we have already extended this notice to a greater length than was first intended, and must now pass over in silence many things well worthy of note. Our object is to do that for Mr. Griffin which justice to his original and ingenious work demands. To him belongs the honour of being the first originator of the new system of chemical philosophy, which, grounded as it is upon strict induction, must remain in its fundamental principles forever the same.

To M. M. Gerhardt and Laurent belongs, however, the merit of originating the same ideas quite independently of Mr. Griffin, and the still higher merit of perseveringly defending them, and extending them to the whole science of chemistry; in which they have already gained a reputation that will endure as long as the science itself.

Had our author fully realized the importance of the ideas which he then suggested, and proceeded in a careful and philosophical manner to apply them to the investigation of existing theories, he might have achieved for himself a world-wide reputation, and stood as the Priestley of the day. As it is, however, the present work is one which will forever connect him with the history of the science.

Montreal, April 20th, 1848.

AMERICAN MEDICAL INTELLIGENCE.

1.—*Remedy for Dysmenorrhœa.*

MESSRS. EDITORS:—I propose to offer to the Profession, through your Journal, a most excellent remedy for the various forms of *dysmenorrhœa*, and particularly applicable to *neuralgic or irritable dysmenorrhœa*. In doing so, it will not be expected that I will give a lengthy description of the pathology, nature and treatment of the disease, inasmuch as my views are already known upon this subject. Some few remarks in regard to the use of the speculum, and the impossibility of using it in the latter form of the disease, will embrace what I have to offer at present.

Diseases of the womb have become so common now-a-days, that there are but few practitioners who are not consulted for some one of the various forms to which the uterus is subject. By some these affections are not properly understood; their treatment is therefore not satisfactory to themselves or the patient. This has in some degree arisen from the decided opposition to the use of the speculum in exploring the vagina, and detecting the various diseases to which females are subject. That an examination is not more often submitted to by ladies is owing, in a very great degree, to the amount of prejudice which many of our Profession manifest when the speculum is mentioned by one who is accustomed to its use, and who has had the experience of treating many cases of uterine disease. We can only attribute this decided opposition to the fact, that we imbibe prejudices to all innovations upon established customs, and without investigating the subject for ourselves. We rush blindly, with facts staring us in the face, and endeavor to throw our weight and influence in the scale of opposition, without, perhaps, giving the subject a moment's reflection. Would it not be better, where we see a new idea presented to our notice, especially if it comes from a respectable source, to give it a hearing before we condemn? When such men as Ashwell, Duparque and Lisfranc have shed so much light upon this hitherto obscure part of therapeutics, we are in a degree surprised to see the opposition manifested by enlightened members of our Profession, who cannot be ignorant of the value of such examination, and the advantages to be derived from a well directed course of treatment locally to the uterus. To remove this prejudice, it is necessary for those who are devoting their time and attention to this particular class of diseases, to present to the profession facts and evidence, stating the efficacy of this mode of treatment. We know that ladies have a natural and laudable reluctance to an exposure of their persons, and we are glad to see it; but when stern necessity drives them to submit, it requires but little moral courage of the physician to propose, and, by a proper explanation of conducting the examination, to secure at once the consent of the lady. With all the objections I have heard urged by ladies against the use of the speculum, none have been so great but a few moments' conversation has convinced them of the necessity of submitting, and many have regretted that false delicacy should have deprived them of

the great benefit of this treatment, when so much suffering has attended the delay. It is known that I have devoted much time to diseases of the womb, and, as far as my experience and observation extend, I hazard nothing in saying that we cannot dispense with the speculum, if we wish to investigate them properly. It has, however, been my misfortune, after obtaining the consent of the lady, to find that the introduction of the instrument gave such intense pain, spasms of the womb, and this succeeded by active convulsions of the whole muscular system, especially in *neuralgic* or *irritable dysmenorrhœa*, that I have been compelled to abandon its use. The congestive, plethoric or inflammatory form is easily managed by a well directed course of treatment. It is only in the *irritable form* that we encounter so many difficulties, and which heretofore has been considered incurable. That others have met with such cases, and that we may meet with them again, is very certain; to relieve such a case will be considered a great desideratum. The object of this paper is to throw some light upon this subject, and to propose a remedy, that never fails to relieve pain, and if continued, will establish the *period*, and make it regular, free and healthy. The frequent trials I have made with it, in all the forms of dysmenorrhœa, and particularly in the neuralgic or irritable, has convinced me that it acts specifically upon the uterus, in relieving pain and spasms of the womb, increasing the flow, dilating the canal, and permitting coagula or membranes to pass, without the least inconvenience to the lady. These pills have been tested fully in a case that I have now under treatment, of neuralgic or irritable dysmenorrhœa, which has been accompanied with convulsions. The speculum having been abandoned for some weeks, induced me to give them a thorough trial; that they have succeeded most admirably in arresting all pain and establishing the discharges to a free and healthy quantity, will not admit of a doubt by the physicians who have seen the case from time to time, while under treatment. The ulceration in the canal and cavity yet remain to be healed; to relieve which I intend administering the chloroform, and while under its influence, to introduce the speculum and pass the caustic along the canal to the cavity. The chloroform has been repeatedly given in this case to relieve the convulsions; its effect has been pleasant, and at the same time speedily quieting the system.

A full detail of this case, with several others, will form the subject of an essay for the November number of your Journal.

The prescription is as follows:—

R Gum Camphor—3 ii ss.

Ext. Belladonna—3 ss.

Sulph. Quinine—3 ss.—m.

Divide into 80 pills.

These pills are to be taken only at each period, and at the commencement of pain. Dose—two pills every hour until the pain ceases. In extreme cases, they may be given during the intervals of the periods, say one pill two or three times a day. No bad effect will result from their continued use for months.

Very respectfully,

H. J. HOLMES.

Spring Ridge, Miss., June 12, 1848.

[We have received a communication from Professor Dunglison in relation to the critique which appeared in our May number. The letter is a private one, but contains a defence of himself, which we feel is due to him to give to the profession.—EDRS.]

Philadelphia, June 2, 1848.

DEAR SIR :—In the Preface to that work* you will find I remarked, that "the improvements and modifications incessantly taking place in the departments of pathology and therapeutics render it advisable from time to time to incorporate them, so as to furnish those to whom the different general treatises, monographs and periodicals are not accessible, with the means of appreciating their existing condition."

In the fulfilment of this object, I laid every essay of value under contribution, and, so far as my ability permitted, endeavored to render the work a consistent whole, to which the reader might refer with satisfaction to learn the existing condition of the branch of science on which it treated. Where *facts* or *histories of disease* were concerned, I regarded them as common property, unless when the descriptions—which was rarely the case—were entirely original; and in all cases I have striven to do justice to the labors of observers, who have in any way enlarged, or seemed to me to have enlarged, the boundaries of science. Especially have I considered this important where *opinions* were stated, which belonged apparently or really to the individual. Accordingly, in the first edition of the "Practice," I placed in a parenthesis, after a description that could, in any manner, be esteemed to belong to any one, his name; and, in the part of the work "*on Diseases of the Eye*," which has been selected by the reviewer for especial comment, this was done repeatedly, and in cases where the name of Dr. Taylor ought scarcely, perhaps, to have been added alone.

When the second edition of the work was called for, the plan of parenthetical insertions was objected to by my publishers, and it was regarded by the reader as destroying the continuity of the sentences. It was, therefore, abandoned; but still, as you will find, the "*Library of Medicine*" was especially referred to, and the name of Dr. Taylor repeatedly introduced, wherever I deemed it advisable, and so often that the idea of concealment on my part must be out of all question.

In the number of the Journal to which I have referred, passages are taken from my "Practice," and from Dr. Taylor's articles in the "*Library of Medicine*," and sentences detached from the rest, are placed in such juxtaposition and so animadverted on, as to lead to the inference, that extensive passages have been adopted without acknowledgement; and a special reference is made to the diagnosis of Iritis, as having been taken without acknowledgement from Dr. Taylor.

The main object I have in view, in this communication, is to show you that the passages in question are not original with Dr. Taylor, and have been regarded by him so completely as common property, that although they and several others have been adopted by him with little or no verbal variation from his predecessors and contemporaries, he has

* The Practice of Medicine.

not considered it necessary to refer to authorities. If you will take the trouble to examine the chapter on Iritis in Mackenzie, and certain paragraphs on the same subject in Lawrence, you will find them almost identical with those in the "Library of Medicine."

Many of the paragraphs on the "Purulent Ophthalmia of Infants" are scarcely changed from those in Lawrence, whilst the article on "Corneitis" differs hardly at all from Mackenzie. All these works, with the best of France, Germany, &c., were before me; and it was difficult, if not impossible, to say to whom to give the priority for the analogous and identical passages; but, *certainly*, the "Library of Medicine" was not entitled to it.

I would draw your attention, also, to the divisions of purulent ophthalmia, by Dr. Taylor, cited by the reviewer, which are taken, almost verbatim, from Lawrence, to whom, indeed, he refers; and you will find, that a similar division is adopted, on like principles, by Dr. Watson and most modern writers. I do not, indeed, know of a single systematic work that has appeared of late, which does not contain passages against which the same objections might be made as against mine. Descriptions of natural history, and, as a part, of the natural history of disease, have never been regarded as belonging to individuals; nor, so far as I know, has it ever been esteemed improper to use them. It is otherwise as regards thoughts and opinions.

3.—*Medical Board of Examiners.*

In the first number of the *New Orleans Medical Journal* we published all the laws, &c., relating to the practice of Physic &c., in the state of Louisiana. Those laws have undergone no modification since that date; the requisites to become a Licentiate of the Board remain the same, and the penalties for practising physic &c., without the sanction of the Board have not been abated one jot or tittle. Let those then, who, as good citizens, are willing to comply with the law, and who have heretofore neglected to do so, present themselves either to the Eastern or Western Board, for authority to practise their profession throughout the State. We observe with much satisfaction, that the majority of medical gentlemen, recently removing to this state, show every disposition to comply with the law, by presenting themselves before the Board for Licenses.

As a general rule, those only who feel and know that they are incompetent to practice the profession of medicine, refuse to come forward and pass the ordeal of the Board.

Of this number, many are to be found, both in the country and city, and we regard it the duty, both professional and moral, of such as have complied with the requirements of the law in this respect, to urge those who have not, to obey the authority of the State, as good subjects, that others, seeing them comply, may be induced to imitate their example.

Indeed, Art. 14, of the "*laws relative to the Medical Board of Louisiana*," declares that "this Board will, every year, at its first meeting in January, appoint a committee of physicians, in each parish of its district, (and recommend the '*Western Board*' to do the same in its

district) whose duty shall be to co-operate with this Board in enforcing compliance in their respective parishes with the requisites of the law."

Again, Art, 15. Be it "*Resolved*, that in the opinion of this Board, the term 'Practice of Physic and Profession of Apothecary' in the several acts and regulations for its exercises, by the constituted authorities of this State, properly embraces every branch of the Profession, as oculists, venders of patent medicines, &c., it is the sacred duty of this Board to provide that the public within its jurisdiction is not imposed upon by those who have not the qualifications required by law."

We need scarcely say, that not one of the above resolutions, to our knowledge, has been carried into effect, the consequence of which is that the community is imposed upon and drugged by venders of patent medicines, to an extent almost incredible.

For ourselves, as we advocate the largest liberty for the greatest number, we can see no reason why a man, who is allowed to vote, should not likewise be permitted to swallow a dose of patent medicine, as well as an oyster; though the first might poison, and the second fatten him!

We would say of the people, as Horace of the Poets:—

"Deus immortalis haberi
Dum cupit Empedocles, ardentem
Frigidus Ætnam insiluit. Sit
Jus, lice-atque perire poëtis.
Invitum qui servat—idem facit
Occidenti. Nec semel hoc fecit; nec
Si retractus erit, jam fiet homo,
Et ponet famosæ mortis amorem."

As the law bears hardly upon the proud and educated gentlemen of the Profession, and permits the servile and ignorant impostors to go "unwhipped of justice," we think from its unequal and unjust operation, it should be altogether abolished; and let every man be free to choose whatever occupation he may think best adapted to his talents. The law selects the hangman to execute the *criminal*; but who can question the right of every good citizen to choose his doctor or lawyer, though the former should kill him outright, and the latter rob him and leave him bankrupt?

The following medical gentlemen compose the Board for the *Eastern District* of Louisiana, for 1848, nominated by the Governor and confirmed by the Senate: Isadore Labatut, M.D., *President*. Warren Stone, M.D.; J. H. Lewis, M.D.; A. Hester, M.D.; Charles Turpin, M.D.; Charles Deléry, M.D., and J. U. Landreaux, M.D. *Apothecaries*: P. L. Massey and P. A. Bertrand, *Secretary*.

4.—*Proceedings of the Attakapas Medical Society.*

To the Editors of the New Orleans Medical and Surgical Journal.

GENTLEMEN:—I send you the proceedings of the Attakapas Medical Society, which you may deem worthy of publication in your next number.

The Society was first organized on the 10th November, 1846, and numbered ten members; it now numbers about twenty-five members.

Sitting of the Attakapas Medical Society, Tuesday, May 9th 1848.

The Society met pursuant to adjournment and was called to order by Dr. J. B. Dungan, president.

The following members answered to the roll: Drs. H. J. Sanders, E. K. Fassett, Jas. F. Fontaine, Jérôme Mudd, Joseph Lyman, J. B. Hacker, W. G. Mills, E. Sheil, J. B. Dungan, A. Duperier, G. W. Scranton, R. C. Hilliard, R. H. Day, J. H. Baldridge, J. T. Alexander, E. F. Beauchamp, and J. G. Howard.

From the Committee appointed to investigate the cause, treatment, etc., of the epidemic, which prevailed in the vicinity of Breaux's Bridge, during the past summer and fall, Dr. Duperier read a letter from one of the resident physicians of that place, giving it as his opinion that the disease was yellow fever. The committee not having had time to furnish a report, was, on motion of Dr. J. Mudd, continued until the next meeting of the Society.

From the committee on "Laws to regulate the practice of medicine, and to incorporate the Attakapas Medical Society, Dr. J. Mudd read a report which was filed among the archives of the Society." The same committee was continued.

The following preamble and resolutions were offered by Dr. A. Duperier and unanimously adopted:

Whereas, it is the opinion of the Physicians, comprising the Attakapas Medical Society, that a State Medical Society would tend to create concert of action between the physicians throughout the State, and thereby be conducive to the advancement of Medical Science in the State of Louisiana;—therefore

Resolved, that the Attakapas Medical Society earnestly recommend a State Medical Convention, to be composed of delegates from the different County and Parish Societies in the State, and of the Medical men throughout the State, to convene in the city of New Orleans, on the first Tuesday in March, in the year 1849, for the purpose of organizing a general State Medical Society.

Resolved, That a copy of these resolutions be forwarded to the Editors of the New Orleans Medical and Surgical Journal.

On motion of Dr. Mudd, a committee was appointed to address the members of the Western Medical Board, to ascertain from each one of said Board, whether he will be willing to meet and at what particular time, and whether Opelousas will not be the most convenient and central point—and further that said committee be authorized to take all such steps as may be necessary to procure a reorganization of said Board, and to secure regular action for the future. Drs. E. K. Fassett, W. Mills, and G. W. Scranton, were appointed to compose the committee.

Drs. J. B. Hacker and James T. Smith were appointed to prepare Essays for the next meeting of the Society.

(A true Copy of the proceedings.)
New Iberia, June 12th, 1848.

A. DUPERIER, M.D.
Secretary.

[From the above proceedings of the Attakapas Medical Society, we see it earnestly recommended to hold a *State Medical Convention* in

New Orleans, on the first Tuesday of March, 1849, to be composed of delegates from the several parishes of the State. This suggestion meets with our entire approbation and we shall, if seconded by the profession, strive to bring it about. The resolution speaks well for the zeal and intelligence of the members, comprising the Medical Society of Attakapas.

With pleasure we publish the proceedings of this Society, and should be pleased to receive any interesting papers or communications from the same source.—EDRS.]

NECROLOGY.

DEATH OF DR. JAMES A. MC'PHETERS, OF NATCHEZ.

At a Meeting of the Physicians of Natchez and Concordia Parish called on account of the death of the late Dr. James A. Mc'Pheters, Dr. S. A. Cartwright was called to the chair, and Dr. W. L. Jones appointed Secretary.—The following resolutions were unanimously adopted:—

Resolved, That in regard to the great worth of the deceased, both as a gentleman and physician, and out of respect to the memory of his many virtues, the Members of the Medical Faculty will wear crape on their left arms for thirty days.

Resolved, That a copy of the foregoing resolution be furnished to his disconsolate family, with the sincere condolence of the Physicians of this City, in their late bereavement.

Resolved, That the proceedings of this meeting be published in the Natchez Papers, also in the Concordia Intelligencer, and New Orleans Medical and Surgical Journal.

Signed,

S. A. CARTWRIGHT, *President*.

Natchez, May 25th, 1848.

W. L. JONES, *Secretary*.

NEW ORLEANS, JULY 1, 1848

With this number, we enter upon the *Fifth Volume* of the *New Orleans Medical and Surgical Journal*. Four years' experience have convinced us that the profession in the South and South-West is fully able to sustain a journal of practical medicine; and we avail ourselves of this opportunity to return our grateful acknowledgements to those who have aided us in this new (and now no longer doubtful) enterprise.

We have been anxious to make this a practical Journal; we shall still aim to give it that character, and from the spirit of the communications which have come to hand, we believe the profession will second our efforts. The age demands every thing that is practical; nothing theoretical or hypothetical, save it be to give point to fact, will pass as current coin in any of the professions or departments of life, whether civil or political.

This tendency of the age—this proclivity of the human mind, is destined to revolutionize the scientific, as it has already shaken the political world. Be it accomplished.—Observation, and the careful collection

of facts, followed by a rigid analysis of the same, must supersede the idle and often unfruitful speculations of a superficial philosophy.

Vast accessions are being daily made to our practical knowledge in almost every department of our profession; the physiologist, with his ingenious art, wrings from agonizing and expiring nature, truths that would create an epoch in any other age, but which are now regarded as things of daily occurrence, and excite not the "wonderment of men." The microscopist, with far-seeing eyes, counts the atoms and even the globules of blood, which go to make up the physical system of man. The chemist, with a rod, more potent than that which smote the rock of old, commands the unwilling elements to fraternize, and lo! we have the pain-destroying chloroform!

But enough of this. Of the sanitary condition of the city little need be said; as, during the month of May, as usual at this season of the year, the health of the city was excellent. Throughout this period but little rain fell, and the atmosphere was agreeably dry and balmy. Towards the close of the month, and the beginning of June, rain began to fall almost daily, and continued beyond the middle of the month. With this change in the thermometrical condition of the atmosphere, characterized by the increase of heat, &c., diseases, which anterior to this time, assumed the form of mild Intermittents and Remittents, gradually ran into or wore the livery of malignancy, and became difficult to manage. Instead of simple, open and frank attacks of febrile disease, easily controlled by mild laxatives, followed by the sulphate of quinine, we had to combat pernicious forms of these affections, attended with symptoms of congestion, local determinations, gastric irritability, and in some cases, all the external phenomena of our epidemic fevers.

Even as early as the 10th of June, we saw a case at the Charity Hospital, in the person of a German just arrived from Havre, with all the usual external symptoms, save the black vomit, that characterizes the yellow fever. He was seized with a chill on entering the river, and eight days afterwards was sent to the hospital.

It was at this time we saw him; his skin and eyes presented the usual deep icterode hue, raging delirium, &c. The House-Surgeon assured us that other cases similar to the above, had been recently received into the institution. With those who watch the progress of disease in this city, and especially at the Charity Hospital, during the summer, such cases are considered as fore-shadowing the coming of yellow fever. Time will confirm or refute these predictions.

It is apprehended that the arrival of troops here from the late seat of war, by way Vera Cruz and Tampico, will introduce the yellow fever into our city. This result is confidently predicted by those who advocate the importability of the disease; others, however, who deny the infectiousness of the yellow fever, assert that nothing is to be feared from the debarkation of troops at this point, even in the midst of the summer months. Individually, we should regard the arrival of invalid soldiers in our midst, from infected points, such as Vera Cruz and Tampico, at a season when our epidemic usually commences, as extremely hazardous both to our citizens and the soldiers themselves. Such a conjunction of circumstances would be likely to produce several *foci* of infection, from which the epidemic might spread throughout the city. Let the author-

ities guard against such a contingency. The Board of Health, with commendable activity, has already taken the initiative in this matter.

We are assured by the medical gentlemen connected with the army, that no time will be lost in transporting the sick soldiers to the different hospitals selected for their accommodation out of the city, as fast as they arrive.

Two or three cases of yellow fever have already been imported from Vera Cruz ; but in no instance have we learned that the disease extended to any other person. So much for the contagiousness of the disease. During the last two or three weeks, cases of small pox have been very numerous throughout the city ; hence considerable alarm has existed on the subject. For the week ending the 4th of June five deaths from this disease were reported ; and for the same period, ending the 11th of the same month, the deaths from small-pox were ten. If the people neglect in time to avail themselves of the only prophylactic, vaccination—they must expect this loathsome disease to spread among them.

Since the commencement of warm weather, a few cases of *coup de soleil* have occurred ; and, as the summer advances and the heat increases, we expect the mortality from this cause to become greater.

If, as some political philosophers assert, population constitutes the wealth of a nation, it then becomes the sacred duty of those in authority to use every precaution—to adopt the most efficient means to protect the public against the ravages of fatal epidemics.

Impressed, no doubt, with these considerations, Alderman Smith, of the Second Municipality, with his well-known promptitude and philanthropy, has already brought before the Council a proposition to have moored along the Mississippi a number of baths, for the accommodation of the public at large. This is an important measure, and will no doubt add materially to the comfort, cleanliness and health of the laboring and poorer class of our population. We hope some benevolent Aldermen of the First and Third Municipalities will emulate the example of Mr. Smith in this truly charitable enterprise.

The present Board of Health of the City and Parish of Orleans, exercising the powers granted it by the act of the last Legislature, has appointed two "Health Wardens" for each of the three Municipalities, "whose duty it is to visit from time to time, and inspect the condition of the houses and lots of their several wards, and should they discover therein any nuisance, likely to prove injurious to the public health, it is made the duty of any Health Warden of the Ward, (or district,) in which the nuisance may be found, to order its removal ; and if the removal be not made within the time specified, it is made the duty of said Health Warden to report the same to any two members of the Board of Health, and if said members approve the order made by the Health Warden, they shall direct the immediate removal of said nuisance, at the expense of the tenant or owner of the property. If there be no tenant (of the house or lot) in which the nuisance was found, the Municipal Council in which the nuisance was found, shall promptly advance the money for the removal of the nuisance, and shall institute suit for the recovery of the same, before any court of competent jurisdiction."

Landlords, boarding-house keepers, tenants, &c., should aid the Health Wardens in the discharge of their duties; as upon this the health and comfort of our citizens may in a great measure depend. The appointment of these Health officers is a new feature in our police and sanitary regulations; and if the duties are faithfully discharged, salutary results may be anticipated.

Another important ordinance lately passed by the Board of Health is, to compel the sextons of the different cemeteries to deliver their reports to the Board, under oath. This will force them to keep accurate accounts of the interments made in the several burying grounds in and about the city; besides they are prohibited from receiving and interring bodies unaccompanied by a proper certificate, duly made out and signed by a licensed physician, or other persons qualified by law to do the same. Such regulations are imperative in a city where death might be brought about by unlawful means, and the body interred by the certificate of the person or persons who perpetrated the deed.

From the cover of the Journal, it will be seen that our colleagues Drs. Carpenter and Fenner have withdrawn from the work. The former has retired into the country for the summer, the latter still remains among us, and has given us two interesting papers for this number. The connection was dissolved in the same friendly manner that it was originally formed, with every assurance of a cordial support from the parties retiring.

A. H.

HOSPITAL REPORTS.

Continuation of Dr. Brickell's Hospital Reports.

MESSRS. EDITORS:—I take pleasure in continuing my "Hospital Reports." No. 2, Secondary Syphilis, was discharged a few days after my last note, on the case, apparently *well*.—I proceed with notes on cases of Inter. fever, wherein large doses of Quinine are administered in combination with Opium.

My object is, if possible, to ascertain the *best* method of administering this valuable remedy; its true nature as a Medical agent; and its "modus operandi"—and for this purpose, I propose instituting a series of experiments, to be conducted with all possible accuracy, and to be made public through the medium of your journal—my deductions from the same to be made hereafter.

Since the 20th ult., the character of disease has become almost completely changed in my wards.—Previous to that date, Diarrhœa and Dysentery prevailed, together with Intermittent Fever and Pulmonary affections; now *Remittent* fever has the ascendancy, and each day appears to add materially to the severity of the disease.—There are but two cases of Typhus fever in my wards, and these are emigrants who have recently arrived from Ireland.

B—.

Large doses of Quinine in Inter. Fever.

CASE 4.

"May 3rd, 1848."—John Wicher, native of Germany—æt, 27 years.

has resided in this country three months, trunk-manufacturer; quite robust, entered ward 13, Charity Hospital. Was taken sick with Inter. fever (tertian type) on the 25th ult.—paroxysm recurred regularly at 11 A. M., until the 30th ult., when it recurred at 7 A. M., and has continued thus up to the present time.—Expects paroxysm to-morrow morning.

“Treat.”—Ol. Ricin. f. $\frac{3}{4}$ i—to be taken at once.

Also $\mathcal{B}$. Quin. Sulph. grs. xxx.

Pulv. Opii. grs. ij.—to be taken at 5 A. M. to-morrow.

Good diet.

“4th, 9 A. M.”—No recurrence of paroxysm this morning—in profuse perspiration—skin cool—very slight ringing in the ears.

“Treat.”—Port wine and good diet to day—Quin. Sulph. grs. xv.

Pulv. Opii. gr. i; To be taken at 5 A. M. on the 6th.

“6th., 9 A. M.”—Missed his paroxysm again—is in profuse perspiration—says he feels quite well.

“7th, 9 A. M.”—Discharged him.

CASE 5.

“May 18th, 1848.”—J. Lehmann, native of Germany—æ. 17 yrs., quite robust, has resided in this country two years, entered ward 13, Charity Hospital, yesterday afternoon. Has been sick four days with Inter. fever—quotidian type—paroxysm recurs regularly at 4 P. M.—took purgative after entering ward yesterday—operated freely.

“Treat.”—Quin. Sulph. grs. xxx.

Pulv. Opii. grs. ij.—To be taken at 2½ P. M. Low diet.

“17th, 9 A. M.”—Missed paroxysm yesterday—perspired very freely after taking medicine—is quite cool this morning, and says he feels much better—appetite improving—tongue coated—bowels in good condition.

“Treat.”—Quin. Sulph. grs. xv.

Pulv. Opii. grs. i.—Take at 2½ P. M. Good diet.

“18th, 9 A. M.”—No recurrence of paroxysm—looks cheerful this morning—appetite good.

“Treat.”—Quin. Sulph. grs. x.

Pulv. Opii. gr. ss.—Take at 2½ P. M.

Port Wine and full diet.

“19th.”—Discharged him.

CASE 6.

“May 17th, 1848.”—G. A. Welz, native of Germany—æ. 53 yrs., has resided in this country, fifteen months, mattress-maker, entered ward 11, Charity Hospital, yesterday afternoon—Has had Inter. fever for ten days past—quotidian type—at first the paroxysm recurred at 1 P. M.; after three or four days at 12 M.; and now at 10 A. M.—patient is quite prostrate—took dose of oil after entering the ward—operated quite freely—appetite not very good—tongue coated.

“Treat.”—Quin. Sulph. grs. xxx.

Pulv. Opii. grs. ij.—Take at 9 A. M. Good diet.

“18th, 9 A. M.”—Paroxysm recurred soon after I left the ward—

nurse administered the medicine, however, during the cold stage—patient perspired much more profusely than heretofore—thinks the paroxysm was very much mitigated—I find him in the cold stage of a paroxysm this morning—each recurrence seems to be earlier.

“Treat.”—Warm drinks etc. for to day—Quin. Sulph. grs. xxx.
Pulv. Opii. grs. ij.—To be taken at 6 A. M. to-morrow.
Half-diet.

“19th, A. M.”—No paroxysm this morning—has perspired very freely—feels much better.

“Treat.”—Port wine and good diet to day—Quin. Sulph. grs. xv.
Pulv. Opii. gr. i.—To be taken as before.

“20th, 9 A. M.”—Missed paroxysm—says he feels quite well—looks cheerful.

“Treat.”—Quin. Sulph. grs. x.
Pulv. Opii. gr. ss.—Take as before. Full diet.

“22nd,”—Discharged him.

CASE 7.

“May 27th, 1848.”—Thos. Seagrave, native of Ireland—æt. 26 yrs., has resided in this country one month, tanner, entered ward 13, Charity Hospital, yesterday afternoon—Has been sick since the 25th inst., with Inter. fever—quotidian type—is quite pale—tongue coated heavily—complete anorexia—thirst—skin cool and moist.

“Treat.”—Ol. Ricin. f. $\frac{3}{4}$ i.—take at once.
Also, Quin. Sulph. grs. xxx.
Pulv. Opii. grs. ij. Take at 5 A. M. to-morrow.
Lemonade and low diet.

“28th, 9 A. M.”—No recurrence of paroxysm this morning—is in profuse perspiration—says he feels much better—no cephalalgia—appetite improving.

“Treat.”—Quin. Sulph. grs. xv
Pulv. Opii. gr. i.—Take as before. Half diet.

“29th, 9 A. M.”—Improving rapidly—says he feels quite well.

“Treat.”—Quin. Sulph. grs. x.
Pulv. Opii. gr. ss.—Take as before. Good diet.

“30th,”—Discharged him.

CASE 8.

“May 28th, 1848.”—J. Maguire, native of Ireland—æt. 17 yrs., has resided in this country eighteen months, entered ward 13, Charity Hospital, yesterday.—Has been sick two weeks with Inter. fever—quotidian type—tongue is heavily coated—anorexia—took dose of oil yesterday.

“Treat.”—Quin. Sulph. grs. xxx.
Pulv. Opii. grs. ij.—Take at 12 M. just two hours before expected paroxysm. Low diet.

“29, 9 A. M.”—No paroxysm yesterday—says he feels much better—appetite improving.

"Treat."—Quin. Sulph. grs. xv.

Pulv. Opii. gr. i.—Take as before. Half diet.

"30th, 9 A. M."—Improving rapidly.

"Treat."—Port wine and good diet.

"31st,"—Discharged him.

CASE 9.

"May 30th, 1848."—H. Black, native of Germany—æt. 34 yrs., has resided in this country two years, carpenter, entered ward 11, Charity Hospital, yesterday.—Has been sick with Inter. fever, eight days—quotidian type—took purgative after entering the ward—tongue coated heavily—thirst—paroxysms recur regularly at 5 P. M.

"Treat."—Quin. Sulph. grs. xxx.

Pulv. Opii. grs. ij.—Take at 3 P. M. Low diet.

"31st, 9 A. M."—No paroxysm yesterday—says he feels much better—appetite improving.

"Treat."—Quin. Sulph. grs. xv.

Pulv. Opii. gr. i.—Take as before. Half diet.

"June 1st, 9 A. M."—Improving rapidly.

"Treat."—Port wine and good diet.

"2nd, 9 A. M."—Is entirely well of Inter. fever—has gonorrhea however, and will remain in the ward until well.

CASE 10.

"June 6th, 1848."—Michael Downs, native of Ireland—æt. 21 yrs., has resided in this country two years, baker, entered ward 13, Charity Hospital, yesterday.—Has been sick with Inter fever one month—tertian type—paroxysm recurs regularly at 7 A. M.—tongue coated—anoxia—quiet anemic.

"Treat."—Quin. Sulph. grs. xxx,

Pulv. Opii. grs. ij.—Take at 5 A. M. to-morrow.

Low diet.

"6th, 9 A. M."—Had slight chill this morning, notwithstanding he took the medicine as directed—has slight fever now—not yet perspiring—no cephalalgia—paroxysm much mitigated.

"Treat."—Spt. Nit. Dulc. f. ʒ ss.—also, Quin. Sulph. grs. xx.

Pulv. Opii. gr. i.—Take at 5 A. M. the 6th. Low diet.

"8th, 9 A. M."—No paroxysm—says he feels well—good appetite.

"Treat."—Porter and good diet.

"9th, 9 A. M."—Improving rapidly—will remain a few days longer.

CHARITY HOSPITAL.

MAY 1848.

Admitted :	Males,	-	-	-	-	-	522	} 655
"	Females,	-	-	-	-	-	133	
Discharged :	Males,	-	-	-	-	-	419	} 538
"	Females,	-	-	-	-	-	119	
Died :	Males,	-	-	-	-	-	57	} 69
"	Females,	-	-	-	-	-	12	

List of interments in the City of New Orleans from the 12th of February to the 28th of May, 1848, being 15 weeks.

Abdomen, contusion of, 1; Abscess, hepatic, 3; Accidental 2; Accouchement, 2; Adynamia, 1; Anasarca, 2; Anemia, 2; Angina maligna, 1; Apoplexy, 23; Apoplexy, Pulmonary, 1; Arthritis, 1; Ascites, 7; Asthma, 1; Atrophia, 1; Bowels, constipation of, 1; do. inflammation of, 18; Brain, compression of, 2; do. congestion of, 24; do. dropsy on, 2; do. inflammation of, 1; do. softening of, 4; Bronchitis, 14; do. chronic, 4; Burn, 3; Cancer, 2; Catarrh, 19; do. pulmonary, 6; Cerebro Spinal Irritation, 1; Cerebritis, 16; do. chronic, 1; Cholera infantum, 11; Cholera Morbus, 2; Chorea, 1; Colic, 1; Colitis, 2; do. chronic, 1; Congestion, 2; Consumption, 167; Contusion, 3; Convulsions, 54; Cramp, 2; Croup, 20; Cynanche Maligna, 3; Cynanche Trachealis, 3; Debility, 36; Del'm. Tremens, 11; Dentition, 22; Disease, chronic 3; do. inflammatory, 5; Diarrhoea, 48; do. chronic, 25; Dropsy, 24; Drowned, 32; Dysentery, 109; do. chronic, 29; Eclampsia, 1; Emphysema, 1; Empyema, 1; Enteritis, 18; do. chronic, 3; Encephalitis, 3; Entero-Colitis, 1; Entero-Meningitis, 1; Epilepsy, 8; Erysipelas, 4; Fever, 6; do. ataxic, 1; do. bilious, 3; do. congestive, 7; do. do. typhus, 1; do. hectic, 3; do. intermittent, 1; do. do. malignant, 2; do. nervous, 7; do. pernicious, 5; do. puerperal, 2; do. remittent, 1; do. scarlet, 74; do. typhoid, 55; do. typhus, 180; do. verminose, 6; Femur, fracture of, 1; Gangrene, 2; Gastritis, 4; do. chronic, 1; Gastro-enteritis, 28; do. do. chronic, 7; Gastro-duodenitis, 1; Gastro-entero-colitis, 1; Gastro-hepatitis, 1; Gout, 1; Hæmatemesis, 1; Head, disease of, 1; do. injury of, 2; Heart, disease of, 8; do. hypertrophy of, 2; Hemiplegia, 2; Hemorrhage, cerebral, 2; Hepatitis, 4; do. chronic, 3; Hernia, strangulated, 1; Hydrocephalus 6; Inflammation, chronic, 2; Intemperance, 3; Intestine, cancer of, 1; do. disease of, 1; Irritation, 1; Kidney, disease of, 1; Laryngitis, 1; Leg, fracture of, 1; do. gangrene of, 1; Lungs, congestion of, 1; do. gangrene of, 1; Liver, cancer of, 1; do. rupture of, 1; Marasmus, 13; Measles, 29; Meningitis, 14; Metro-peritonitis, 2; Mortification, 11; Nephritis, 1; Old age, 17; Paralysis, 6; Parotitis; Parturition, 3; Pleuro-pneumonia, chronic, 1; Peritonitis, 2; do. acute, 1; Pertussis, 6; Phrenitis, 2; Pleurisy, 3; Pneumonia, 45; do. bilious, 1; do. chronic, 1; do. typhoides, 5; Scrofula, 2; Skull, fracture of, 8; Small Pox, 28; Spasms, 1; Sore Throat, 1; Spine, disease of, 1; do. fracture of, 1; Splenitis, 1; Still-born, 71; Stomach, cancer of, 2; Stomach, hemorrhage from, 1; Sudden death, 1; Suffocation, 1; Suicide, 1; Sun stroke, 1; Syphilis, 3; Rheumatism, 3; Tabes Mesenterica, 1; Tetanus, 16; do. Traumatic, 7; Tonsillitis, 2; Trismus Nascentium, 16; Uncertain, 146; Uterus, cancer of, 1; Vomiting, 1; Wound, gunshot, 4; penetrating, 2. Total, 1748; of these, 687 were under 10 years of age; 1278 were white, and 470 colored.

(Extracted from the Reports of the Board of Health.)

A. HESTER,

- Secretary.

ABSTRACT OF A METEOROLOGICAL JOURNAL FOR 1848.

By D. T. LILLIE, AT THE CITY OF NEW ORLEANS.

Latitude, 29 deg. 57 min.; Longitude, 90 deg. 07 min. west of Greenwich.

WEEKLY. — 1848.	THERMOMETER.			BAROMETER.			COURSE OF WIND.	FORCE OF WIND, Ratio 1 to 10.	Rainy Days.	Quan- tity of Rain. — Inches.
	Max.	Min.	Range.	Max.	Min.	Range.				
April - 30	86.0	71.0	15.0	30.20	29.75	0.45	S.E.	2 $\frac{3}{4}$	0	0.000
May - 6	87.5	70.0	17.5	30.25	29.87	0.38	N.E.	3	0	0.000
" - 13	85.0	72.5	12.5	30.32	29.79	0.53	E.	3 $\frac{1}{2}$	0	0.000
" - 20	85.5	72.0	13.5	30.32	29.98	0.34	S.E.	3 $\frac{1}{2}$	3	7.750
" - 27	91.5	74.0	17.5	30.16	29.90	0.26	S.E.	2 $\frac{3}{4}$	2	3.925
June - 3	88.5	73.5	15.0	30.05	29.70	0.35	S.S.W.	3	4	10.445
" - 10	88.5	74.5	14.0	30.15	29.90	0.25	S.E.	3	3	3.090
" - 17	82.5	71.0	11.5	30.18	29.94	0.24	E.S.W.	3 $\frac{1}{2}$	7	15.471
" - 24	90.5	74.0	16.5	30.30	29.95	0.35	S.W.	2 $\frac{3}{4}$	2	1.300

REMARKS.—The Thermometer used for these observations is not attached to the Barometer, but is a self-registering one, and is placed in a fair exposure. Regular hours of observation, 8 A.M., 2 P.M. and 8 P.M.

The Barometer is located at an elevation of 19 feet above the level of the ocean, and is suspended clear of the wall of the building.

The Rain Guage is graduated to the thousandth part of an inch, and the receiver is elevated 40 feet from the ground.

ERRATA. VOL. IV, NO. VI.

Page 696, Seven lines from the top read *tumour*, instead of *intestine*. Twelve lines from the bottom read *adhesive*, instead of *adhesion*. Six lines from the bottom *relatives*, not *relations*. Page 697, Three lines from the bottom read *sloughing*, instead of *strangulating*.

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OF

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THE NEW ORLEANS MEDICAL AND SURGICAL JOURNAL.

SEPTEMBER, 1848.

Part First.

ORIGINAL COMMUNICATIONS.

An Examination of the "Reply" etc., of P. H. Lewis, M. D., of Mobile, to the Review of his Medical History of Alabama. By WM. M. BOLING, M. D., of Montgomery, Ala.

"Oh that mine enemy would review a book!"

"I must be cruel only to be kind.—Thus bad begins."

It was our misfortune on a recent occasion to write a Review of a paper which was ushered into the world by Doctor P. H. Lewis, of Mobile, under the title of "*The Medical History of Alabama*." In the performance of this duty, we desired to give to the production such commendation as it really merited, and to deal as mildly with the author as the circumstances of the case would justify. In pursuance of this latter intention especially, we passed over not a few (to us at least) seemingly objectionable features of the paper without comment, and noticed others but lightly; and it was from the impression that we had by this course rendered ourselves amenable to the charge of remissness in the performance of the task we had assumed, that, at the close of the Review, in extenuation and in deprecation of the professional censure which we feared we might hence incur, that we remarked—"To the hypercritical, who may think we have been remiss in the performance of our assumed duty, we would suggest, that it is an easier task in many instances, to discover defects or what we may suppose to be defects, in the composition of another, than to produce a labor more perfect;"—further also, offering such apologies as suggested themselves to our mind at the time, for the imperfections of the paper. Notwithstanding

this, as truth however leniently expressed, is sometimes unpalatable, our Review has been most ungraciously received by the author of the Medical History, and has called forth a "Reply" of forty pages in the New Orleans Medical and Surgical Journal for March, 1848, in which the fairness and justice of the criticisms are called in question, our "motives" impugned, and, contrary to an expressed intention, a fitful effort also made to "carry the war into Africa," by references to, and quotations from some of our own former productions, and by injurious personalities and denunciations of a most terrible and formidable character. Now, whatever the result may have been, we will here most seriously affirm, that in the Review we had no intention to do the author injustice, none to misrepresent, nor place a false or unwarrantable construction on anything contained in the Medical History; but, so far as our observations might extend upon it, to give a fair analysis of the production, with most especial care that the inferences deduced should be fully justified by the data presented. If it has been otherwise—if, on a most careful perusal of the *reply*, we can discover one single instance in which we have done to Dr. Lewis any injustice however slight, the acknowledgement shall be fairly made and the amende accorded. As for the personalities and denunciations which have been showered upon us, we hope the reader will excuse us should we happen to pass them over quietly, or with little notice. They are weapons which as yet we believe we have not *proved*; but that elevation of sentiment, a high-toned principle of morality, pride of character, or even great profundity in medical lore, are not absolutely requisite for their skilful wielding will be admitted when it is remembered that a Billings-gate fish-woman would probably be more than a match for even the most expert.

If we are not greatly mistaken, it will appear to those who have read the History, the Review and the Reply, without bias or a feeling of partiality for any concerned, that we have not done the author that injustice of which he complains; and that where even, in the Reply, an air of probability or plausibility appears in the statements of Dr. Lewis to that effect, that it is accomplished by involving the subject in confusion, so that the true issue may be lost sight of; by starting false issues, deducing conclusions not authorized by the premises, and finally, by absolutely misstating (unintentionally of course) the facts, representing us to have said that, of which we had not even dreamed. Let us see if such does not prove to be the case.

"We regret" (commences Dr. Lewis) "our inability to make any acknowledgements to Dr. Boling for the general expression of his opinion, that the essay entitled the 'Medical History of Alabama' will prove of great value for reference, and that, as a whole, 'the paper possesses high merits.' Praise from a pen so prolific of censure is doubtless intended to soothe and palliate the wounds a self-satisfied critic imagines he has inflicted; but, as the Dr's. blows have been too feebly made to take effect, his commendations, like his censures, have fallen short of their purpose."

We have quoted the above paragraph to give expression over it, to the admiration, which the philosophical indifference of Dr. Lewis is calculated to inspire. Still in feeling we differ in this respect, as in

others, from Dr. Lewis. We cannot but be proud even of the little commendation he has bestowed upon us in the Reply, and feel almost tempted to do as he has done with ours—re-produce it here, upon our own pages, to the reader in inverted commas.

“Believing” (continues Dr. Lewis) “that a mere partizan controversy is not only unproductive of good, but exhibits its actors in an unenviable light before the public, we shall confine ourselves to a defence of the facts, and the conclusions based upon them, which the essay contains.”

The first is a wise remark, and the resolve following it, evincive of moderation; but has the Doctor adhered to it?—

“And if, in so doing, it should become apparent to the reader that this reviewer, in devoting sixty-four pages to a deformed, unfinished essay, has been actuated by feelings and motives other than those which should influence the enlightened physician, be the fault with him, not us.”

Well, be it so; we must endure it. But what motive for a review could be required other than such as would be naturally afforded by the appearance of a new production, on a subject of the deepest importance, and presented before us with a title no less ambitious than “The Medical History of Alabama,” especially to a physician of Alabama? Who is there that has read the “Medical History,” that will not think that “motive” enough is contained in it for a review? Who (Dr. Lewis alone excepted) will for a moment suppose that the “motive” which it contained was transcended in the review?—That sixty-four pages were devoted to it, as it was “deformed, unfinished,” indeed, we may further say,

“*Prodigious*, and untimely brought to light,”

will not seem so strange to others, we presume, as to Dr. Lewis. Many will be disposed to think that in proportion to the number and extent of the deformities of the paper reviewed, should with propriety be the length of the review; while, at the same time, it may appear singular that a rejoinder of forty pages should be required to a review of which the “blows have been too feebly made to take effect,” and of which the “commendations, like the censures, have fallen short of their purpose.” We have, in more than one instance before this, had occasion to refer to some curious specimens of ratiocination by which Dr. Lewis has arrived at conclusions the very reverse of what others would have considered deducible from the premises.

We do not think, however, that our commendations and censures *have* fallen short of their purpose.” One of the legitimate aims of “impartial criticism—of that criticism which is willing to award all merited praise, and fearless equally in condemning where condemnation is deserved—is to point out the objectionable features of the productions considered, to which the paternal fondness of the authors often renders them blind, that in future efforts improvement may result. Who then, we will ask, is there, that, having read the “Medical History”

and the "Reply," is not of opinion that the latter, as a literary effort, is in some measure an improvement on the former?

On the second page of the Reply, Dr. Lewis, speaking of the Medical History, tells us that "he was aware that it went into the world 'deformed and unfinished,' and vulnerable in many respects; for this he apologised, and in effect stated, that as the paper was presented and accepted by the Society, not as an attempt at a *book* or finished production, but merely as the beginning of a Society record, which is intended to note the shifting and changing features of disease within the bounds of the State; *he hoped a generous medical public*, in consideration of the matter, would excuse the manner and style in which it appeared."

It is altogether probable that Dr. Lewis did *intend* to apologise for the "Medical history," on its appearance; and no doubt thought, when writing the above paragraph, that he had done so. He of course had begun to think that some extenuation would be proper. But not one word of what he says in regard to this matter have we been able to find in the Medical History, from the title page to the end; and we think it exceedingly unkind in Dr. Lewis to belabor us as he does in his *Reply*, seeing that we really executed his neglected intentions, and that it was from our own pen that the only words that have been uttered to the public in the way of excuse for the "manner and style in which it appeared" have proceeded. How curious, too, that Dr. Lewis, here admitting as he does, that the Medical History "went into the world deformed and unfinished, and vulnerable in many respects," should still, a page or two further on, assert that there is no part of it attacked "which is not perfectly defensible!" and, moreover, seems principally disinclined to attempt to defend it at all, merely because "*it would be a waste of time*" to do so.

We quote again from the *Reply* :—

"In reference to terrestrial emanations and the influence of soils in modifying disease, the author remarks, that 'there will be no further advance in the science of ætiology without the aid of chemistry and geology.' The author here, as a matter of course, has reference to this particular branch of medical inquiry. Dr. Boling objects to this conclusion, and says that few will be found to admit the correctness of such an assertion. Now it so happens that Dr. Boling has published a treatise on remittent fevers, upon the first page of which he judiciously observes, that the effects of malaria are sufficiently *striking and peculiar* as* to produce confirmation (of its existence) in the minds of *most practical men* who have lived in tropical regions. On the same page the Doctor tells us that the chemical and physical properties of malaria remain yet to be explained. Dr. Boling here sanctioned the general doctrine of malaria. We believe it is the general opinion of mankind that *this malaria has its source* in organic remains or comminuted mineral substances. If this be so, how are we to obtain a knowledge of its chemical and physical properties (of which Dr. Boling

* This "as" has been added by Dr. Lewis.

says we are entirely ignorant) without the aid of chemistry and geology? Can Dr. Boling give 'a logical' reply to this question without sustaining 'the assertion' of the author? But as there is great want of stability and fixedness of purpose pervading his writings, it is possible that the Doctor, since he wrote his essay on fevers, has changed his opinions, and now believes in the planetary, volcanic or animalcular origin of disease, &c."

Here is a long quotation, the reader may begin to think, to very little purpose. The whole matter resolves itself into this:—Does "ætiology" mean malaria, or the chemical and physical properties of malaria? Should Dr. Lewis decide this to be the correct definition, even then, we think, there is still sufficient ground left for our remark in the Review, to which Dr. Lewis, in the Reply, objects—that "Dr. Lewis is, perhaps, rather hasty in the conclusion at which he here arrives;" for, although few will be found to deny the influence of locality and peculiarities of soil, &c., in modifying what may be called the standard characteristics of disease, the number is still smaller of those who would be willing to admit that "there will be no further advance of ætiology without the aid of chemistry and geology." We repeat, that even should Dr. Lewis decide that "ætiology" means the *chemical and physical properties of malaria*, (and unless he insists upon this, we cannot possibly discover his aim in introducing the remarks from our paper on remittent fever, relative to the imperfection of our knowledge of malaria,) we cannot admit the correctness of his conclusion; for it is not entirely impossible that our knowledge of this agent may be extended somewhat through other means than those of chemistry and geology.

Doctor Lewis, in his Reply, it will be observed, tells us that this remark was made "in reference to terrestrial emanations, and the influence of soils in modifying disease," and further, that "the author here as a matter of course, has reference to this particular branch of medical inquiry. Now it is true, that, at the time, he was speaking of the "influence of soils in modifying, &c.," as he says; but there is not one qualifying word to the remark—nothing to lead to the inference that the author, "as a matter of course, had reference to this particular branch of medical inquiry," and this only; or that he did not consider this as embracing the entire subject of ætiology. We will give the remark as quoted in the Review, from the Medical History, with the context:—

"We find in Alabama, that although the diseases may belong to the same family, yet they are distinguished by certain differences as apparent as are the physical characteristics peculiar to the regions where they respectively prevail."

"Finding that this connection and dependence everywhere exist, and the influence which physical geography exerts, independent of latitude, is not less apparent; the writer is forced to the conclusion, that there will be no further advance in the science of ætiology without the aid of geology and chemistry."

Now, *ætiology*, it will be remembered, according to the received in-

terpretation of others, means the "doctrine of the causes of disease." The "*science of ætiology*," then, must mean—the science of the doctrine of the causes of disease, or, we may in using the word *science* here, leave out the word *doctrine*, and then it would be, the *science of the causes of disease*. Here, then, in other words, is the conclusion of Dr. Lewis, which we did not think the premises warranted, and to which we objected in the Review—that there will be *no* further advance in the *science of the causes of disease, without the aid of geology and chemistry*. This, Dr. Lewis *defends* in the Reply; and to prove himself correct, or that there is inconsistency in us, (we cannot discover which,) quotes from our paper on remittent fever, showing that we believe in the existence of a disease-producing agent, which has been called malaria, and in which we admit that the "chemical and physical properties belonging to it remain yet to be explained."

Indeed, Dr. Lewis does some singular things. Upon the objection to the Doctor's conclusion in this matter, expressed in a sentence in the Review already quoted, but which we must here repeat,—“Dr. Lewis is, perhaps, rather hasty in the conclusion at which he here arrives; for, although few will be found to deny the influence of *locality, and peculiarities of soil, &c.*, in modifying what may be called the standard characteristics of diseases, the number is still smaller of those who would be willing to admit that there will be *no* further advance of ætiology without the aid of geology and chemistry,”—is based, we presume, an assertion made further on in the “Reply,” that we have said, “that the aid of chemistry is not required in ætiological investigations.”—Fie! But, we must suppose for a moment, that “’tis no sin for a man to live in his vocation.”—“Hol.”

To the charge contained in the long quotation made a page or two back from the Reply, that “there is a great want of stability or fixedness of purpose pervading” our writings, we scarcely know what to say. It is a general one, and such are easily made; but if it is predicated upon the supposition that any material change has taken place in our views in regard to the existence of an agent called malaria, which is productive of a somewhat peculiar class of diseases, we will merely say that it is without foundation. Our views have not undergone any change upon the subject. Still we do claim the right of modifying our *opinions* on any particular subject, according to the character of the testimony which may be presented and the force with which it may impress us, mainly anxious, however, at all times, that our *facts* should remain unchanged.

We will here beg the reader (intending further on to explain our reason) to look back with us some three years to an article published by Dr. Lewis, in the New Orleans Medical and Surgical Journal for March, 1845. He there says,—

“I will now travel so far out of my course as to give a few of the facts which have been gathered respecting this race” (the negro) “to other diseases indigenous to Alabama. I *practised two summers* in the interior of the State; during the autumnal months, congestive fever prevailed so generally in my neighborhood as to amount to an epidemic. There were in my circle *two blacks to one white*, yet I did not see the

first case of congestive fever in a negro, nor did I hear that any died of the disease in that section of the country."

We will next quote from the Medical History :—

"In 1835 and 1836, the writer's circle of practice embraced a population of 1500, *one thousand of whom were negroes and the others whites*. During the autumn of these two years, he treated eighty-eight cases of grave congestive fever. Among the whole number there were but *three cases occurring among negroes*, and two of these were of that variety described by Dr. Ames, and occurred late in October."

Dr. Lewis charges us with an attempt to "break up the connection of *genuine history*," and also indirectly of a wish to "stay the hand of investigation." Now we hope, that the Doctor is mistaken in this; we believe that we have never had any such intentions or desires. As the subject has in a measure become one of personal defence on our part, perhaps this little exhibition of egotism will be excused. We really think that we are attached to the Profession, and sincerely believe that those who know us have no other opinion in regard to this, than that we are anxious for its honor and advancement, and that for these ends we are willing to labor to the extent of our ability. We do hope, also, that those who have read the Medical History (must we call it *genuine history* in future?) and the Review, will come to the conclusion that we are not justly amenable to the charges above alluded to. Feeling thus on the subject, we cannot but be anxious to convince the Profession of our sincerity; and, with this view, we propose this question,* for the hand of investigation,"—as a matter somewhat pertaining to "*genuine history*,"—whether the *three cases of congestive fever* above alluded to, occurring in *negroes*, were dressed in "Buckram suits" or in "Kendal green?"

We again return to the "Reply."—

"It is stated by the author, that in the territorial days of Alabama, when the population was thin and spare, and a very limited portion of the rich cotton lands had been brought into cultivation, the fevers were of a remittent and intermittent type, and strictly non-malignant; but, that these fevers not unfrequently glided into a continued irritative type, attended with great nervousness, twitching of the muscles, general emaciation—continuing for many weeks and constituting what in these days was termed 'nervous fever.' With a few exceptions, to which the author called the reader's attention, he states that these cases of *nervous fever* were then the only ones of a serious or dangerous character. He could not discover that any malignant disease, running its fatal course in a few days, existed anterior to 1817 and 1818, hence the adoption of the word 'ataxic,' to designate the character of disease belonging to that period. Dr. Boling considers the word 'indefinite' as characterizing a state of disease.' Our recollection is much at fault, if Cleghorn, Cul-

* Perhaps it might not be amiss to suggest it to some of the Juniors, as an interesting subject for an inaugural dissertation.

len, Philip, Pinel, Brown, Forbes, Johnson, Chomel, Rush, Jackson and Bartlett do not use it in this sense. As for the application of it in this instance, we have recently discovered that *every* writer on the early diseases of Pennsylvania and Virginia made, under the same circumstances, the same application of it that the author has. Our medical nomenclature may be so defective in many instances as to require alteration or amendment; but in this the term is so proper in its application, and so firmly fixed by time, that not even the distinguished authority before us is likely to affect it.

Now, as of course we could not deny that the term "ataxic" was, though we have not time to ascertain whether it has been used by all the authors above named, in use among the most respectable medical writers, there was scarcely a necessity for a *general* defence of it by Dr. Lewis; nor can we conceive what possible shadow of injustice to him there can be in our admission, that to us the term had seemed indefinite when used as characterizing a state of disease. The remark it appears, was made in reference to the term itself. We must confess however, on reviewing again the manner in which it has been used by Dr. Lewis, (admitting that his application of it is correct, and he insists upon it that it is,) that our impressions in regard to the exact condition it is intended to designate, have not been rendered more distinct by what he says. We will quote a few paragraphs from the Review. We there remark:—

"Having always ourselves looked upon the word ataxic as exceedingly indefinite when used as characterizing a state of disease, we deem it the better way to give such extracts from the paper" (meaning the Medical History) "as make allusion to the *diseases, and the symptoms marking them during this epoch, that the reader may judge himself, as to the fitness of the term used, and its accordance with the symptoms named.*"

After the above remark in the Review, we go on to quote the following paragraphs from the Medical History, relative to the diseases of Alabama during that period which Dr. Lewis calls the "ataxic epoch:—"

"The character of fever prevailing in these years consisted of the *various types of intermittent*, with *now and then in midsummer* the occurrence of *bilious remittent*; *quotidians* in the spring and first of summer, with *tertians* and *quartans* in autumn, and frequently continuing through the winter."

"The *remittent* fevers of this day were *exceedingly mild*, yielding readily to the operation of one emetic, and not unfrequently to the spontaneous discharge of bile and copious sweats that usually occurred in the *first paroxysm*. In the most serious and obstinate cases, the intermissions and remissions were irregular and incomplete, the hot and sweating stages rapidly succeeding each other, and the chill or cold sensation becoming less distinct with every revolution."

"If, in the neglect so often attending the treatment of non-malignant diseases, these fevers were suffered to continue, they not unfrequently glided into a continued irritative type, attended with great nervousness,

twitching of the muscles, general emaciation and debility, constituting what in those days was called 'nervous fever.'"

This latter is the only paragraph in reference to the diseases of the "ataxic epoch" in which allusion is made to any condition or grade of disease, which could, so far as we understand the term, with any possible degree of propriety, be termed *ataxic*. It was the remittents only, of course, we presume Dr. Lewis means, that *glided*, if *neglected*, not *unfrequently* into a "continued irritative type," and bearing in mind that remittents only occurred "*now and then* in midsummer," and that "the remittent fevers of this day were exceedingly mild, yielding readily to the operation of one emetic," &c.,—how very small indeed must have been the proportion of cases by which Dr. Lewis characterizes the *epoch*! We really cannot yet, then, perceive with what propriety the term "*ataxic*" could be used as distinctive of an epoch in the history of the diseases of the State, in which even according to the account given by Dr. Lewis himself, a very small proportion of the cases presented symptoms of ataxia; for we can draw no other inference from his statements than that these *nervous* cases—like a few others of a rapidly fatal character, of the same epoch, spoken of also in the Medical History—were of an exceptional character. Especially are we impelled to this remark, too, when it is remembered that these "*ataxic*" cases are of as frequent occurrence in this, the "*adynamic epoch*," as they were in the *ataxic*; for, even if it should be decided that we are mistaken in our supposition, in regard to the similarity between these ataxic cases and such as we have spoken of as having seen occasionally ourselves, Dr. Lewis sets the question at rest by telling us in his "*Reply*"—referring to cases presenting the "*ataxic or nervous*" state—that "*cases of this description are common among the wood-choppers about Mobile*,"—and if *common* among the wood-choppers about Mobile, they of course occur at least "*now and then*" in other subjects; for, we may, without at all denying the influence of certain callings, with the circumstances under which they are exercised, in predisposing to or producing particular diseases, presume there is nothing in the occupation of a Mobile wood-chopper conferring on him an exclusive liability to a certain series of morbid phenomena.

Although "*every* writer on the early diseases of Pennsylvania and Virginia," in all probability, did use the term as others had done and as others still continue to do, in reference to particular cases, in which ataxic phenomena were prominent or predominant, we must, undoubtedly, question the strict correctness of the statement of Dr. Lewis, in the above quotation; that they "*made, under the same circumstances, the same application of it that the author*" (Dr. Lewis himself) "*has*"—to wit, as designating an "*epoch*," in the progress or history of diseases, in which cases with ataxic symptoms were, according to the account of Dr. Lewis himself even, as we have already shown, rare and exceptional. In the circumstance that neglected or badly managed cases of remittent fever should "*not unfrequently*," then, as now, and as has been and ever will be the case, we have every reason to believe, in all ages, be attended with what are recognized as *ataxic*, *nervous* or *typhoid*

symptoms, there is nothing peculiar—nothing belonging especially and exclusively to the “epoch.”

The diseases then, generally, of this “ataxic epoch,” we are inclined to think, even according to the descriptions of Dr. Lewis already quoted, are not *very* unlike many of our intermittents and remittents of the present day; and at the time of writing the Review, we were impressed with the belief, (and our opinion yet remains unchanged,) that we also occasionally, at the present day, meet with cases of this “nervous fever” of the “ataxic epoch,” produced, too, in a somewhat similar manner, and with this impression made the following remark:

“We imagine that these latter cases were not unlike many that occur at the present day. Indeed it is no very uncommon thing, in some parts of the State, to see cases of remittent fever, which at first perhaps, were rather of a mild character, by neglect or maltreatment, assume the appearance here described; and, in fact, the terms “nervous fever,” “typhus fever;” and “typhoid fever,” are not unfrequently applied to them in this stage; though, on careful inquiry, the peculiar characteristics of remittent fever may always be discovered to have marked their early stages.”

Upon this paragraph, Dr. Lewis, in his “Reply,” remarks:—

“When Dr. Boling supposes that the medical men of Alabama do not distinguish between the ataxic or nervous and the typhoid condition, he underrates their judgement and practical discrimination very much.”

Now it will be perceived that there is nothing contained in the paragraph alluded to, on which a construction could with propriety be placed, conveying a meaning at all disrespectful towards the great body of the *medical men of Alabama*. Dr. Lewis must have thought so, however; for we do not wish to suppose that he would willingly misrepresent or place a false construction; but in justice, at the same time, to the “medical men of Alabama,” we must state our belief, also, that the Doctor underrates their judgement and practical discrimination very much,” if it does so happen that he supposes that *they* can be wheedled into the same belief. We will remark, in the present connection, although we have the highest respect for the mass of the Profession in Alabama, and really believe that in few of the other States, if in any, can a greater proportionate amount of intelligence, professional enthusiasm or gentlemanly courtesy in their mutual intercourse be found, and also, that although we have on a former occasion, in connection with the Medical History, evinced our sincerity in regard to the present statement, by a manifest willingness to splinter a lance in their cause, it would not be less than the most gross and contemptible sycophancy to say that there were not medical men in Alabama who make a confused, loose and inaccurate use of the terms “typhoid fever,” &c., in application to other distinct diseases. We think also, indeed, that by those who have had the patience to accompany us in our former “long and weary pilgrimage” through the Medical History, it will be recollected that examples in point were there presented. But “something too much of this.” We have spent some time and wasted paper over what

may be deemed indeed a small affair—to prove that according to the construction Dr. Lewis himself chooses to place upon our remarks in the Review, on his application of the term *ataxic*, we have done him no wrong. An anxious solicitude, even in affairs of small moment, not to appear before the Profession in an unfavorable light, has led us to it; for, we know that men, with much propriety, are inclined to think that a want of fairness in one relation implies its lack in others; and that he who acts unfairly in small affairs, would probably do the same in matters of more importance. Then, indeed, we did not wish to misrepresent the Doctor, and so far, at least, we think it is manifest that he has no just cause of complaint. No one, we are sure, who has read the Medical History, will think there was a necessity for misrepresentation; but that even the most “prurient ambition for criticism” (of which the Doctor also accuses us) might, without other “motive” than the paper itself, batten over and over again, to satiety upon its pages. Appropos—in regard to this “prurient ambition for criticism,” of which we also wish to rid our skirts—it is but justice to ourselves to state, (seeing that the remark of Dr. Lewis might lead to the inference that we had been in the habit of frequent perpetrations of the kind, and ergo, it was not strange that the Medical History should come in for a share,) that prior to the appearance of the Medical History, we could claim the authorship of nothing in the shape of a review or critical notice of a book or paper, except a brief notice of one or two pages for a former number of the New Orleans Medical and Surgical Journal.

Dr. Lewis, contrary to an intention announced, as it will ere this have been observed, carries the “war into Africa,” and in so doing, quotes a paragraph from an article of our own, published some years ago on the health of the clergy, in which we gave expression to the opinion that the temperance movement had been productive of no small amount of good, and that ministers of the Gospel had been mainly instrumental in promoting it; instancing at the same time as an example, Father Matthew. The bad taste of the language in which it is clothed we freely acknowledge, and were it not for the fact that we had already blushed over it on its first appearance from the press, we could find in our heart to thank Dr. Lewis for thus calling our attention to it, that we might more certainly guard against similar errors. After the quotation to which we have just alluded, the Doctor proceeds to remark—

“The above extract, in connection with opinions advanced in the Review, places Dr. Boling in the awkward predicament of pandering on one occasion to the vanity and prejudices of the clergy, or on the other of catering to the lowest of professional appetites.”

Before proceeding to examine the main question, we will remark, that we cannot understand the meaning of the last part of the above paragraph, unless indeed the Doctor is of opinion that the love of intoxicating liquors is somewhat peculiarly a *professional* appetite; and even then, we are in doubt whether he means of all the professions or of but one, and in the latter event, whether of Divinity, Law or Physic.

We have said, it appears, on one occasion that the temperance cause has been of much service, and as a consequence of this, we believe of course, that the abuse of alcoholic liquors has been the cause of much mischief; that it is, in short, detrimental to health. Now, that we should really be reduced to the "awkward predicament" in which the Doctor would place us, it should be made to appear that we have in the Review given utterance to sentiments of an opposite character. Let us see if this has really been the case. If it has been, we retract them.

In the medical History, it will be recollected, Dr. Lewis tells us, that during the "epoch" in which diseases were of an *ataxic* character, the constitutions of the inhabitants were most robust and vigorous; and he also tells us, that during the "phlegmonic epoch," when the diseases were of the *phlogistic*, or highly *inflammatory* grade, a general state of *enfeeblement of constitution or impaired health*, had become manifest among the population. Among other causes advanced by the Doctor for this general impairment of health, was "excess of libation." It seemed strange to us, we must confess, that during an epoch, in which the inhabitants of the State were favored with the most robust and vigorous constitutions, (a condition as a general rule all admit, we believe, under which diseases are liable to assume a *high phlogistic* grade, when they do occur)—the diseases should be of an *ataxic* character, and more strange, that during an "epoch" in which "the *once* stalwart man was reduced to a state of decrepitude" &c., during an epoch, in which from "excess of libation" &c., (are not the diseases of the drunkard preeminently *ataxic*?) there was much enfeeblement of the general health of the people, and great constitutional debility—circumstances under which diseases are prone to assume a *low* grade of action, they should appear of a *highly phlogistic* grade. We did not, however, deny the possibility of the occurrence of a combination of circumstances that might thus reverse the more common order of things, but it did seem most strange of all to us, that Doctor Lewis should offer this very condition of impaired health, and enfeeblement of constitution, as explanatory, in part at least, of the highly phlogistic or inflammatory grade, which he says marked the diseases of the "phlegmonic epoch." The question was not then as to whether "excess of libation" and other irregularities, and sensual indulgences indeed, calculated to impair the general health, predisposed to disease, (for all admit this,) but whether this deteriorated condition of general health, in whatever manner induced, could with propriety be offered in explanation of a highly phlogistic character or grade of disease when it did occur, and it was in regard to this that our doubts were expressed. We quote from the review, for we cannot make our meaning plainer here than it is there, to prove that in this we have not misrepresented Dr. Lewis, as also to show that we have not said one word in denial of the deleterious effects of Alcoholic drinks, and consequently that he has not placed us in the "awkward predicament" of which he speaks. The question then we repeat is not as to whether, "sensual indulgence," "excess of libation" &c., are calculated to induce disease; but whether diseases occurring in constitutions impaired and enfeebled by these and other means combined, generally assume a highly phlogistic grade. Here is what ap-

pears in the Review. What we have there quoted from the Medical History, we will on the present occasion place in italics, to distinguish it from our comments, made at the time.

"We will now examine some of the circumstances, which, according to Dr. Lewis, gave to the diseases of this epoch, (the ataxic) their peculiar features, by which they are distinguishable, from those of the succeeding epoch."

"As a consequence of these changes (spoken of in the paper) of atmospheric and terrestrial temperature, the rapid increase and decay of vegetable and animal matter, together with physical causes not enumerated, such a change in the gaseous emanations is doubtless produced, as tends to designate the character of diseases at various periods of time."

"Connected with this matter there is at the same time an altered condition of the constitution and temperament as man advances from a state of native simplicity to the refined and luxurious habits that wealth can command. In that primitive state of existence the mind was unfettered by corroding cares, the articles of food simple in their nature, and the oaken couch was as soft to the wearied husbandman, as a pillow of down—then the robust frame enjoyed an almost uninterrupted condition of health, and great longevity marked man's pilgrimage on earth."

"But when refinement slowly and stealthily creeps into his habitation; when the rude cabin obscured in the tangled vines of the wild-wood, is exchanged for the more modern mansion of the exposed and cultivated plain; when the unlicensed freedom of sensual indulgence, with the pampered appetite, and excess of libation reign supreme, then is forcibly marked the cause of disease, tending to reduce the once stalwart man to a state of decrepitude, and the prattling urehin instead of the glow of health that once sat upon his cheek now presents the aspect of refined infirmity, laying the foundation of an early grave."*

"To these various causes can be traced a gradual change in the character of disease from the first occupancy of the country to the present day."

"The early settlers came mostly from Georgia, Carolina, Virginia and Tennessee. Owing to the fact that these immigrants brought but few laborers with them, they chose the upland and hammoeks, rather than encounter with a feeble force the rich and heavy timbered alluvion of the river swamps."

"Simply then we must infer that the circumstances principally which contributed to the comparative good health of the earlier settlers of the State, were, their avoidance of the "rich and heavy timbered alluvion of the river swamps," the small extent of land as yet brought under cultivation, "exposing the virgin soil to a tropical heat," and consequently the comparatively small amount of poisonous material disengaged; as also the absence of that enervation of the system produced by "sensual indulgence," and the "refined and luxurious habits that wealth can command."

"Though the facts relative to the character of the diseases prevalent

* The dissipated little rogues!

during the first and second epochs, the "ataxic" and the "phlegmonic" cannot be thereby invalidated, we must deem the explanation of Dr. Lewis anything but satisfactory. Thus the diseases of the first epoch, we must suppose, of course to have been characterized by ataxic symptoms; while those of the second, as the name would imply—are described and spoken of as of a highly inflammatory character; and yet we are reminded, and forcibly impressed with the idea, that during the *ataxic* epoch the inhabitants were possessed of the finest constitutions; that in this primitive stage of existence the mind was unfettered by corroding cares," and that, "then, the robust frame enjoyed an almost uninterrupted condition of health, and great longevity marked mans pilgrimage on earth." Surely in such subjects, as are here described, we would naturally expect diseases to assume a highly inflammatory character; though the facts seem to have been otherwise according to the information collected by Dr. Lewis—still on what grounds he offers this condition of stalwart health, in explanation of, or as among the causes of the *ataxic* character which the diseases of this epoch assumed, we cannot comprehend."

"In regard to the *phlegmonic* epoch, we are informed, that by this time, the "rude log-cabin had in many instances given place to the stately mansion;" and it is to this period that the author alludes, when he speaks of the enervating habits, and sensual indulgence at the command of wealth, "tending to reduce the *once* stalwart man to a state of decrepitude, and the prattling urchin, instead of the glow of health that once sat upon its cheek, now presents the aspect of refined infirmity, laying the foundation of an early grave."

Another quotation: "*And again, we should not forget that there is a principle well established, in the progress of society, that as wealth accumulates, so luxury creeps into the favored domicile, and under the imperious sway of sensual indulgence, the wealthy inmate is tortured with a protean disease unknown to the tenant of an humble mansion. From the causes thus briefly alluded to, we may distinctly trace on rational conclusions, that marked change in the character of disease, so fully exhibited during this important epoch.*"

"Now we can conceive it possible, that circumstances might conspire, to produce a tendency to high inflammatory action, even in the prevalent diseases of a period, or in a condition of society, in which "luxury and sensual indulgence," and "excess of libation," had tended to "reduce the once stalwart man to a state of decrepitude," but must think the author decidedly at fault, as advancing indeed a doctrine, contrary to one of the most firmly established principles of medicine, when he offers this very state of decrepitude and deteriorated health in explanation of, or, at least, as among the causes of the high inflammatory symptoms characteristic of the diseases of this epoch."

"We cannot but look upon it as a matter to be regretted, that the Dr. could not so have arranged his *facts*, that those circumstances "tending to reduce the once stalwart man to a state of decrepitude," &c., should have been in operation during the *ataxic* period; and equally so, that it could not have been so arranged that during the *phlegmonic* period the mind should have been "unfettered by corroding

ing cares," and that then the *robust frame* should have "enjoyed an almost uninterrupted condition of health, and great longevity marked mans pilgrimage on earth!" But facts, of course, cannot always be bent to meet our wishes."

Where in all this can be found our denial of the deleterious effects, of "excess of libation?" If Dr. Lewis finds himself in regard to this matter in an "awkward predicament," it has certainly not been necessary to resort to a misstatement of the facts, or to the starting of a new issue, to place him in it.

In his reply Dr. Lewis tells us, (with what object or aim we cannot conceive,) that, "we can here in Alabama point Dr. Boling to hundreds of pioneers of the country, who, now at the age of sixty and upwards, are treading with a firm and manly step upon the graves of their own children." Individuals in all times past, have, we believe, lived, (and in all future times we presume, will live) to the age of sixty—(and "all flesh is grass"—and *children* will sometimes *die*. Surely the Doctor does not mean that this "great longevity" is an exclusive boon conferred upon the generation of his "ataxic epoch," and that none of the generation of his "phlegmonic epoch" will be thus favored.

In the review of the Medical History, we introduced incidentally, as containing a somewhat interesting historical fact, a quotation from Dr. Heustis, relative to an epidemic that occurred in Cahawba, in the autumn of 1831, without further comment, than that the account given, "did not sustain the authors views relative to the highly inflammatory character of the diseases of the epoch, the phlegmonic, on which we were then touching. No one who reads this account contained in the American Journal of Medical Science, for February 1832, will doubt for a moment the correctness of the statement. We made no remarks upon the accounts of Dr. Heustis, of preceding or subsequent epidemics, we made no inferences—we introduced the quotation incidentally, as we remarked just now, and as bearing upon a circumstance historically interesting, to wit: That during the phlegmonic epoch, *one* epidemic at least had occurred, anything but highly inflammatory in its character. Dr. Lewis, wherefore we cannot discover, takes umbrage at this. He tells us that he made in the History quotations from Heustis, relative to the epidemics of Cahawba, from 1818 to 1830. Granted our quotation was from the account of the epidemic of 1831—but even had it been made from the accounts of which Dr. Lewis speaks, provided we quoted correctly, making merely a statement of the fact, why should Dr. Lewis complain? He is dissatisfied too on another point; charging us with the "suppression of important relative events." We think, however, that our quotations from the History were neither few nor limited in extent. Had we, however, in a formal manner intended to attempt the disproval of *all* that Dr. Lewis said in regard to this phlegmonic epoch, and to show that no diseases of an inflammatory character occurred during its continuance, we should have been most careful that no "important relative events," should have been omitted. We did not merely, either, as the Dr. intimates, take an "account of a *few* cases occurring in one epidemic," but we gave the account in the language of Dr. Heustis, relative to the general characteristics of the epidemic. "The fever this season was of a congestive character" &c.

Dr. Lewis winds up his remarks upon our allusion to Dr. Heustis' account of the fever of 1831, with this terrible denunciation.

"The reviewer, who, by such partial extracts, by the suppression of important relative events, and by unwarrantable inferences," (the inferences at least were drawn by Dr. Lewis—we made none) "will attempt to break up the connection of *general history*, not only places himself without the pale of polite controversy, but weakens that confidence in himself, which should be reposed in the representations of one writing upon scientific subjects."

We "appeal" from Dr. Lewis' decision. His premises were "unwarrantable" we opine, consequently his conclusions. We will at all times, and under all circumstances, make it an object to keep within this "*pale*," however it may suit the tastes or emergencies of one with whom we may have the bad, or good fortune, to differ in opinion, to deviate from it.

We copy further from the *reply*.

"In another part of the review Dr. Boling uses this epidemic of 1831,* as true congestive fever, thereby leaving the inference that the

* We will now go even ten years further back, and present a few brief extracts from the account given by Dr. Huestis of the epidemic at Cahowba in 1821, in his work on fever.—

"Upon the decline of the hot stage, the circulation at first grew languid in the extremities, which often became cold; though there might be at the same time, a *violent palpitation and throbbing of the heart*, as might be perceived upon applying the hand to the bosom, or even by the visible motion of the thorax. * * * * * "It frequently happened upon the subsidence of the first exacerbation that the arterial action became weak, cord-like, frequent and almost imperceptible."

"The inequality in the distribution of strength, between the arterial and vascular systems," (a typographical error here exists, we are inclined to think. The context shows that it should read—between the arterial and *muscular* systems.) "constituted a striking feature of this disease. Sometimes, though much more seldom, the *reverse* of this took place; the pulse indicating considerable strength of *vascular* action, whilst the debility of the *muscular* and nervous systems was so great, that the patients were unable to continue, even for a few moments in an erect position in the bed. * * * * * "I have known such *sweats* to continue for hours, with a coldness of the surface and extremities, and without any pulsation of the artery, at the wrist, and yet the patient recover: such recoveries, however, only took place where the prostration occurred in the *early* stage of the disease."

But here is a case:—

"In one patient, Mr. LaTourett, who fell a sacrifice to this disease, the vascular system never acquired any strength and fullness after the first twelve hours; but throughout the disease, there was great prostration of the *vital* action, whilst, at the same time the strength of the *muscular* system was disproportionately great. This young gentleman was affected with *inexpressible anxiety* and *restlessness*. Remonstrances and entreaties were alike ineffectual in *confining him to the same situation*. He would occupy successively, and in a short time, every bed in the chamber; and such was his *muscular strength*, that without any apparent difficulty, he would walk from one part of the room to the other, though at the same time his pulse was scarcely perceptible at the wrist. He was constantly complaining of a *distressing sensation of heat*, and insisted upon

author of the Medical History is wrong when he asserts that this disease did not make its appearance until 1834. Although we can satisfactorily demonstrate the great dissimilarity between congestive fever and the disease here described by Dr. Heustis, it is here unnecessary—Dr. Heustis is, we presume, good authority, at least in relation to the diseases he had described and the conclusions to which he had arrived. In an unfinished paper on typhoid and adynamic pneumonia, which has been placed among the documents of the Mobile Medical Society, he uses the following language in relation to “congestive pneumonia.” “Many cases of a similar character occurred during the prevalence of the malignant pneumonia in New Orleans in 1814, noticed in my “observations on the diseases of Louisiana,” but since then I have rarely met with a case, until after the *appearance of congestive fever* in 1834. Since then, cases of this description are not uncommon on the Alabama river.” Before attempting to correct others in matters of fact Dr. Boling should inform himself properly, not only in relation to their authenticity, but the succession in which they run—he should be careful how he pushes his *little shallop* into waters he has never explored.”

Now notwithstanding the boast of Dr. Lewis, that he could “satisfactorily demonstrate the great dissimilarity between congestive fever and the disease here described by Dr. Heustis,” it will be seen that he does not make the attempt. It was “unnecessary.” The account of Dr. Heustis of this epidemic, must stand for itself. We know not what his *unpublished* opinions were; but we think all will admit that the quotations which are presented in the review, from this paper, so far as it extends, embraces one of the very best descriptions of congestive fever extant; and though incomplete, can leave no doubt, we feel assured, on the minds of those who read it without bias, that the account of the epidemic was made up measurably from cases of the grade of fever known in the South under the term *congestive*, though every case of course oc-

being exposed naked to the open air, whilst *his skin, to the touch, was below* its natural temperature.”

Dr. Lewis, further on in the Reply, introduces quotations from an article on the subject of Congestive Fever, by Dr. W. B. Johnson, which he thinks strongly supports his views in regard to the action of the heart, &c., in this disease. If the description of the disease by Dr. Johnson suits Dr. Lewis, of course he cannot object to the cases on which the description may have been based. In reply to an inquiry which we made of Dr. Johnson, in regard to the particular locality, &c., in which the cases of congestive fever occurred, from which his description was made up, he says:—“The first distinctly marked cases that came under my observation were in the year 1822, since which time I have seen more or less of the disease every year. The *locality* where I first met with it was in Triana, in Madison County, Alabama.” Dr. Johnson, then, it seems, saw the disease in Alabama *every year for fourteen years before the date* at which Dr. Lewis asserts that it first made its appearance in the State,—every year for some ten or twelve years before the epidemic alluded to in the Review, spoken of by Dr. Heustis—from which our quotations were made, which were so strongly objected to by Dr. Lewis, as containing an attempt to “break up the connection of *genuine history*.”

curring during the season was not of this grade. We present it again before the reader.

"*The fever of this season*" (1831) "*was of a congestive character, with a great disposition to unequal distribution of febrile action and development. In many cases the head, and particularly the extremities, were cold while the trunk was hot; whilst at the same time there was considerable palpitation of the heart, with a pulse small, weak and frequent.* * * * * * Sometimes after a transient state of febrile excitation, collapse took place at an early stage of the disease, with little impairment of the general powers of the system. This was more especially the case when depletion had been practised too freely. * *

* * * * * The prostration and collapse are, for the most part, confined to the vascular system, while such is the strength of the muscles of locomotion, that the patient is able to rise and set up, and even to walk about."

A favorite plan with Dr. Lewis, it will be seen, is, whenever occasion may require it, to introduce a "correspondent," not informing us, however, whether on such occasions he does his correspondent and his readers the justice of presenting *all* he may have said on the subject. In the long extract made but a short time since from the *Reply*, a slight variation of this course is adopted, and he calls up an unfinished and unpublished paper of a deceased writer. Now it is really too bad, we think all will agree, that Dr. Lewis should upbraid us for not being properly informed in regard to matters contained in this unpublished paper; and sure we are that there are many "*properly informed*" members of the Profession who have not now and, we fear, never will possess the good fortune which has fallen to his lot in regard to it. Respecting the "*authenticity*" of the paper of Dr. Huestis, from which our extracts were made relative to congestive fever, we presume, of course, there can be no possible question, published, as it was, in the most prominent and perhaps widely circulated journal of the day in the country, *under his own signature, many years before his death, and containing nothing contradictory of, or even presenting a seeming inconsistency with, any of his previously expressed opinions.* Had the paper not been *genuine—authentic—*would not Dr. Huestis—*alive* at the time of its publication, and living many years after—have indignantly contradicted it, to the mortification of its author, and discomfiture and exposure of the duplicity that could lead to the attempt, for some sinister and selfish purpose, to foist upon the Profession, under his respected name, a spurious production? Of what then contained in the text just quoted from the *Reply* is the "*authenticity*" doubtful?

In our boyhood's days, we remember to have read something like the admonition here given us by Dr. Lewis, about pushing the "little shallop" into unexplored waters,—

"Vessels great may venture more,
But little boats should keep near shore,"—

And we have been reasonably solicitous that the adage should have over our "*ventures*" a wholesome influence. Of course we may not at all times have been sufficiently guarded, and may have ventured into

waters beyond our "*depth*." We are at a loss, though, in the connection in which it appears, to know exactly what construction to place upon the prudential caution given us by the Doctor; for, we think we have shown that he could not, with even a semblance of propriety, taunt us with not having *explored* a paper, of the existence even of which we had no means of becoming aware; and whatever may be his *secret* opinion, in regard to the Medical History, his modesty would scarcely preclude the supposition that he had reference to *its depth*; perhaps, however, he means—we ventured too deeply into it.

On reading the Medical History, we were at times much puzzled in regard to the exact sense in which the author made use of many terms. We thought, however, at the time, and we are still impressed with the same belief, that in one part of the essay at least, he assumed congestive fever as the type of the diseases of the present epoch, according to his division—the adynamic. He does certainly insist upon it, if we are not mistaken, both in the History, and in the *Reply* even, that congestive fever did not appear in the State before the present epoch. Thus, in the *Reply*, he says—"In another part of the Review, Dr. Boling uses this epidemic of 1831 as true congestive fever, thereby leaving the inference, that the author of the Medical History is wrong when he asserts that this disease *did not make its appearance until 1834*." Well, then, in the *History* he states—"In 1834, the change in type and character, as contrasted with 1828, was complete and striking. During the summer and autumn of this year, the red and scarlet livery of former years had disappeared, and disease henceforth robed itself in darker and more gloomy colors. * * * * *

We find its approach was insidious and unobserved, giving no serious warning of its proximity, until the unconscious victim was secure in its grasp. The patient first complained of depression, *heat* and *burning*, when to the touch the surface was icy cold; that cold—that first stage, is now the stage of disease and peril, and that reaction, which in past days was looked to with fear and trembling, would now be hailed as the messenger of returning health and vigor." Who, we ask, on comparing this with the more special description of congestive fever, by Dr. Lewis, further on in the History, will say that we were not justifiable in our supposition, that his model, for the general description here of the diseases of the epoch, was congestive fever—that he had in view congestive fever as "his type of the diseases of the present epoch."

Again we have Dr. Lewis, in the History, speaking of the "low state of typhoid affection, that marks the character of all disorders of the present day." What else could we infer from this, but that if the Doctor did not mean to say that typhoid fever—otherwise known, also, as the "typhoid affection,"—and congestive fever were the same; that to justify his use of the term typhoid in its other sense, he must find a *striking resemblance* between *typhus fever* and *congestive fever*; and as in the Review, we are still impressed with the belief, "that there are few acute febrile diseases more dissimilar," or "more diametrically opposite in the manner of their developement and progress, and in their symptoms generally."—It is scarcely worth while to examine as to the cor-

rectness of the complaint of the Doctor, that we brought from remote sections of the History parts for comparison. It is sufficient that we compared and attempted to reconcile the parts—how many pages apart we have no recollection—and the result was as stated. In regard to this matter, Dr. Lewis proceeds to observe :—

“Dr. Boling then proceeds to make such extracts from the Medical History as, in his opinion, sustain his assertion ; then, by bringing from a remote and different section of the History the author’s words, “typhoid affection,” by which he has already so sagaciously assumed that he ” (the author) “means *true typhoid fever*, makes out his case and remarks—‘hence Dr. Lewis must believe in the identity of congestive and typhoid fever.’ Having out of this material erected an airy fragile castle, unequal in strength to his *pathological* prowess, the Doctor, Quixotte-like, proceeds with an extraordinary flourish of trumpets to its demolition.”

We must pause over this for a moment. Can anything be more recklessly inaccurate ? Does Dr. Lewis ever “think twice” before he writes, or is it that he sometimes *launches* his ocean steamer—his man-of-war—his three, aye, his four-decker—under full sail, without glass, quadrant, compass or chart, “into waters he has never explored ? Here we are by the Doctor, for instance, likened to the worthy knight of the rueful countenance, because, forsooth, *we* proceeded “with an extraordinary flourish of trumpets” to the demolition of this “airy fragile castle ;” whereas, had the Doctor been at all familiar with the “genuine history” of this redoubtable “avenger of grievances,” of whom he ventures to speak, he must have been perfectly aware that it could not have been his wont to proceed to his *demolitions* “with an extraordinary flourish of trumpets,” for the very best reason in the world—he had neither a regular trumpeter nor trumpet ; his faithful squire, Sancho Panza, constituting his only retinue during almost the entire period of his knightly adventures.

Among the French writers generally, and also a considerable proportion of American writers, at least two distinct fevers are recognized under the terms *typhus fever* and *typhoid fever*. The latter is also known under other names, as for instance, entero-mesenteric fever, enteric fever, the typhoid affection, dothineritis, &c., many objecting to the name *typhoid fever*, as applied to it, because the disease does not necessarily present symptoms of a typhoid character, or in other words, has not in all cases a marked resemblance to typhus, but is occasionally of a sthenic or inflammatory grade. By the writers alluded to, a typhoid condition or grade of fever is also recognized as occurring in febrile affections and in the phlegmasiæ, other than the distinct diseases called typhoid fever and typhus fever ; the term being confined in this sense also, as is implied in the name, to cases which assume a resemblance to typhus. Because, then, a case of fever may assume a typhoid condition—a state resembling typhus—it does not follow necessarily that it is one of the diseases called typhoid and typhus fever ; and writers, generally, in speaking of other fevers in which this condition has supervened, do *not* call them typhoid or typhus fevers, or we are much mistaken ; though, in referring to them under their appropriate

and specific designations, some term in addition may be used expressive of the general condition and grade of febrile action. We do not think, however, that Dr. Lewis has in all instances been thus particular; and in addition to the seeming confusion and the uncertainty in which we are left, in regard to the *identity* of congestive fever and typhoid fever, or a *resemblance* between congestive fever and typhus fever, we find him in other parts of his paper using the term *typhoid fever* in reference to cases, which, from the circumstances of their origin and the symptoms given, we believe all will agree, could not have been cases of this disease, and even, we believe, on the same page and in connection with an account, from another source perhaps, corresponding pretty well with the disease; the former, and the circumstances under which they are grouped and treated of together under the same term, serving to involve in confusion and doubt the real character of the latter. We will not, as it would occupy no little space, quote here the various paragraphs having a bearing on this, interspersed through different parts of the History, but must refer to the paper itself for confirmation of our statement.

The British writers generally, it will be remembered, do not admit the distinction above referred to—treating of all the cases embraced by others under the two heads, typhus fever and typhoid fever, under the single head, typhus fever—believing in their identity. Many of these writers, however, use the more comprehensive generic term, “continued fever,” in the accounts they give of their fevers; and in their descriptive remarks are governed by the *grade* of febrile action rather than admitted or supposed specific differences, and in this sense now use the terms synocha, synochus, typhus and typhoid; the former to indicate fever of a high inflammatory grade; the latter fever of a low grade, attended also with nervous disturbance, and often with what are called symptoms of putrescency; and the second, fever holding a grade intermediate between these two. Of course, these different *grades* of any one distinct fever, pass into each other by imperceptible shades, and it would be impossible to draw the exact line of demarcation; which, however is not considered of so difficult accomplishment between distinct and separate fevers.

This being generally understood of these writers, no confusion could well arise in regard to their descriptions, even if the details were not sufficiently explanatory. In using the term *synochus* then, for instance, they do not, we believe it is generally understood, intend to say that there is a distinct and separate disease called synochus fever, but that the case or cases referred to are of a synochus grade. Perhaps we ought to apologise to the reader for occupying so much of his time with these statements before proceeding to present the *explanations* of Dr. Lewis, in the Reply, by which he *elucidates* his views, and from which, with other parts referred to, he deduces the charge against us of erecting an “airy fragile castle,” &c.

Concluding a quotation from the Review, embracing a doubt in regard to the sense in which he had used the term *typhoid* affection, Dr. Lewis proceeds to explain as follows:—

“With all the Doctor’s pretended ignorance of the author’s meaning,

it strikes us as being too plain to admit of a doubt. We will here inquire what is the general understanding, not only among men in Alabama, but throughout the world, in relation to continued fevers, of an intermediate grade, between pure synocha and pure typhus, or between the periodical fevers of autumn and true typhus of winter. They are called typhoid, adynamic, ataxic or inflammatory, as the symptoms essential to one of these conditions may predominate in the case before them. If, for instance, in Alabama a case of periodical fever, whether in the remittent or continued stage, is attended with many of the phenomena belonging to typhoid fever, it is called *typhoid*; so also with many of our diseases, especially pneumonia, in which this element is frequently manifest."

We certainly admit, that in Alabama, as elsewhere, the word *typhoid* is very properly used by many to designate a typhoid condition existing sometimes in other diseases than typhoid fever, but generally in such a manner as to enable the reader to know at once whether *typhoid fever* is meant, or another distinct disease, attended merely with typhoid symptoms; and we know few besides Dr. Lewis who use the term in such a manner (intending this meaning) as to lead to the literal inference that the typhoid fever is meant. For instance, when they say *typhoid pneumonia*, &c, it is all plain and easily understood. Should, however, a writer—as we believe we have shown has been done—use the term "typhoid fever," or any of its synonyms, as "typhoid affection," &c., in reference to a disease which has generally been considered altogether different—the circumstances and the symptoms presented indicating distinctly the difference, we do not think that any manifestations of surprise should be indulged in, if doubts are expressed in regard to his true meaning.

Well, then, it would seem according to Dr. Lewis, that "the general understanding, not only among men in Alabama, but throughout the world," is, that continued fevers of an *intermediate grade*, between *pure synocha* (a fever of an *inflammatory grade*) and pure typhus, are called "typhoid, adynamic, ataxic, or *inflammatory*," &c. An *inflammatory grade* of fever, of an *intermediate grade*, between *inflammatory fever* and pure typhus? Also, "*continued fevers* of an *intermediate grade*"—"between the *periodical fevers* of autumn and *true typhoid* of winter," according to the same authority, are called typhoid, inflammatory, &c.; as if the periodical fevers of autumn were not *themselves*, under different circumstances, of all grades. Or, does not the author, in this last part of the paragraph, have reference to the actual grade of febrile action? Or, are we to infer that his meaning here is, that the periodical fevers of autumn and the "true typhoid of winter" are not distinct and separate diseases, but are mere presentations of the same fever in different shades, grades or phases? We cannot comprehend the Doctor, though he insists upon it that his meaning is "too plain to admit of a doubt." We know he must, (at least we *think* he must,) sometimes, in the Medical History, in using the term *typhoid fever*, have really intended to speak of the distinct disease recognized by authors under that term; but really his numerous quotations from the English authors, in which these terms, *typhoid*, *typhus*, *synochus*, and *synocha*, are used generally in a different sense—to designate the grade of

fever merely (these quotations being made, too, to prove his use of the term typhoid correct in this sense, at a time when, and under circumstances, or in reference to statements, which, as much as any other in the History, would lead to the inference that he meant the typhoid fever itself)—invests the affair with so much doubt that we know not what opinion to form. If, at one moment, we may think there can be no mistake as to his meaning now,—perhaps on the next page or in the very next paragraph, we find something to reverse the conclusion, or to involve it in the greatest perplexity. It would be too much of a tax on the reader, to introduce, just at this time, the paragraphs to which we refer, but those who have read them will, we think, incline to the belief, that if Dr. Lewis, in the Medical History, anywhere means to speak of genuine typhoid fever, and has not always reference merely to typhoid grades of other diseases, he uses the quotations in which the term is applied in the latter sense, to prove the correctness of his use of it in the former sense. The Doctor ventures on a *simile*, in the points of comparison instituted, of which we have shown that no similitude existed. May we indulge in one? The Doctor then reminds us of a certain fish, a *sly fish* too, which, when closely pressed, resorts to the expedient of blackening the water through which it swims; and we think on reading the paragraphs to which we have just referred, in connection with what he says in the Reply, and what he has said in the History on *continued fever, typhoid fever, &c.*, it must occur to the mind, that like this wily little animal, he has attempted concealment or escape, in confusion and turbidness intentionally self-created. We proceed:—

The Doctor also tells us that, if “a case of *periodical fever*, whether in the remittent or *continued* stage, is attended with many of the phenomena belonging to typhoid fever, it is called *typhoid*.” This looks a good deal like the fever of an *intermediate* grade, between *inflammatory* fever and typhus, being called *inflammatory, &c.*

We find ourselves at this point unwillingly compelled to introduce a few quotations (in which Dr. Lewis speaks of typhoid fever) from the Medical History and from the Reply. We will abbreviate as much as possible.

After remarking that idiopathic typhoid fever is not the prevailing disease of summer and autumn, but more properly belongs to winter and spring, he goes on:—“Yet as the general tendency of the *remittent* is to the *continued typhoid*, taken in connection with those that are essentially so, * * * * * we cannot err in stating that typhoid fever prevails in this section” (the upland country we believe) “of the State, to some extent at least.”

“In the summer of 1839 and '40, the writer witnessed many cases of fever of various types in this section of country—some of which, termed “typhoid congestive,” were attended with a coldness of the extremities, profuse perspiration and a strong accelerated pulse. Contrary to the usual course of fever, the perspiration increases with the arterial excitement and subsides with it. In others again there is bilious vomiting, soft feeble pulse, moist yellow tongue, stupor or coma.”

"In these forms of disease the fever generally lasted from ten to fifteen days, and may be comprehended under that variety described by Chomel, as not being essentially typhoid fever, representing different degrees of severity."

"Typhoid fever, whether *remittent* at the first or continued from the onset, is attended," &c.

"This fever is not so strongly marked in autumn as in winter, and separated from the latter would *not deserve the name*, as it does not comprehend the strict definition of typhoid fever."

"In the treatment of the continued fevers" (in continuation of the same subject) "of this section of the country, whether they are *idiopathic*, or supervening upon a *remittent type*. * * * * * *Quinine has been used at different periods in every variety of dose and form, but always tending to aggravate the disease and increase that disposition to local inflammation which usually exists in the cases of that region.*"

In another part of the paper, (on winter and spring diseases) the Doctor, after stating that it is a generally received opinion that, "typhoid fever, as described by Bartlett and others, has been an annual disease, although not prevailing as an epidemic except from some local and strongly exciting cause," goes on to observe:—

"The first information that is presented to our notice of its appearance in a marked form, was in Dallas County, during the Summer of 1835. In this instance, it was the sequence of *typhoid pneumonia* that had prevailed during the previous winter, assuming at that time an irregular *intermittent type*." The strongly exciting cause, in this instance being several hundred bushels of cotton seed, left in a pile to decay for manure, near the negro houses. About the middle of January several cases of *pneumonia* were developed among the negroes, which continued to attack them until the month of March. The spring opening *warm*, the disease immediately assumed a new type, and continued to prevail until every negro above the age of five years had been seized." Will it not strike the reader that the cause here named was calculated to lead to a suspicion of a disease of a malarious character? And, during the winter at least, the disease does seem to have been *remittent fever*, complicated with pneumonia. Whether Dr. Lewis gives this as genuine typhoid fever, or, whether this is one of the instances held in view in his quotations from the British authors recently alluded to, in justification of his use of the term typhoid fever, to mean something else, and intends merely to say that this was miasmatic or periodical fever of a low grade, we cannot pretend to decide.

Immediately following this, and in the same connection, is given an account of another epidemic, produced under somewhat similar circumstances, quoted and used by the Doctor as typhoid fever—called this at least, and not miasmatic fever of a typhus grade,—in which "there were as many as sixty cases, all of which were benefitted by Quinine.

"The anatomical character of this disease," (still on typhoid fever,)

"from the examinations made by Dr. Clark and the author, appears to consist of an enlargement and softening of the spleen, dark, leaden appearance of a portion of the small intestines, with now and then thickened, dark ash-colored patches in the ileum, which are easily scraped down; very little redness noticed any where in the canal. but on the contrary a pale darkened appearance. In two cases that died in the Marine Hospital, there was ulceration of the elliptic plates of the ileum, such as the writer had seen in the Massachusetts Hospital—the mesenteric glands were enlarged in every case.

In all this account of the *anatomical characteristic* of typhoid fever, we have not one word in regard to the condition of the elliptic plates, except in the two cases in the Marine Hospital. We feel interested to know whether the Doctor, in speaking of the enlargement of the mesenteric glands, "*in every case*," means only these two cases, or were they also enlarged in all the others,—the number not given—in which it is presumable that a morbid condition of the elliptic plates did not exist—the lesion not being mentioned?

We must return to the Reply again, premising that much of what we have here quoted from the History, has also been presented again by Dr. Lewis himself in the former.

"The physicians throughout the southern country are in the habit of calling all cases of continued fever, the leading symptoms of which, regardless of their anatomical characters, are those resembling the morbid phenomena of typhus,—typhoid fever."

Now we are at issue with Dr. Lewis here, and really believe, that—admitting exceptions, however—he is mistaken in this matter as regards the great body of southern physicians, especially in the latitudinarian sense, in which he uses the term *continued* fever, and in view of the numerous instances contained in the *History* and *Reply*, showing that occasionally, at least, when speaking of *continued* fever, typhoid fever, &c., he mixes up, in his descriptions, certain grades of *periodical fever*. Thus, the reader may possibly remember a paragraph in which these words occur—"the treatment of the *continued* fevers of this section, where they are idiopathic, or supervening upon a *remittent type*,"—and these—"the general tendency of the *remittent* is to the *continued* typhoid,"—and these—"the *remittent*, or according to a strict definition, the *continued* bilious fever;"—and then of a *regular gradation* of disease with the advance of the season, in these terms—"Intermittents and irregular bowel affections prevail in *spring and early summer*; as the *season advances* they are *replaced by remittents*; late *summer* and *autumn* find that the *latter* have attained their *maximum*, and the fevers are *continued*." Now we think it is sufficiently evident, that in the continued fever of Dr. Lewis there are occasionally mixed up certain grades of periodical or miasmatic fever; and we must again repeat, that he is mistaken in saying that "the physicians throughout the southern country are in the habit of calling such cases *typhoid fever*, even though they should be presented of a *typhoid grade*."

We quote again from the *Reply*.—

"After *summing up* the facts collected, in relation to the diseases of

Alabama, the author in effect states—that periodical fever is the common endemic of the country at *all* times; that previous to 1830 it was inflammatory, since then, and especially since 1834, it assumed a *low typhoid type*, in some localities and sections of the country, adynamic, or in the language of Watson, it became unusually *asthenic*, offering a strong contrast to the fevers of the preceding period.”

Now this is explicit and to the point, and by itself could be readily understood; but, taking it in the connection in which it is presented in the *Reply*, on the page succeeding the account of the *typhoid fever* of the hilly portions of the State, illustrated by quotations from Watson in which the *grade* of fever is referred to merely, as is also immediately preceding by quotations of similar import, this same typhoid fever of the hilly portions of the State, or the “continued fever,” to which we have already referred, which the physicians throughout the southern country are in the habit of calling typhoid fever; we must acknowledge that we are perplexed. After so much talk about “continued fever,” “typhoid fever,” “intermediate grades between the periodical fever of summer and the pure typhoid of winter,” (as well talk of the *intermediate* grades between small-pox and scarlatina, distinct diseases entirely, either of which, however, may present a synocha, synochus or typhus grade,) “typhoid fever, whether *remittent* at the first, or *continued* from the onset,” &c., &c., &c., does Dr. Lewis really, in this “*summing up*,” mean to say that we have no typhoid fever, and that when he says typhoid fever, continued fever, &c., he does not refer to *typhoid fever*, but is speaking all the time of periodical fever?

If any, through the obscure perplexity in which the views of Dr. Lewis on the subject of “typhoid fever,” “continued fever,” “typhoid affection,” &c., seemed involved in the Medical History, may have thought he had at length obtained a dim and distant perception of his meaning, will he not in view of the *explanations and elucidations* given in the *Reply*, be ready to exclaim, “chaos has come again.” And if, as Dr. Lewis states, we did erect an “airy fragile castle” to demolish, will not every one conclude, we labored much to little purpose; for, were there not “windmills” enough prepared to hand, to weary and exhaust the prowess of even the most redoubted? and, if not “enchanted castles,”—“labyrinths” at least,—which an “enchanter” alone could unravel?

Next.—In the Review of the Medical History, we found it necessary to make on this subject of typhoid fever such comments as these:—

“It cannot be questioned, that there is much confusion and some contradictory statements in regard to this typhoid fever of the ‘upland region.’ First, we are told that cases just spoken of as typhoid are not ‘essential typhoid fever;’ and next, after urging the existence of the disease again,—that typhoid fever in autumn ‘does not deserve the name.’”

Again,—“now we do not know whether the Dr. retracts here the opinion just given, that ‘typhoid fever prevails in this section of

the State to some extent at least,' or wishes to be understood as persisting in it, contrary to what he says is the opinion of Chomel in regard to such cases—that they are not 'essential typhoid fever.' Nor, do we exactly understand whether he means to convey the idea that Chomel pronounces a case typhoid fever, and then declares it not 'essential typhoid fever,' or not. If so, he should have referred us to volume and page."

We now quote from the Reply the explanation of Dr. Lewis:—

"Speaking of his own observations, he" (the author, Dr. Lewis,) states that he saw cases of continued fever, attended with bilious vomiting, quick compressible pulse, moist yellow tongue, stupor or coma, &c., &c., in reference to which the following remark is made:—"In these forms of fever, the disease generally lasted from ten to fifteen days, and may be comprehended under that variety described by Chomel, as not being essential typhoid fever." Dr. Boling here wishes to know, if the author intends to convey the impression that Chomel pronounces a disease typhoid fever, and then declares it not essential typhoid fever; if so, says the Doctor, 'he should have referred us to volume and page.' We have not the volume on our table, but will give Bartlett's version of Chomel's account of cases of this description.—Bartlett on Fevers of the U. S, page 107. 'Chomel admits several forms or varieties of typhoid fever, *not depending upon degrees of severity*. These are the *inflammatory*, the *bilious*, the *mucous*, the *ataxic* and the *adynamic*. * * * * * The *bilious* variety is characterized by some yellowness about the lips and nose; a thick, yellowish or greenish coat of the tongue; a bitter taste, nausea and bilious vomiting. Chomel regards this form of typhoid fever, which occurs oftenest, he thinks, during the summer and autumn, and in particular localities, as identical with the bilious fevers of authors.' This corresponds precisely with the variety of fever seen by the author of the Medical History, in the *up country*. Chomel gives it as a variety of typhoid fever, not depending upon degrees of severity. Touching any further information on the subject, we must refer Dr. Boling to M. Chomel himself, our only purpose being to show that this French writer gives to the author all the latitude he has taken."*

* Dr. Bartlett says:—"Chomel admits several forms or varieties of typhoid fever, not depending upon degrees of severity. They are the *inflammatory*, the *bilious*, the *mucous*, the *ataxic*, and the *adynamic*. They depend upon the relative severity or predominance of certain symptoms or groups of symptoms." We are uncertain, of course, whether Dr. Lewis' opinion, as given in the Medical History, is based upon the original authority, or upon the "version" of Dr. Bartlett. If upon the latter however, and if from the words—"Chomel admits several forms or varieties of typhoid fever, not depending upon degrees of severity," he is led legitimately to the inference, that Chomel has, in his arrangement, *yet another principal* form or variety which he terms *essential typhoid fever*, he is, we think, led into error. Having arrived at this conclusion, should he consequently infer, however, that any *one* of these five varieties, "not depending on degrees of severity," is not typhoid fever, or not essential typhoid fever,—for the meaning is, we believe, the same—it would just as much follow, that the other

Now we cannot see that this throws any light whatever on the question. We are not shown that Chomel says, that *any one of these* varieties of typhoid fever, "not depending upon degrees of severity," is not "*essential typhoid fever*." On the contrary, he seems to consider them *all* equally essential typhoid fever. With the same propriety might Dr. Lewis say, that any case of disease, in which, from some coincident circumstance, a class of symptoms common to it was presented with somewhat more than proportionate prominence, was not essentially the disease which he had named it. Chomel does not do this. It is true, however, that Chomel believes that his *bilious* variety of *typhoid fever* has been described by others as *bilious fever*. But this does not imply that *he* considered it not *essential typhoid fever*. An example somewhat in point.—Dr. Bartlett expresses the opinion, in his work on fever and elsewhere, that certain cases of which *we* have spoken as bilious fever, *were cases of typhoid fever*—not, that they *were not* essential typhoid fever. Dr. Lewis, it will be seen, lays much stress on the words, not of Chomel, but of Bartlett—"not depending upon *degrees of severity*,"—but surely this cannot be interpreted into,—"*not essential typhoid fever*"—or, in the same manner, and by a similar process, his other varieties might be excluded also, and then there would be no "*essential typhoid fever*." His bilious variety, it seems, has its name from a prominence of what are called *bilious* symptoms, owing to locality, and the reason of its occurrence. We may presume that his *ataxic* variety is owing to a somewhat disproportionate degree of nervous disturbance; yet, for this reason, it would be scarcely proper to say that it was not typhoid fever; or, if so, apply the rule, (if it would work) to all diseases, and disease would be exterminated. For instance, suppose any other disease—scarlatina for example—occurring in the Autumnal season, in a miasmatic locality, with a somewhat disproportionate degree of gastric and hepatic derangement, as would in all

four varieties also are not essential "*typhoid fever*." We will not pretend to say whether there is anything in the language of Dr. Bartlett or not, which could possibly lead to the inference of yet another principal form—"essential typhoid fever—in the arrangement of Chomel; but, if there is, we are much inclined to think that the views of Chomel are not fairly and clearly given. He divides typhoid fever, ("*essential typhoid fever*," we will venture to call it on the present occasion) *into five principal forms*—the inflammatory, the mucous, the bilious, the nervous and the adynamic. We cannot quote from Chomel himself, but will present his division, as given in the article *Dolhienterie*, in the Dictionnaire de Medecine.

"M. M. Chomel et Genest, dans leur ouvrage sur la fièvre typhoid * * * admettent *cinq formes principales*, qu'ils designent sur les noms de fièvres typhoid *inflammatoire, muqueuse, bilieuse, nerveuse et adynamique*." The author of the article—E. Littré—expresses some dissatisfaction with the division of Chomel and Genest, and proceeds to propose one of his own, in these words—"J'aime mieux *diviser le dolhienterie en forme abdominal, pectorale et cerebral*; y ajouter une quatrième forme, le *forme adynamique*, une cinquième que j'appellerai d'après M. Louis, *forme latent*; enfin, une *forme anthrétique*." Yet not one word by either of the authors, in the division of typhoid fever into different forms, of *another* distinct form,—"*essential typhoid fever*."

probability be the case under such circumstances ; would Dr. Lewis say that this was not *essential* scarlatina? or in the same disease, from its occurring at a later season, or during an epidemic tendency to catarrhal affections—should the catarrhal symptoms happen to be more than usually prominent—would he consider himself justifiable in saying that it was not essential scarlatina? Unfortunately for Dr. Lewis' *explanation* and for mankind, the rule won't work ; diseases cannot be "ruled off" in this way, and man must continue to endure them. If, then, Dr. Lewis still considers himself justifiable, because Chomel divides typhoid fever into five varieties—"in all the latitude he has taken"—in selecting one of them and asserting that Chomel says it is not essential typhoid fever, we think it is but reasonable to conclude, that from anything or nothing, he may consider himself justifiable, in "all the latitude" he may find it convenient to take. Truly it does not seem necessary to give the Doctor even "an inch" to induce him to "take an ell."

We are compelled to make another long extract from the *Reply*, in which we are charged with a most grave literary offence.

"We are here" (says Dr. Lewis) "under the painful necessity of convicting this reviewer of a piece of *garbling*, which for recklessness has no parallel. It is the suppression, not of a distinct and connecting passage, but of one-half of a paragraph, which is inseparably connected with the part Dr. Boling quotes, and is alone illustrative of the author's real views and conclusions relative to the general character of disease during the third epoch. We will first *re-quote from the Review that portion of the passage* which this writer extracts for his purpose.

"'Causes of a prominent nature have been assigned for the change which took place in the character and temper of disease about 1818. Whether those causes were adequate to the production of these new pathological features cannot be ascertained with absolute certainty, yet such a conclusion is plausible, and sustained in part by events in the histories of other and older States. But for the change we are now contemplating, the mind cannot fix upon any tangible or supposed cause with certainty or satisfaction. In many parts of Europe as well as America, it was noticed about this time, that disease assumed an adynamic type. This change, though not so marked elsewhere as in Alabama, was still sufficiently prominent to attract general attention. Watson and other observers, seem to hold the opinion, that it was in some way influenced by the epidemic cholera, which immediately preceded.'

"So far reference is made *only* to congestive fever, and here Dr. Boling *stops*, in the middle of the paragraph. We will now quote the remainder, of the *suppression* of which we complain.

"Owing to the fact that many cases of congestive fever (then a new disease in Alabama) resembled the cholera in some of its symptoms, the same conjecture has been made here. But when it is known that the congestive fever of Alabama is identical with a disease designated by the names 'cold plague,' 'congestive typhus' and 'cold sickness,' that has occasionally showed itself in certain localities along

the Mississippi and Apalachicola low lands since 1820, it becomes obvious that this particular form of disease is essentially indigenous to the country, and that its development in Alabama was owing to local causes rather than any (inexplicable) foreign atmospheric agency. But *this particular form of fever, and all speculation on the subject aside, and it becomes our duty, so far, at least, as unerring testimony can make it, to state, that diseases of every form, season and locality, in the bounds of the State, became at this particular time, characterized by a low enfeebled state of the circulation, opposite that which previously existed; and that even the enlightened and observing surgeon, in cases of injury and violence, had frequently to stimulate and nourish, where but a short time before the most active depletion would have been required."*

We think all who have read the Medical History, must have been forcibly struck with the fact, that the language and the construction of the paragraphs are somewhat peculiar; that in some instances, what would seem the most obvious and natural interpretation, on a minute investigation and analysis, would not be really the strict and literal one; and that occasionally the true meaning is doubtful, or a double meaning conveyed. As an example in point, not however from the Medical History, but from the *Reply*, we may instance the long quotation just made. "We will first," says Dr. Lewis, "*re-quote from the Review*, that portion of the passage which this writer extracts for his purpose." Well, after *quoting*, not from the Review, but from the Medical History—and not as it was presented in the Review—about one-half of the paragraph under consideration, purporting to be as it appeared in the Review however—it will be recollected he makes this remark,—“So far, reference is made *only* to congestive fever, and here Dr. Boling stops in the middle of the paragraph.” Now we request an examination of the part of the paragraph alluded to and above presented; and, as Dr. Lewis says that congestive fever *alone* is alluded to in it, how are we to understand his real *allusions*? Certainly the words “congestive fever” do not occur in the part referred to, and no one, we feel certain, would suppose that it especially and *alone* was alluded to; but “disease,” (which Dr. Lewis here and elsewhere seems to use in the plural number,) in a *general* sense apparently, is the word given. Is not, then, the most obvious and even most strictly literal meaning—may we not say, the only possible interpretation, without a key,”—in this instance, very different from that which Dr. Lewis says he alluded to? and it is *only* in the “suppression,” of which he *complains*, that we can even now discover, that congestive fever particularly and especially *is alluded to*.

We request the reader to recollect, that in the Medical History that, which in the *Reply* (from the intervening remark of Dr. Lewis, that we *stopped in the middle of the paragraph*) seems divided into two separate paragraphs, was but one. Well, in view of the exceeding difficulty, which we have here rendered so manifest, of arriving at Dr. Lewis' *real allusions*, and we have neither *cypher* nor *key* to the History, we will not venture to assert positively, that we may not have placed an interpretation on the paragraph, that was not intended, since he

seems to be so much dissatisfied in regard to it ; though it may possibly strike the reader, that he was rather pleased than otherwise, at the omission of the part, seeing that he has made such struggling efforts to sustain upon it a charge of so unworthy an act as that of *garbling*." However, should those more competent to judge than either Dr. Lewis or ourselves under the circumstances, be of opinion that we have really done the Doctor injustice, or misrepresented his literal meaning, after *we have requoted* from the *review*, what we there gave from the paragraph under consideration, we can only say in vindication, that it was not done intentionally. To use a trite but expressive phrase, "the error was of the head, not of the heart," a venial one we hope, considering the great difficulty of unravelling some of the Doctor's sentences. We repeat then emphatically, we disavow any intention to misrepresent the opinions of Dr. Lewis. We could not but look upon it as an unworthy act, intentionally to misstate an author's views, while to *interpolate*, for the purpose of evading an unpleasant position in which we might in the course of criticism be placed, is a subterfuge, (though a very little one) to which we hope we may never feel inclined to resort. Now it will be seen, that though Dr. Lewis directly tells us that *he will requote from the Review*, what we have there quoted to suit our purpose, *he does not do so* ; but from the Medical History. Had he fulfilled his promise, and given it really as presented in the Review, our object in the omissions would have been apparent to the reader, and the extent of the basis upon which the *charge of garbling* was rested, too palpable.

It is, we believe, one of the privileges, or rather it is a duty of the reviewer, to condense the views of his author, where condensation is possible, without giving a false coloring to, or incorrectly stating them ; to present with as little incumbrance as he can, the real *pith* of the subject. Now as we really did *not* wish to write, as though fame consisted "in filling a certain space on paper," according to what we presume Dr. Lewis intends as an improved revision of the Noble Poet—it was an object, held in mind in the review, to give (when we felt *more than usually* certain that we had discovered the aim of the author,) his opinions in as brief a space as possible. Here *is* what we quoted *into the Review*, and the *form* in which it was there presented. Let it be compared with what Dr. Lewis says *he requotes* from the *Review*, and then with the entire substance of the paragraph in the History.

"Causes of a prominent nature have been assigned for the change which took place in the character and temper of disease about 1818.

* * * * * But for the change which we are now contemplating, the mind cannot fix upon any tangible or supposed cause, with certainty or satisfaction. In many parts of Europe, as well as America, it was noticed about this time that diseases* assumed an adynamic type.

* * * * * Watson and other observers seem to hold the opinion that it was in some way influenced by the epidemic cholera which immediately preceded." Dr. Lewis, however, is inclined to the opinion, that

* It is "disease" in the History.

its "developement in Alabama was owing to local causes, rather than any inexplicable foreign atmospheric agency."

We then proceed to make "a few extracts, in which Dr. Lewis' description of the peculiarities characterizing the diseases of the present epoch is conveyed," and among them, some fifteen or twenty lines further on, we find this—" * * * * * Diseases of every form, season and locality in the bounds of the State, became at this particular time characterized by a low, enfeebled state of the circulation. * * * * *

We could not well quote the entire History, and we cannot see in what way any peculiar views of the Doctor are strengthened by the part omitted. Judging from his remarks, unless there is some hidden or latent *allusion*, which we have been unable to discover, he really seems to labor under some singular delusion in regard to the real import of the part quoted and the part omitted. If there has been "*garbling*" or "*interpolation*" in regard to this affair, who has committed it? Taking all the facts and circumstances into consideration, might we not, with no little propriety, say that this charge of Dr. Lewis, sustained in the *manner*, and by the *means especially*, it is, might afford "argument for a week—laughter for a month—and good joke forever?"

"We will now" (says Dr. Lewis) "return to typhoid fever, or a disease known in Alabama by that name. It is fearfully and rapidly increasing in the South, and it is time there should be some understanding in relation to its diagnosis and true character. Dr. Boling denies the existence of *true typhoid fever in Alabama*."

Dr. Lewis! !—*pro pudor!*"—We confess we have been seldom more surprised or startled than on reading the above remark. In what moment of forgetfulness have we said this? or what carelessness of phraseology have we been guilty of, to admit of such a construction? were questions that at once suggested themselves; for, it did not at first occur to us that an assertion so openly and unhesitatingly stated, could have been made by Dr. Lewis himself, even, notwithstanding the great carelessness of expression at times, as we have shown, pervading his style, except on the most unequivocal testimony. We have searched the Review thoroughly, we believe, and what we have found we will present to the reader, begging that the manner in which the term "typhoid fever" is applied, in the descriptions of Dr. Lewis, to diseases evidently of a distinct and separate character—and the confused manner in which the details of these are further mixed up with accounts that may possibly strike the mind as really having reference to the disease itself named—may be borne in mind. We will "begin at the beginning."

Speaking of the *remittents* of the first epoch, Dr. Lewis says:—

"If, in the neglect so often attending the treatment of non-malignant diseases, these fevers were suffered to continue, they not unfrequently glided into a continued irritative type, attended with great *nervousness*, twitching of the muscles, general emaciation and debility, constituting what in those days was termed 'nervous fever.' We then proceed to remark:—

"We imagine that these *latter* cases, were not unlike many that

occur in the present day. Indeed it is no very uncommon thing, in some parts of the State, to see cases of remittent fever, which at first perhaps were of a mild character, by neglect or maltreatment, assume the appearance here described, and in fact the terms 'nervous fever,' 'typhus fever' and 'typhoid fever,' are not unfrequently applied to them in this stage." It will be seen that the subject of comment here was *remittent fever*, and the conclusion, that protracted cases of this disease, with "great nervousness," &c., were even at the present day erroneously called "nervous fever," "typhoid fever," &c.

We showed in the Review, that at least in one part of the Medical History, Dr. Lewis, in giving his general characteristics of the diseases of the present epoch, had in view *congestive fever* as the principal type or model from whence his description was drawn. In another place, he speaks of "the low stage of *typhoid affection*, that marks the character of all disorders at the present day." In relation to this, in the Review, we ask—

"What does Dr. Lewis mean here by 'typhoid affection?' He cannot, of course, we must believe, have reference to the disease known under the names of the 'typhoid affection,' 'typhoid fever,' 'dothinenteritis,' follicular enteritis,' &c.; for no one who is at all familiar with the diseases of Alabama could be so far led astray, even by a favorite hypothesis, as to discover among our *more prevalent diseases* anything at all answering to the description of this affection. Or, does he wish to convey the idea by the word *typhoid*, that the diseases of the present epoch are characterized by symptoms resembling typhus? If this be the case, we consider the term equally objectionable. Most certainly, in a few rare and exceptional cases of fever which have become protracted by neglect at first, an injudicious treatment, or perhaps an unusual degree of obstinacy in the disease itself, symptoms of a typhoid character do supervene; but, from this small population, perhaps not over one in every two or three hundred cases, it would be incorrect to draw the characterizing features of the diseases of this epoch.

"Further on we shall have to state—deriving the impression from another part of the essay—that Dr. Lewis has assumed the congestive fever as the type of the diseases of the adynamic epoch. How, then, can he reconcile this with the idea of typhus or a typhoid state? * * * * * Certain we are, and all we think will agree with us, that no febrile diseases are more diametrically opposite in the manner of their developement and progress, and in their symptoms generally." These remarks, it will at once be seen, were in relation to the general characteristics or predominant features of the more prevalent diseases of the epoch.

In another part of the Review, again animadverting on what seemed a confused application of the term *typhoid fever*, we remarked—

"We are exceedingly puzzled to know what to make of the above sentence. We find, however, the conclusion—that 'typhoid fever prevails in this section of the State to *some extent* at least,'—a conclusion, which we think, without a clearer case than is *here* made out, he need not have put himself to the trouble of repeating, as he had already informed us, in a quotation from a correspondent, that in 'August, Sep-

tember and October of 1840, in Benton County, typhoid fever prevailed as an epidemic.' "

Again, in the Review, bearing in mind the confusion which seemed to pervade Dr. Lewis' account of typhoid fever, the fact also that we had conversed with physicians, who, from never having themselves seen the disease in Alabama, and from having known the term occasionally applied to other distinct affections assuming a low or typhoid grade—had actually begun to have doubts in regard to its existence at all within the State—we were led into a remark of this character:—

"It cannot be questioned, that there is much confusion and some contradictory statements in regard to this typhoid fever of the 'upland region.' First, we are told that cases just spoken of as typhoid, are not 'essentially typhoid;' and next, after urging the existence of the disease again,—that typhoid fever in autumn does not deserve the name. Is not all this calculated to strengthen the doubts, which many physicians of Alabama already entertain as to the existence of typhoid fever at all within the bounds of the State?"

In another place, again, bearing the same in mind, in referring to Dr. Lewis' descriptions under the term typhoid fever, &c., we make this observation:—

"Notwithstanding the statement of Dr. Lewis, 'that it is a *generally* received opinion among medical men in various portions of the State, that typhoid fever, as described by Bartlett and others, has been an annual disease,' there are many physicians of our acquaintance who are much inclined to doubt whether the genuine typhoid fever has ever appeared among us, and are very positive in relation to this fact, as regards their own particular neighborhood, for such time at least as they may have resided in them. Will these doubts be removed by the lengthened quotations we have made, after comparing them with the accounts given by Dr. Bartlett? * The description, however, of typhoid fever in the 'upland region,' for which we regret that we cannot make room, as given by Dr. Clark and quoted by Dr. Lewis, seems less wide of the mark. Still we are reminded of the fact, that this is the same gentleman, who, on a previous occasion, as we have shown, says—that 'it attacked *indiscriminately all ages*, without regard to sex or color.' "

It cannot but be perceived, that all we have said is not to the actual existence of the disease, but is to the sufficiency of Dr. Lewis' description; and we will now advance one step further in regard to the

* Dr. Lewis seems to think that the admission of the *general* correctness of the description of typhoid fever by Dr. Bartlett must be a most painful one to us, and that because we are willing to take it as a standard by which to judge and measure the general description of typhoid fever as given by himself, it follows that Dr. Bartlett must be our *authority* in every particular in which a question in regard to this disease is mooted. He is certainly, however, mistaken in his supposition, and his inference is by no means a necessary one.

matter than we did in the Review, and state, that we *ourselves would have doubted*, had the proofs of its existence rested upon the accounts given in the Medical History alone. How Dr. Lewis could have made the statement he did, we are at a loss to imagine. We do not like to question men's *motives*, or the supposition might arise, that—being a convenient method—it was for the purpose of bringing Dr. Bartlett in array against us; and we must therefore suppose that it was a result of that *carelessness* and haste, of which we have before given examples, so peculiarly the Doctor's own. We do think, however, that on a calm and dispassionate rehearsal of his *Reply*, the Doctor will be forced, over some of his careless inaccuracies, to the blush, unless indeed he is as much favored and as highly endowed as the exquisite in the play, who congratulated himself on *having courage enough not to be ashamed of anything*."

After adducing very respectable testimony in proof of the existence of typhoid fever in Alabama, Dr. Lewis thus brings the subject to a close:—

"We could extend and enlarge upon this testimony, but it is wholly unnecessary to our present purpose. *Dr. Boling, and the physicans of his acquaintance*, may still deny, if they like, the existence of typhoid fever in Alabama; the *medical public will* sustain the author of the *Medical History*," (the italics are ours,) "and possibly teach Dr. Boling that a mere *opinion*, dogmatically promulgated, will not rule the minds of men, *nor stay the hand of investigation*."

This is terrible—awful! We have nothing to ask or expect for ourselves; but to our unfortunate friends, gentlemen, *do* be lenient; indeed they did *not* dogmatize—they merely *doubted*.

Again we must make a somewhat lengthened quotation from the Reply.

"Dr. Bartlett, with a view to ascertain the geographical limits of typhoid fever, addressed a letter through the New Orleans Medical Journal to physicians of the South on that subject.—January No. 1847. 'Dr. Boling, of Montgomery, Alabama, in an * * * * article on remittent fever, published in the American Journal, speaks of *protracted cases attended with spontaneous diarrhœa and tympanitic distension of the abdomen*, and a feeling of pain or uneasiness *between the umbilicus and right iliac fossa*. He also reports two *protracted cases*, in which he found ulceration of Peyer's gland after death. Now it seems to me *hardly possible to doubt that these, all of them*, were cases of true continued typhoid fever.' To this portion of Dr. Bartlett's letter, Dr. Boling makes an early and direct Reply; it is to the point. We quote from a letter written from Dr. Boling to the Editors of the New Orleans Medical Journal—March No., page 685.—'By the way, to the remark of Professor Bartlett, in the last No. of your journal, page 552—"Now it seems to me *hardly possible to doubt, that these, all of them*, were cases of true continued typhoid fever"—I must answer, that with me there is no doubt whatever on the subject—*emphatically*, they were not such.' The want of courtesy distinguishing this reply is thrown

entirely into the shade by the coolness and confidence with which it is made."

For whatever "*want of courtesy*," there may have been on our part, taking the circumstances into consideration, we take the present opportunity of tendering to Dr. Bartlett an apology. The remark was made at the termination of a hurriedly written letter; and we are free to acknowledge, that the seeming petulance of the words, (the result perhaps of haste) on subsequently having our attention called to them, caused us some regret. Dr. Bartlett's *opinion*, however, in regard to the cases in question, was of course, it is probable, *no more deliberately formed than our own*, while our opportunities for observation in regard to them were somewhat superior to his. We had also the advantage of the reading of his own valuable chapter on the disease which he supposes them to have been. The opportunities for observation on any particular medical question being equal—without acknowledging any Medical Idol, or indulging in the language of fulsome adulation—we may say, that there are not very many of the profession, with whose writings we are familiar, to whose opinion we would much more readily defer than to that of Dr. Bartlett. But, as "none are all evil," we must also believe that Dr. Lewis is mistaken if he supposes that any, *are all perfect*; and it is an acknowledged imperfection of our nature, that our views in regard to any particular question are apt to be influenced somewhat, by preconceived and favorite opinions, in allusion to any subject on which it may have a bearing. Whether we are right or wrong, in the particular instance in question, in excuse for the seeming presumption of opposing our opinion to that of Dr. Bartlett, we may offer, that though he may have seen much of typhoid fever, we have also seen much of periodical fever, perhaps one-half of the cases prescribed for during our entire course of practice, having been of this character—and in regard to the particular cases in question we *did* see them, he *did not* see them.

We will quote at the risk of being tedious, every thing we can find in the essay alluded to, on remittent fever, which we suppose could have influenced Dr. Bartlett's opinion; more especially such sentences as bear a resemblance to the quotation given by Dr. Lewis, from the letter of Dr. Bartlett, in which his opinion is expressed.

On page 8, in regard to the *general* condition of the bowels in remittent fever, we observe:

"Generally speaking, they are easily influenced by the action of purgatives, which even in moderate doses, sometimes irritate the mucous membrane of the bowels very much, * * * * Sometimes indeed the disease is ushered in by a spontaneous diarrhœa, or as this is generally combined with vomiting, a genuine cholera morbus. This is rare, however, but still the tendency to diarrhœa is so strong, and its consequences so injurious, that great care has to be observed all the time, more especially after the first or second exacerbation, in the administration of purgatives."

Page 9, speaking of evacuations, thin and serous, transparent or reddish, containing flocculi of mucus:

"When the operations are of the latter character, they are invariably more frequent during the exacerbations, at which time they are also

smaller in quantity, attended with griping and *considerable soreness of the abdomen.*"

On page 12, "Pressure on the abdomen causes a sensation of pain, always in the epigastric region; in the left hypochondriac region, when enlargement of the spleen exists, *and towards the close of protracted cases*, sometimes an obscure sensation of pain, between the umbilicus and right iliac fossa * * * *

The abdomen early in the disease possesses its natural fulness; after the bowels are evacuated, it generally sinks in proportion to the extent of the purging, *if not carried too far*; but *beyond a certain point, purgation* scarcely ever fails to produce *tympanitic distension*. Sometimes in cases of a *very violent* character, *this tympanitic distension* supervenes early in the disease, under the action of few doses of purgative medicine, and accompanies, almost always, a *spontaneous diarrhœa*, which in a few cases makes its appearance early. In the advanced stages of cases that are at all *protracted, more especially where purgatives have been used freely*, a slight degree of tympanitis attends, and in almost every such case is accompanied with a state of the bowels requiring the restoring action of opiates or astringents."

Page 29, speaking of the use of purgatives in the disease :

"In some cases, however, the bowels having been once freely evacuated, *purgatives* will be no further needed, as one or two evacuations in the twenty-four hours will take place *spontaneously.*"

"The bowels are extremely *susceptible* to the action of purgatives, which, without great caution, are apt to induce much irritation and *hypercatharsis.*"

Page 37. Speaking of the necessity of caution in the use of calomel, "in *protracted cases* where *purgatives*, calomel among the rest, have been used to any considerable extent. * * In such cases, such an irritable state of the intestines will frequently be found, that even in small doses, it is apt to produce griping, and then serous, or bloody discharges."

Page 45, "whenever a *protracted* case, presenting violent symptoms, is met with, the probability is, ten to one, that it was originally a mild case, treated with drastic purgatives &c."

Page 53. "The ileum, more frequently than any portion of the intestinal canal, the stomach and duodenum perhaps alone excepted, presents traces of inflammation, in all cases *increasing in intensity as we approach the ileo-cæcal valve.* The glands of Peyer, to which attention has been more particularly directed of late, in consequence of their diseased condition being considered the anatomical characteristic of typhoid fever, have by different observers been found in different conditions. They are stated by Dr. Bartlett, to "have been found uniformly healthy." He has had reference merely, however, to cases examined by Dr. Graham and Dr. Stewardson, in the Pennsylvania Hospital different times. In September 1843, I examined a case of remittent at fever, an originally mild one, but which by neglect and mismanagement, was permitted to assume a grave form. The case was unusually

protracted, commencing August 7th, and terminating fatally on the 17th of September. * * * The small intestines presented slight traces of inflammation, throughout nearly their whole extent, increasing in intensity in descending to the ileo-cæcal valve. The *elliptic plates* were of a much darker color than the rest of the mucous membrane, granular to the eye and touch, the margins well defined and slightly elevated, and some of them presenting distinct traces of ulceration.”*

In another case of a negro girl, terminating fatally on the 9th day August, 1844, examination five hours after death, it is stated, that there was “considerable inflammation of the small intestines; elliptic plates of ileum elevated, granular to the touch and darker than the rest of the canal. Solitary glands much enlarged, many of them ulcerated, the margins hard, and the edges of the ulcers abrupt; * * * In this last case *diarrhœa* existed, in the former *not*. These are the only instances in which I have observed any morbid alteration of the elliptic plates.

In these observations, we were stating facts merely, and exactly as they presented themselves to us, and we think it must be admitted, without the least bias whatever, from any *favorite view*, for that we had none is sufficiently evident, we had neither “made up nor expressed an opinion.”

Dr. Bartlett remarks: “Dr. Boling of Montgomery, Alabama, in an article on *remittent fever*, * * * speaks of *protracted cases, attended with spontaneous diarrhœa and tympanitic distension of the abdomen, and a feeling of pain or uneasiness between the umbilicus and right iliac fossa*. He also reports *two protracted cases*, in which he found ulceration of Peyer’s glands after death.”

Dr. Bartlett has, (unintentionally we are sure,) the impartial reader will, we think, acknowledge, made out a much stronger case, than our language will justify. Thus in but *one* of our cases did we discover any *ulceration* of the elliptic plates, or Peyer’s glands; while Dr. Bartlett makes us say that this condition was present in both. The words “protracted cases,” “spontaneous diarrhœa,” “tympanitic distension of the abdomen,” and on pressure, “towards the close of protracted cases, sometimes an obscure sensation of pain between the umbilicus and right iliac fossa,” too, it will be perceived, are separated from their proper connections, and placed in juxtaposition, so as to convey a much more complete picture of what was *uppermost* in the mind of Dr. Bartlett,

* Dr. Bartlett, in a note, page 337, of his work on fevers, states: “Since writing the above, I have had a correspondence with Dr. Boling in regard to the most striking of these two cases. His letter to me contains nothing to induce me to change the opinion already expressed in regard to the true character of these cases.” We have *not* “argued the case,” however, as is stated by Dr. Lewis in his reply. *In reply* to a letter of inquiry from Dr. Bartlett, we briefly stated some of the points upon which our opinion in regard to the nature of the case was founded, but without either attempting, or expecting to convince him. We intend no “want of courtesy” to Dr. Bartlett in stating on the present occasion that neither in anything connected with the correspondence, nor in the recent edition of his book, have we been able to find anything to induce us to change our views “in regard to the true character of these cases.”

than the different expressions, as they stand in their own proper relations, could possibly do. Thus, although we do speak of "protracted cases of remittent fever, (all admit that such may occur) "tympanitic distension of the abdomen and even in connection with a "spontaneous diarrhœa," which sometimes in cases of a very violent character supervenes early, and also of an "obscure sensation of pain between the umbilicus and right iliac fossa," *we do not* say anything of "protracted cases *attended* with spontaneous diarrhœa, and tympanitic distension of the abdomen, and a feeling of pain or uneasiness between the umbilicus and right iliac fossa." We can perceive nothing in our language, to warrant the bringing together of these different fragments of sentences, in the array in which they appear, from the pen of Dr. Bartlett. If *all* the cases in which any of the phenomena alluded to, in the isolated manner in which in our statements they appear, are cases of typhoid fever, then indeed is the disease of most extensive prevalence. If, because sometimes, towards the close of a protracted case, an obscure sensation of pain is felt on pressure between the umbilicus and right iliac fossa, or if because *early* in an attack, of a *very violent* character, *tympanitic distension* supervenes under the action of a few small doses of purgative medicine, or, if because it may accompany a "spontaneous diarrhœa which in a few cases makes its appearance early," if we repeat, from the occurrence of any one of these circumstances, "it is hardly possible to doubt," that the case in which it is presented is one of "true continued typhoid fever;" there are few cases indeed, occurring in any miasmatic locality, but of "true continued typhoid fever," more especially, under the more general purgative and mercurial treatment, of a few years back. But we will proceed to consider the more important question,—the condition of the elliptic plates. The *first part* of the book of Professor Bartlett is on typhoid fever; relative to the lesions of which he says:

"In all cases of typhoid fever, there is lesion of the small intestines. *This lesion is peculiar; it is found in no other disease.*

"The invariable and characteristic lesion found in the small intestines, to which allusion has been made, consists in alterations, differing somewhat in different cases of the *elliptic plates*, or *Peyer's glands*."

Now, if it should appear that the opinion of Dr. Bartlett, as here *made up and expressed*, that a morbid alteration of the elliptic plates is peculiar to typhoid fever, occurring in no other disease, has been conclusively and definitely settled and decided, of course we will yield our opinion, and admit that though we looked upon our two cases, in which these were found apparently altered, as cases of remittent fever, they must of necessity have been cases of typhoid fever. If, however, we have misunderstood the proposition of Dr. Bartlett, as above laid down, which amounts to this that *any* alteration of the elliptic plates will answer for the anatomical character of typhoid fever, and that these are *never* altered in any other disease, and his meaning is, that the alteration itself, of the elliptic plates in typhoid fever, is one of a peculiar character, with the admission that other morbid states of these plates, differing from this *peculiar* one of typhoid fever, may occur in other dis-

eases; then we will merely remark, (leaving the question of identity, to be settled by others) that *we have not said* that the condition of the plates in our two cases, was the same as that occurring in typhoid fever, (certainly they differ very materially from the appearance presented by these parts, in a case examined, since which we considered typhoid fever, and which at least differed very distinctly from miasmatic fever, and presented a very dissimilar train of symptoms during life, from either of the cases under consideration) and it is admitted that an alteration of these plates may possibly now and then occur in other diseases, why not, among the number, in remittent fever? Now most respectable authors do speak of an altered condition of the elliptic plates in other diseases; and, farther on in his book, Dr. Bartlett himself speaks of it, in these words:

"In typhus, they" (the elliptic plates) "are very rarely altered and when so at all, only to a very trifling extent." * * * "The diseases other than typhoid fever, in which alterations of the intestinal glands are most frequently found are: tubercular phthisis, scarlet fever, and certain forms of cholera." The condition of the glands, he goes on to say, is different from what it is in typhoid fever. Still we presume it must be considered a morbid alteration. "As to scarlet fever, and some forms of cholera," (he proceeds) "it is enough to say that the only changes of the intestinal glands observed in these diseases, have consisted in a *moderate thickening with or without redness and softening of the follicles.*" An alteration then of the elliptic plates does occur in other diseases than typhoid fever; and if in cholera and scarlatina, why not now and then possibly in remittent fever; a disease, which, in the South at least, as much, or more than any other, we might almost venture to say, is characterized by a tendency to morbid action in the gastro-intestinal mucous membrane? And, if in the two diseases named, which when fatal generally prove so in a *short time*, "a *moderate thickening, with or without redness and softening of the follicles*" is sometimes presented, it is not difficult to suppose, that in a case of remittent fever of about forty days duration, (admitting of course the possibility of an alteration of the plates in this disease at all) "distinct traces of ulceration" might be superadded. This indeed would seem but the natural advance of morbid action, with the advance of time. If in the work of Dr. Bartlett, the lines of distinction are clearly and exactly drawn, between the change in the elliptic plates, constituting the chief anatomical characteristic of typhoid fever, and the alteration of the elliptic plates, which he does admit occurs in other diseases, we have not been able to find it.

In regard to the alteration of the elliptic plates in typhoid fever, Dr. Bartlett observes, "we can hardly hesitate I think in attributing this lesion to inflammation. We know nothing of any other morbid process, that can produce similar results." * * * "But * * * it seems to me, that all the analogies in pathology tend to show, that this inflammation is not common, but specific." This conclusion will be considered as holding good, we are inclined to think, only in view of the distinct and separate character of typhus and typhoid fevers. Admitting their identity, seeing that, in that case, in so large a proportion of the whole number, no

traces of this morbid state of the plates is presented, "all the analogies in pathology" rather incline to an opposite view; that the inflammation is not specific, but common, produced by circumstances of which we as yet know but little, if anything; in which something connected with the locality, diet, mode of life &c., may have an influence. It is our own opinion, or rather we are, from the perusal of the book of Dr. Bartlett more especially, inclined to the belief, that these are in reality two distinct and separate diseases. Admitting this opinion, in favor of which Dr. Bartlett certainly makes out a most plausible case, *there is another marked example on record of the occurrence of a morbid condition of the elliptic plates, besides those to which we have already alluded, presented to us, too, in the work of Dr. Bartlett; provided any reliance whatever is to be placed on the symptoms presented during life, as diagnostic*, when the post mortem appearances do not square exactly with what they should, according to our preconceived views. If in other words we are to be governed at all by the symptomatology of a case in regard to its character, and not entirely by the appearances we may discover after the death of the patient. The example to which we have allusion, was an epidemic typhus fever, which prevailed at Rheims, between October 1839 and April 1840; the Historian, M. H. Landouzey.

We will give a brief abstract, from the pages of Dr. Bartlett himself, of the more prominent features of this epidemic. He himself observes of it:

"There are some circumstances, which render the history of this particular epidemic of *great value*. I will mention only one, and that is, that all the cases came under the observation of the medical attendants, immediately on the commencement of the disease."

"*In every case except the first, which was not carefully examined, there was an abundant cutaneous eruption, consisting of small spots, or ecchymoses, as M. Landouzy calls them, of a red violet, or black color, not elevated above the skin, and not disappearing on pressure.*" * * * *

"*They were abundant and confluent, in proportion to the gravity of the disease. The bodies of the sick exhaled a strong offensive odor, resembling that of mice.*" * * * *

"*The contagious character of the epidemic was very manifest.*" * * *
 * * * *None of the persons who had typhus fever in 1814 were attacked with the disease, but two medical students and one physician, who had had typhoid fever, the former six months, and the latter twenty years previously, suffered very severely.*"

"*Meteorism and abdominal pains were uniformly absent. There was diarrhœa, at the beginning of the disease, in only four cases. In all the others, there was no apparent disturbance in the functions of the intestinal canal. The bowels were more inclined to constipation than to looseness.*"

* * * "The spleen was *not* increased in size in *any* of the cases; in four it seemed somewhat softened." The number of cases was 138.

"Many," (Dr. Bartlett remarks) indeed most of the foregoing circumstances, in the history of this local epidemic, correspond to the pheno-

mena which we have found in typhus fever. But according to Mr. Landouzy, in the six autopsies, which were made, the intestinal lesions characteristic of typhoid fever, were present. The elliptic plates were either thickened and elevated, or they were the seats of ulcerations; and the mesenteric glands corresponding to them, were enlarged."

It is an exceedingly difficult matter, to avoid the exercise of an undue influence over our mental operations, by preconceived and fondly cherished opinions. Thus, notwithstanding the most laudable efforts, apparent to the most cursory reader, on the part of Dr. Bartlett, for the avoidance of anything of this kind, the conviction will force itself upon his readers, that he looks upon every thing connected with an altered state of the elliptic plates, through a *typhoid fever lens*; and though this as yet, is not of sufficient focal power, to make typhoid fever cholera, small pox and scarlet fever, when presenting after death an altered state of these plates, we may not unreasonably expect in a next edition, in explanation, an invocation of a commingling of the causes of typhoid fever in all such instances, with the causes respectively of these other affections. This inherent imperfection in our nature indeed requires that while we should not in any case, "requiring careful observation and rigorous analysis, trust in any, the slightest degree to incomplete, inadequate or equivocal facts," the most watchful caution should be observed that none are set aside, *merely* because they do not happen to have a favorable bearing on doctrines of which we may have become avowedly the advocates. Thus of this epidemic of typhus fever, —most clearly marked during life, by the distinctions which he himself has pointed out, from typhoid fever—Dr. Bartlett speaks as a "*supposed epidemic typhus fever.*" Again he says—"In this epidemic, *if entire reliance is to be placed upon the observation of its historian,*" &c., presenting no circumstance, however, investing anything connected with it in doubt, while it will be remembered that there were circumstances connected with it, which gave to the historian the very best opportunities for observation, and a faithful and minute delineation of its features." Again, in a suggestive way, in regard to it he says,—"*Is it possible, that, even admitting the two diseases to be essentially dissimilar, under certain circumstances, the causes of both may be so commingled as to give rise to a mixed disease, in which there is a combination of the elements of both?*"—And yet, if Mr. Landouzy had the book of Dr. Bartlett before him, and had tried *from the distinctions which he has there drawn*, to present a perfect picture of typhus fever, with an exclusion of every possible circumstance and symptom pertaining particularly to typhoid fever—the *post-mortem appearances alone excepted*—he could not have well been more successful, in producing an exclusive and pure delineation of typhus fever, than he has been.

"Setting aside," (says Dr. Bartlett, in his book,) "as I do for the present, the true typhus fever, *there is no disease*, more readily and positively recognized, than a case of well-marked typhoid fever, of extreme or even average severity, when observed from its commencement and followed through its entire course. *It is hardly possible to confound it with any other affection. There is no other in any considerable degree resembling it.*" And yet could it be believed?—subse-

quently, we find Dr. Bartlett, not merely admitting the possibility of confounding it with *any* other disease, but, *under the potent sway of the elliptic plates*, actually asserting that it *has* been mistaken for one of such dissimilar and strongly marked features as yellow fever.

In the New Orleans Medical and Surgical Journal for September, 1844, Professor Harrison, of *New Orleans*, in a paper on *Yellow Fever*, speaking of the post-mortem appearances, says,—“In some cases of a *typhoid type*, in which there existed before death a low nervous delirium, we found sometimes ulceration, and at others hypertrophy and softening of Peyer’s glands.” Dr. Bartlett, in animadverting on this, very unhesitatingly, and with the utmost complacency, remarks,—“*these were unquestionably cases of true typhoid fever.*”*

In the paper just referred to, we learn that Professor Harrison had at the time been engaged for thirteen years in the practice of medicine in the city of New Orleans—had been connected during ten years of that time, with the great Charity Hospital of the place, and had made or had assisted in making several hundred post mortem examinations. If the physicians of *New Orleans* cannot tell what *yellow fever* is—if, in a practitioner and teacher of medicine in *New Orleans*, with the advantages for observation just spoken of, the qualifications necessary for distinguishing a case of *yellow fever* may not reasonably be expected, on whom could we reasonably rely, to tell us what typhoid, typhus, or any other fever is?

Dr. Ames exhibited, at a recent meeting of the Montgomery Medical Society, a pathological specimen, relative to which he furnishes the following particulars, with the privilege of publication :—

“A black child three years old, who had not made any complaint previously, was attacked in the afternoon with very acute pain in the abdomen, and some pain in the head. Some fever occurred during the night. On the morning of the next day there was decided fever, with pain in the abdomen and head; at 12, M., convulsions, and at 5½, P. M., death. This imperfect history was obtained from the child’s mother. When I visited him, he was in articulo mortis; the symptoms present being those only of approaching death, which took place in a very few minutes.

“An examination was made 20 hours after death. * * * * * In the abdomen, a portion of the lower part of the ileum about a foot long *was impacted with lumbrici*, and appeared much injected with red vessels when viewed externally. This part after being opened and carefully washed, was found to contain *several diseased elliptic plates*, which corresponded in appearance very nearly with the alteration described by Louis, as the second stage of the first kind of morbid change which these glands undergo in typhoid fever. *They were red, distinctly elevated above the adjacent mucous tissue, granular or mamelonated; and the mucous membrane covering them was thickened and softened.* No note

* Dr. Bartlett seems generally exceedingly well satisfied with himself, and now and then, really some of the most amazing specimens of cool “I am Sir Oracle-”ism, with which we have met, drop from his pen.

was made of the condition of the rest of the mucous membrane. There was no abnormal appearance in any other part of the alimentary canal. The mesenteric glands were not altered.

In connection with the above case, we will give *from memory* some of the particulars of a case (being unable to find the notes) which, with Dr. Baldwin, we examined some two or three years ago :—

A negro child, about *ten months old*, having previously appeared *slightly indisposed for a day or two only before*, was attacked with violent convulsions, attended with slight fever, which proved fatal in a few hours. On a post-mortem examination, a *number of long round worms were found in the ileum*. The glands of Peyer were slightly but distinctly elevated, and darker than the rest of the canal. Many of the solitary glands were enlarged, others deeply ulcerated, and corresponding with one there was an entire perforation of the canal, through which one of the worms had partly passed into the peritoneal cavity. The examination was made about an hour after death.

We will give no name to either of the above cases. In the latter, the alteration of the solitary glands was much more profound than that of the elliptic plates. It is not unreasonable to suppose, however, that had the case not terminated in so short a time, ulceration would also have been set up in the latter; the causes capable of producing it in one having power, it is presumable, to produce it in the other; the two sets so far as is known, we believe, being functionally and, except in the degree of aggregation, anatomically the same.

We shall present another case, taken from a note from Dr. Baldwin :

"On the 23d of June, 1844, I was called to see Caroline—a negro child, aged about 18 months. She had been sick about four days, with an attack of measles, complicated with worms. She died on the 27th day of the month, being about the 9th day of her disease. I made an examination of the stomach and bowels only. The mucous coat of the stomach was slightly injected in its more dependent portions. The mucous coat of the small intestines was also considerably reddened in patches—the canal containing quantities of a viscid secretion, of an orange color *and several lumbrici*. The elliptic plates and solitary glands were much thickened and considerably elevated above the surrounding mucous membrane. This was more especially the case in the lower part of the ileum, becoming more marked as we approach the ileo-coecal valve."

Worms are a common complication of remittent fever in the South, more especially among the negroes.

It may, perhaps, not be considered "*selons les regles*," for a party to vouch for witnesses presented by himself. This much at least, however, we may venture without impropriety to state—that Dr. Ames and Dr. Baldwin are both in the habit of making and witnessing post-mortem examinations, and are gentlemen of discrimination and sound judgment: and, that it is the common practice with the physicians of Montgomery generally, to permit no opportunity for pathological investigation to pass unimproved.

We are permitted to make the following extracts from a manuscript

now in preparation for the press, by Dr. Ames, on an Epidemic Cerebro-spinal Meningitis, which has recently prevailed to a considerable extent in Montgomery.

"In four cases there was an alteration in the agminated solitary and mesenteric glands, which, on account of the controversy regarding the peculiar circumstances under which alone such lesions have been supposed to occur, became a subject of particular attention and interest, and seem to require here a somewhat detailed notice."

"CASE 1. T. K. White, male, 19 years old. Attack, sudden. Death on the second day.—Ex. six hours after death. A part of the ileum next to the coecum was taken out and washed. The mesenteric gland near this part of the gut, as well as some distance above it, were enlarged and red, but not perceptibly softened. The internal surface had a deep red color imparted to it by the injections of the sub-mucous cellular tissue; the vessels forming a beautiful arborescence under the elliptical plates, where they were most numerous. On raising portions of the mucous tissue its color was found to be rather a pale red. There were three elliptical plates in the part examined, all of which were elevated above the adjacent surface half a line or more; their surface covered with prominent granules, which roughened the surface to the touch; and the cellular tissue beneath was thickened, apparently to the extent of the elevation, and softened. The lower one of these plates was more deeply colored and more elevated than the one immediately above it, which was itself more altered in these respects than the third. A great number of the solitary glands were also enlarged and elevated."

"CASE 2. J. H. White, male, aged 12 years. Attack, sudden. Death on the fourth day. Ex. twenty hours after death. The mesenteric glands were enlarged and red. About four feet of the lower ileum were taken out and washed. The mucous membrane was more or less reddened, softened and thickened throughout this part. There were found in it eight of the agminated, and a great number of the solitary glands, in a diseased state. In all of them the elevated and granular aspect was very obvious, but not equally so in all. The difference, however, did not offer a regular gradation of changes from below upwards. Those most altered were, as in the preceding case, nearest the colon, but next to these there were others which were less altered than those farther up. Under the glands the cellular tissue was thickened and softened."

"There were no symptoms of abdominal disease in either of these cases."

But to return for a moment to remittent fever.—It would be a useless consumption of time, we are sure, to adduce proofs that diarrhœa, coming on in the beginning of an attack or later in the disease—sometimes spontaneous, sometimes seemingly the result of the irritant effect of remedies—may exist in intermittent fever; or that tympanitic distension of the abdomen may now and then occur. Nor will it, we are confident, be denied by any, that an attack of remittent fever *may*, in some instances, be protracted to several weeks, though as it is now treated generally throughout the South, its duration seldom exceeds a few days. To those, also, who are aware of the numerous and varied

organic lesions, developed in the progress of an attack of remittent fever at all protracted, it will not seem improbable that inflammation of the ileum should now and then occur, and consequently that an obscure sensation of pain should be induced by pressure on that part of the abdomen in which this intestine is lodged. Admitting, then, the possibility of *protracted* cases of remittent fever, that in these inflammation of the mucous membrane of the ileum may occur, (and we venture the assertion that few cases prove fatal without it,) does it seem improbable, that during this protracted inflammatory condition of the mucous membrane of the ileum, a morbid state of the elliptic plates and solitary glands, seated in, or covered by this mucous membrane, should be induced, or that this should occasionally result in relaxation? On the contrary, is it not in accordance, indeed, with what we know of morbid processes, that, under the circumstances, such in reality should be the case? are not all the analogies in pathology in favor of such a supposition? Can we indeed readily conceive it possible, for a mucous membrane, to continue for a lengthened period in a state of inflammation, without the follicles, covered by or seated in it, constituting it, would seem indeed a part of it, undergoing any alteration? We wish it to be understood, that we are not contending for an identity of the morbid condition of the elliptic plates, as presented in typhoid fever, with the morbid condition of these plates sometimes accompanying other diseases, including remittent fever; but that in the latter they are now and then found morbidly altered to such an extent as is implied in the description of the post-mortem appearances in the two cases to which we have principally had reference in these remarks.

We are not alone in our opinion, and what we have attempted to prove neither improbable nor unreasonable, others also have actually observed.

In a number of the new York Journal of Medicine, for 1841, cases are reported which the physician of the New York Hospital, Dr. Richardson, took for remittent fever, in which the elliptic plates were found in a morbid condition. No one, we must think, with whom anything but a post-mortem diagnosis is relied upon, in examining the cases, will say that there could be any possible doubt in regard to the true character of some of them at least, so unequivocal were the symptoms, and the attending circumstances and history leading to no other influence. By Dr. Condie, in a note in Watson's Practice, several authorities, among the number Drs. Giddings of Charleston and Harrison of Cincinnati, are cited as having observed the lesion under consideration in this disease; and in a review, in the American Journal of Medical Sciences for April, 1844, of the Medical History of the Expedition to the Niger, &c., we find the following stated as among the morbid appearances after death from the remittent fever of the western coast of Africa, as observed by Dr. McWilliam.

In the lower portion of the ileum—"a series of small ulcerations were seen in four cases. In one the membrane was thickened, rough, and the ulcerations had nearly perforated the bowel; this case proved fatal by terminating in dysentery. *The agminated glands of Peyer were distinct and enlarged in these cases.*"

Professor Dickson, late of Charleston, though he does not mention especially the condition of the elliptic plates, as presented after death from remittent fever, does speak of "follicular ulceration," not stating, however, whether in both the aggregate and solitary glands, or either alone. "The mucous membrane of the stomach and intestines is highly injected," (he observes,) "in severe and short attacks. *In more protracted cases, follicular ulceration* may be found throughout the whole extent of the bowels."

Other authorities also might be adduced, and like the above, of such respectability and standing, *that we know not to whom with propriety we could look for an authoritative reversal of their decision.*

The next subject, which, following the order of the *Reply*, it becomes necessary to consider, is that of congestive fever. It is a favorite one with Dr. Lewis; for, of the forty pages of the *Reply*, some eighteen or twenty are devoted to it. The entire description of the disease, not omitting the "remarkable colluvies," as it appeared in the Medical History, is here, we believe presented again, to which has been added some new matter of the Doctor's own, as well as certain portions from other sources, with new *cullings* from newly *called* up* correspondents. Although with the new material we are not in any manner bound to have anything to do—our legitimate task on the present occasion being merely an examination of the *Reply*, to ascertain to what extent the complaints of Dr. Lewis, of the Review of his Medical History, were well-founded, and how far injustice has been done him, as his views in the latter are presented and sustained—still, as the subject is one of interest and importance, to such parts as we find striking or peculiar, we may, or may not bestow a passing notice, as the humor may take us at the time.

The subject is thus commenced:—

"At the same time that it is impracticable to follow Dr. Boling very closely in his winding and uncertain path, it is difficult to get a fair and direct issue with him; we will endeavor, however, not to misrepresent him."

Now, how unkind and ungenerous it really does seem in Dr. Lewis, to lay the blame of the "winding and uncertain path" on us, recollecting, as he must, that we followed him closely, page for page, scarcely deviating a moment from the direct track, through the entire Medical History, and that we offered, in the Review itself, this fact in "explanation of the *desultory* character of our notice."

We continue the paragraph:—

"In his comments on that portion of the Medical History devoted to congestive fever of the prairies, he seems to find no positive objection to the general description given of that disease, but assents with that recklessness and unreflecting confidence which distinguishes his writ-

* Is it ever from the "vasty deep" of his own fertile imagination—giving thus, to "airy nothing a local habitation and a name?" We could at least wish to be honored with the acquaintance of that "correspondent from Montgomery—that is—if "substance could be call'd, which shadow seemed."

ings, that he is unable from these descriptions to discover the distinctive phenomena characterizing this disease. The physicians of Alabama will doubtless be surprised to learn, that there is a medical writer—a critic, forsooth, living and practising in the South, who openly declares that there are no ‘symptoms pathognomonic of congestive fever,’—a disease, which, of all others, is most peculiar and easily diagnosed. For this extraordinary conclusion it is difficult to account,” &c.

Dr. Lewis will not find the *physicians of Alabama*, at present, as easily *surprised*, we are much inclined to think, as he supposes; for if we are not greatly mistaken, he has already prepared them to receive from him, without astonishment, some very curious assertions. It will be recollected that he promises to *endeavor* not to misrepresent us, and as a matter of course, we must suppose that he has made the promised effort. But to what purpose? Will others have any difficulty in discovering the issue? We might perhaps have objected (did we not, indeed?) to some of the points in his “general description,” had the question been in regard to its general correctness. But attention was directed to the point at issue; and those who have read the Review will probably remember that instead of saying *we* were unable to discover the pathognomonic symptoms of congestive fever, that there were no “symptoms pathognomonic of congestive fever,” we merely stated, and sustained, we think, our statement most incontrovertibly, *that Dr. Lewis had failed* to point out the difference between what he said *was congestive fever*, and that which he said *was not* congestive fever; that though he had led us to believe that he would designate the “vital or important derangement of the functions,” and the “symptoms pathognomonic of congestive fever, distinguishing what he calls congestive fever, from that which he asserts is not congestive fever, he had entirely failed to do so. Here was the point—this the “issue.” Dr. Lewis found it *difficult* to “*get a fair and direct issue with us*,” but out of our demonstration, that he had failed to present us the promised distinction above referred to, makes—“Dr. Boling says there are no symptoms pathognomonic of congestive fever.” It really must have been a somewhat difficult matter with the Doctor, to get up this *issue*. He perhaps hesitated over it a moment, and would have preferred the real one, could he have discovered it; but it did not suit him. Now, on the contrary, we do not hesitate to say here, that which is called congestive fever, as well as all the other forms and grades of periodical fever, are of tolerable easy diagnosis.

We will present before our readers our opinions, as expressed in the Review, that they may judge to what extent, in view of the interpretation he has put upon them, Dr. Lewis has been successful in his *promised endeavors* not to misrepresent us. It will be recollected, however, that according to Dr. Lewis, congestive fever is a disease of a distinct character, and not, as is maintained by, we believe, the great body of the Profession in the South, merely an aggravated or modified grade or form of our ordinary autumnal periodical fevers; and it will be seen, as we have stated, that the question was, as to whether Dr. Lewis had succeeded in pointing out certain promised distinctive characteristics, between what he called congestive fever

and what he asserted was not congestive fever ; and not as to whether the disease, or form of disease called congestive fever was difficult or easy of recognition.

We now quote from the Review. Commenting on a position of Dr. Lewis, we observe :—

“Dr. Lewis here for the first time hints at a position, which further on he enters upon more fully, to wit ; that there is a radical and total difference between an ordinary autumnal fever and congestive fever, or as he terms it, “the fever of the prairies ;” indeed that the latter is a distinct and separate disease—a disease *sui generis*, and not, as seems to be the more general impression among physicians, a mere modification of the former.”

“As we look upon the determination of this question as of no trivial importance to the Southern physician, we must beg the indulgence of the reader, should we dwell more especially upon all that the Doctor has to say upon the subject ; as, seeing that he confidently and boldly takes the position named, we may reasonably expect that he has cogent reasons, if not the most absolute proofs, to advance. At present he promises us that, “*the difference between the two diseases will become apparent when we have described those of the prairies.*”

Again, in reference to remarks of Dr. Lewis, involving this position, we say :—

“A question of no little importance is involved in the preceding remarks. It is, whether there really exists a disease in Alabama, distinct and separate in its nature from our ordinary autumnal diseases, more especially deserving the name of congestive fever, and radically differing from the various aggravated shades of the former, which has been described frequently and spoken of under the latter term.”

In another place, referring to some of Dr. Lewis' attempted or promised distinctions, we remark as follows :—

“We will look forward to Dr. Lewis' explanation of this “vital or important derangement of the functions,” with some interest, as he of course bases his distinction of the two diseases mainly upon it, and intends to show that such exists in the congestive or “prairie fever,”—different in character, and not merely different in degree, from that existing in the other forms of autumnal disease. Now so far as we may judge from what has already been said, the conclusion might readily be drawn, that the difference is not of a radical nature, but of degree merely. * * * * Thus, farther, the Doctor has given us no reason to believe that what is called congestive fever is not a mere modification of our ordinary autumnal fevers.”

And again, under similar circumstances, we say :—

“We are not a little pleased with the conclusion of this paragraph, for now we have reason to expect, that, in addition to an explanation relative to the “vital or important derangement of the functions,” formerly alluded to, as existing in *the* congestive fever, and not in other forms of malarious fever, we have also the hope held forth, that we

are to have the *pathognomonic symptoms* of the congestive fever given us, and then, of course, if in reality there exists the distinction for which Dr. Lewis contends, it will be made clear and manifest."

It will be recollected, that Dr. Lewis in referring to a paper of our own on remittent fever says, that *it contains no account of congestive fever*. After giving in the Review a few quotations from Southern writers, who supposed when writing that they were describing congestive fever, we proceeded to contrast our own account of what we supposed when writing to be congestive fever—asserted by Dr. Lewis, however, not to be such—with his own account of the disease; but before doing so, make the following remark in regard to the comparison:—

"Now should it appear that the description of Dr. Boling corresponds in all important points with the statements of Drs. Wharton and Perry* and others, who have supposed they were describing congestive fever; and that the account of the disease by Dr. Lewis differs in no essential feature from these, we think it will be but a fair conclusion, not only that there is a more general and correct accordance among physicians as to what constitutes congestive fever, than Dr. Lewis would lead us to believe, but also, *that he has also failed to make out his position*—that there is a congestive fever of a distinct and separate character. If Dr. Boling describes none such, and we should be able to discover no important difference, no constant diagnostic symptom, in the account of the disease by Dr. Lewis, distinguishing it from the disease described by Dr. Boling, surely he describes none such."

After presenting our own imperfect account of the disease, we proceed to place before the reader extracts from the accounts given by Dr. Lewis from his correspondents, prefacing them with this observation:—

"We will now present the reader with extracts from accounts of congestive fever, from several of our author's "correspondents," which he himself quotes as descriptive of the genuine disease, that an opportunity may be afforded of detecting if possible the difference *between this, and that which Dr. Lewis says is not congestive fever*."

After giving the quotations³ we remark:—

"Having failed so far in discovering in the cases and views of the 'correspondents' of Dr. Lewis, as presented by the Doctor himself, in his essay, any distinctive phenomena characterizing any form of disease spoken of, as peculiar and separate from those various modifications and shades of our autumnal fever, recognized generally as congestive, entitling it to be considered a distinct disease, we turn to the Doctor's own remarks, and will transcribe verbatim the only case given from the records of his own practice, with the hope that here, at least, we may be able to discover the 'vital or important derangement of the functions,' and the 'symptoms pathognomonic of congestive fever,'

* Which we had just quoted.

with which he has intimated that we are to be favored—*distinguishing the congestive fever of Dr. Lewis* from the forms which others have heretofore spoken of, and treated under that name.”

After presenting the case we observe :—

“We acknowledge our incompetency, in the case given, to discover the ‘pathognomonic symptoms,’ *distinguishing it* from the modified forms of remittent fever, which have been denominated congestive fever by others, and which may be traced in every degree, from the most simple remittent to the most malignant form, passing into each other by imperceptible shades,—unless, indeed, we admit that one is presented in the last sentence quoted.”

Of this sentence we subsequently made what seemed to us a proper disposition. These are the words :—

“Action of the heart changed to a tumultuous flutter.”

Further on in the Review, in reference again to this attempted distinction of Dr. Lewis, between what he said *was not* congestive fever, and his own account of congestive fever, we use this language :—

“We must reiterate our belief *that Dr. Lewis has entirely failed* in pointing out one single diagnostic symptom—one single particular in *which the two forms of disease differ, save in degree*. We have not so far failed, nor shall we in the conclusion of our task fail to present to the reader anything in the paper which may seem to have a bearing in favor of *the Doctor's opinion* ; and if it shall be apparent to one even, of our readers, that *his position is made out*, or its correctness rendered even probable, by the enumeration of the ‘symptoms pathognomonic of congestive fever,’ or a connected chain of argument, we will cheerfully submit,” &c., &c.

It will of course at once be evident that all this has reference, not as to whether there are any symptoms pathognomonic of congestive fever, but as to whether Dr. Lewis had succeeded—by pointing out “the symptoms pathognomonic of congestive fever,” as he had intimated he would do—in establishing and making out the difference between what he said *was* and what he said *was not* congestive fever. If, in the Review at all, we have asserted that there are no symptoms pathognomonic of congestive fever, the assertion must be contained in the paragraphs which we have above quoted ; and merely that an opportunity may be fairly presented of judging, we have been thus tedious in regard to them.

In the last quotation which we made *from the Reply*, it will be remembered, that the words—“symptoms pathognomonic of congestive fever,” appear in inverted commas, as if given as a quotation from the Review. In another part of the Reply, another word is slipped within the inverted commas, and it reads—“no symptoms pathognomonic of congestive fever.” Now we have above, we believe, presented before the reader every instance of the occurrence of the above word in the Review ; in which also they appear only as quoted from the Medical History itself, *and in referring to Dr. Lewis' attempted distinction.*

Can it be discovered where *we* have said that there are "no symptoms pathognomonic of congestive fever?"

"Look here upon this picture" from the *Reply*.—

"The physicians of Alabama will doubtless be surprised to learn that there is a medical writer—a critic forsooth, who openly declares that there are no 'symptoms pathognomonic of congestive fever.'"

"And on this," from the *Review*.—

"We turn to the Doctor's own remarks, and will transcribe the only case given from the records of his own practice, with the hope that here at least we may be able to discover the 'vital or important derangement of the functions,' and '*the symptoms pathognomonic of congestive fever,*' with which he has intimated that we are to be favored, *distinguishing the congestive fever of Dr. Lewis from the forms which others have heretofore spoken of and treated under that name*; that, if possible, may be discovered our *open declaration*, 'that there are no symptoms pathognomonic of congestive fever.'"

When the plainness, and what we must believe the (to others) unmistakeable tenor of our language here are considered, in connection with the fact, that in this particular instance, especially, Dr. Lewis *promised to make an effort not to misrepresent us*, may we not venture to suppose that, under a calmer and less excited state of feeling, the words which he himself has used—"heedless and unreflecting confidence,"—"want of correct perception," &c., will rise up in unpleasing array before his imagination? And again, considering the manner and the circumstances of the presentation of the words—"symptoms pathognomonic of congestive fever," in the *Review*, and the connection in which they there appear, and then the manner and connection in which they are presented in the *Reply*; will we be giving Dr. Lewis more than merited credit, for a sense of justice, and correct feeling, to suppose that *other* words which he has used,—"*garbling,*" "*suppression,*" &c.,—may grate harshly upon his ear, when in hours of cool reflection they are sounded? We hope and believe not.

It will not, we are certain, be once supposed by any who have had the patience to follow us through this most unprofitable controversy, that we can have ought than the most exalted opinion of the *courage** of Dr. Lewis; yet we do believe, that, had our *brave* friend of the play, to whom we have once before alluded, been the author of the *Reply*, *even his would have failed him*, on a calm re-perusal of some of his statements.

We will quote again from the *Reply*.—

"The author of the Medical History, when attempting to place before the reader the fact, that congestive fever was very partial to some localities while it did not appear in others, uses the following language in relation to Montgomery, Dr. Boling's place of residence.

* See page 62.

—A correspondent residing for many years in Montgomery, a town situated amidst diluvial elevations of the prairie region, informs us that he 'has not seen any case of congestive fever, such as has been described by medical men living in other sections of the State.* That he has seen simple cases of fever collapse, or become complicated, (under the influence of injudicious remedies,) resembling in that condition the congestive state.' From abundant testimony it would appear that this town is exempt from the disease in its true characteristic form; take, for instance, the papers of Dr. Boling on the fevers of that vicinity. He describes no case of febrile affection which physicians of certain localities in Green, Marengo and Dallas would recognize as congestive fever. When the author of the Medical History made this respectful reference to Dr. Boling's work on Remittent Fever, he little dreamed that he was treading on such dangerous ground. As Dr. Boling's descriptions of disease, coupled with the statement at the close of his treatise, that all his observations in relation to remittent fever, 'are made with reference to the disease as it prevails here in Montgomery, and in its immediate vicinity,' did not impress the author with the belief that he had seen *true* congestive fever, he naturally concluded that, for want of a practical knowledge, the Doctor avoided any special attempt at description. But it appears from his indignant rebukes at our bare intimation that he had not described the disease, that he comprehends in that essay, an *account* of all the fevers common to the State. Although we avoided any other than respectful and complimentary allusion to Dr. Boling's treatise on Fevers, notwithstanding that it came within our province—and even now would be pleased to avoid any notice of it, as descriptive of the prominent diseases of the State; yet, as it is unceremoniously and indecorously thrust into our face, it will become necessary *bye-and-bye* to examine its pretensions to the character lately claimed for it."

By the way, how sinister, threatening and portentous of some future onslaught this "*bye-and-bye*" looks. "Marry, this is miching malicho—it means mischief."

In regard to the lack of *ceremony and decorum*, it will be found to amount to this. In the course of the Review of the Medical His-

* Dr. Lewis makes this "correspondent" modify slightly his language on the present occasion. In the *History*, he says he has not "seen any case of congestive fever, such as has been described by medical men as *prevailing* in other sections of the State." In the *Reply*, it will be seen that he says, he has not seen any case of congestive fever such as has been described by medical men now *living* in other sections of the State."

In the Review of the Medical History, it became necessary for us to show, that previous to the appearance of this paper, *very few* descriptions had been published of the disease, "as *prevailing* in other sections of the State," and even these, it appears, did not suit the fancy of Dr. Lewis. He has, however, discovered that there is a physician, Dr. W. B. Johnson, now "*living*" in the State, who has given a description of the disease, but whether "*as prevailing* in other sections of the State," or as *prevailing* in the "*Western country*," we are left by Dr. Lewis somewhat in doubt. Hence, probably, the necessity for the change of phraseology in the language of the "*correspondent*."

tory, it became an object of interest with us to ascertain the correctness or otherwise of the position—that congestive fever was radically and totally a different disease from remittent fever; and in what, according to Dr. Lewis, consisted the difference in the two diseases. Of course the only way to arrive at the point desired, was by comparison; and as our own paper, embracing an account of the different shades of remittent fever, and this only, had been *especially* referred to as containing *no* description of congestive fever, it naturally presented itself, or rather, we may say indeed, was forced upon us, for comparison. In the progress of the investigation, after making a proper disposition of certain phenomena, (e. g., the “remarkable colluvies,”) spoken of by Dr. Lewis, as attendant on the congestive fever, it became apparent that the distinction had not been established. This brings us to the notice of another instance of Dr. Lewis’ most peculiar method of induction. Stated in plain and brief terms, which we will take the privilege of doing, it may be reduced to this:—Dr. Boling has shown, by comparing my account of congestive fever with that *contained in his essay, which I have said was not congestive fever, that I have failed in making a distinction apparent; therefore*, Dr. Boling “comprehends in that essay an account of *all* the fevers common to the State.” That the comparison, which, under the circumstances, we could not well avoid, was so unpleasant to Dr. Lewis, and in his opinion so unceremonious and indecorous, of course we regret. Would the circumstances have warranted us in doing so, it should have been rendered less disagreeable. That Dr. Lewis *felt himself rebuked*, in regard to the matter, was not, it must be admitted, because we used towards him the *language of rebuke*. It was from *the facts* alone, and these, it has been said, speak more impressively than words.

To be Concluded.)

2—*The Epidemic of 1847: or Brief Accounts of the Yellow Fever that prevailed at New Orleans, Vicksburg, Rodney, Natchez, Houston, (Texas,) and Covington, Louisiana. Collected and published by E. D. FENNER, M. D., of New Orleans.*

Under this title I shall lay before the Profession such observations as I could hastily throw together, in addition to those I gave under the head of “*Health of the City*” in the last volume of this Journal, together with those obligingly furnished me by Drs. B. J. Hicks, of Vicksburg, W. G. Williams, of Rodney, W. McCraven, of Houston, S. A. Cartwright, of Natchez, and J. Gilpen, of Covington, La.

Wishing to obtain as full an account as possible of the late epidemic in the lower part of the Mississippi Valley, I took the liberty of addressing the above named gentlemen, who gave me the papers that follow, as well as several others who did not reply. They will please accept my grateful acknowledgements for their kind attention to my request, and I doubt not, their favors will be deeply appreciated by the Profession at large. To all of them, except Dr. Gilpin, I addressed a set of interrogatories which I thought would cover the grounds of inquiry. Dr.

Gilpin's letter was published in the November number of this Journal, but I deem it proper to republish it in this connection, by way of completing the history of the epidemic.

Dr. Lewis, of Mobile, did me the favor to send over a hasty sketch, when I expected to publish in March last, but afterwards concluded to draw up a more elaborate Report of the fever at Mobile, which may be found in the last number of this Journal.

Dr. Ashbel Smith, of Galveston, Texas, also promised me a communication, but afterwards determined to report more fully in a different way, which I hope will soon see the light. We shall thus, amongst us all, I trust, make up a pretty good history of the epidemic as it appeared at different places, which is the sole object I have had in view.

I.—*The Yellow Fever of New-Orleans.*

I sincerely wish that some abler hand had undertaken that portion of the task which relates to New Orleans, and I have postponed writing, partly under the hope that it would be, but seeing no prospect of it, I have determined to come forward and again throw myself on the indulgence of the Profession, as it would be a reproach to us all for such an epidemic as that of 1847 to pass without farther notice than the brief editorials of a periodical. I shall, therefore endeavor to somewhat enlarge the historical account, though I must necessarily be brief at this late day. This publication should have been made in the March number of this Journal; but, after laying aside my editorial labors, I really felt so much jaded in mind that I could not summon sufficient energy to undertake the task. The historical sketch of the epidemic in this city which I am now about to make, must necessarily be very imperfect—in fact, confined pretty much to my own observations, as I have not been able to obtain as much information as I should desire from the physicians of the city, nor has anything on the subject emanated from either of our medical societies. I shall take up the most salient points as they occur to my mind. In the numbers of this Journal for September, November and January last, under the head of “Health of the City,” may be found some editorial remarks on the epidemic, relative to its beginning, progress, extent, mortality &c., to which I would refer the reader.

Meteorology of the year.—I have compiled from the bi-monthly meteorological reports furnished to this Journal by Mr. D. T. Lillie, the following abstract, which will give a good general idea of the weather throughout the year:

January and February.—Weather cold, wet and changeable.

Thermometer ranged from	. . .	77.5, to 30.°
Barometer “ “	. . .	30.55, to 29.72.
Number of rainy days	. . .	16
Quantity of rain, (inches)	. . .	17.960.

March and April.—Weather very variable and bad—spring unusually late.

Thermometer ranged from	. . .	88.3, to 40.
Barometer “ “	. . .	30.50, to 29.72.
Number of rainy days	. . .	13.
Quantity of rain	. . .	14.341.

May and June.—Weather variable—May cool, June hot.
 Thermometer ranged from . . . 90.5, to 63.5.
 Barometer “ “ . . . 30.26, to 30.
 Number of rainy days . . . 21.
 Quantity of rain . . . 13.176.

July and August.—Rather cool, very wet, much thunder and lightning.

Thermometer ranged from . . . 91.5, to 71.
 Barometer “ “ . . . 30.24, to 29.87.
 Number of rainy days . . . 30.
 Quantity of rain . . . 22.265.

September and October.—Weather fair and beautiful.
 Thermometer ranged from . . . 89.5, to 52.
 Barometer “ “ . . . 30.38, to 29.91.
 Number of rainy days . . . 17.
 Quantity of rain . . . 10.435.

November and December.—Weather very changeable.
 Thermometer ranged from . . . 83, to 29.
 Barometer “ “ . . . 30.50, to 29.88.
 Number of rainy days . . . 12.
 Quantity of rain . . . 19.625.

Whole number of rainy days during the year, 108.

Whole quantity of rain, $97\frac{3}{4}$ inches.*

It will thus be seen that the weather throughout the year was for the most part cool, changeable and remarkably wet. The quantity of rain that fell is quite extraordinary. The wettest month was July, during which it rained 22 days. In June it rained 17 days—in August 8 days.

Intercourse with Vera Cruz.—In the month of March, the city of Vera Cruz, (considered to be the favorite abode of yellow fever,) was taken by our invading army under the command of Gen. Scott, and from that time the intercourse between this city and that has been constant and most extensive. As the main body of our army proceeded from Vera Cruz on towards the city of Mexico, every thing in the way of men, arms, ammunition and provisions had necessarily to pass through the former place. Throughout the year, new regiments continued to go to this line of operations, and the discharged, the sick and disabled were constantly returning to this place. From the best information I could procure, Vera Cruz has never been entirely free from yellow fever since it fell into our hands; yet, notwithstanding the large num-

* The Thermometer used for these observations is a self-registering one, not attached to the Barometer, and is placed in a fair exposure. Regular hours of observation, 8 A. M., 2 P. M., and 8 P. M.

The Barometer is located at an elevation of 19 feet above the level of the Ocean, and is suspended clear of the building.

The Rain Gauge is graduated to the thousandth of an inch, and the Receiver is elevated 40 feet from the ground.

ber of sick and discharged soldiers that returned from that place to this, during the spring and summer of 1847, very few cases of that disease were brought over, and the most careful inquiry I could make has not brought to light a single instance in which it was communicated directly to any person in this city or Lafayette. The nearest approach to it occurred in the following cases, the notes of which were given to me by Dr. Carpenter.

Case 1st.—J. Strider, from Vera Cruz, attached to the Quartermaster's service—came from Vera Cruz on the steamer "Galveston," about the 17th June, and landed at Lafayette—had all the characteristics of yellow fever, and died with *black vomit* on the 22d of June. Dr. C. got this note from Dr. Thorpe, who attended the man. I have not been able to see Dr. Thorpe, since the occurrence, but ascertained from Dr. Sunderland, who practises in Lafayette, that the man had laid sick in a large public room at a hotel in which a number of persons slept—that on inquiry, he never heard of any of them contracting the fever—in fact that the next case of yellow fever that occurred in Lafayette, happened in a person who lived four squares from this hotel, and nearly a month after Strider died. About the 25th of July the epidemic broke out in Lafayette and raged severely.

Case 2d.—Theodore Bell, musician to the 5th infantry, entered the *Maison de Santé*, June 17th. Had just arrived from Vera Cruz, on the steamer "Massachusetts"—had been in the hospital at Vera Cruz, in bad health—was attacked with fever on the voyage—had fever when he entered hospital here, with yellowness of the skin and eyes &c. On the 2d day after admission had hemorrhage from gums and fauces—became very yellow—vomited a dark colored matter—urine dark—died on the 27th of June.

I saw this case, together with several other physicians, none of whom seemed satisfied that it was a *decided case of Yellow Fever*. The truth is, the man had long been sick with diarrhœa, which I have before seen terminate fatally with hemorrhage and jaundice. I never heard of any sort of connection between this case and the next of yellow fever admitted into this hospital, which occurred on the 20th July.

Although yellow fever was prevailing at Vera Cruz at this time to a moderate extent, there were really hardly any cases brought to this city among the returning soldiers. Their chief disease was chronic diarrhœa. In the number of this Journal for July 1847, was published a letter from assistant surgeon Charles McCormick, medical director, stationed at this place, from which is taken the following extract relating to the sickness amongst the returning soldiers and at Vera Cruz.

"New Orleans, July 22, 1847.

GENTLEMEN :—At your request I furnish the following information in relation to the sick and wounded of the army: On the 16th instant the steamship Massachusetts arrived from Vera Cruz, having on board 163 sick from the army. On the 21st the steamer James L. Day arrived, having on board 120 sick men, also from the army and Vera Cruz. The military hospital at the barracks being nearly full, as many of the

men were received there, as Surgeon R. C. Wood deemed it proper to take in, with a view to their proper comfort and accommodation. The balance were placed in Dr. Luzenberg's and Stone, Kennedy & Carpenter's Hospital; and after these two hospitals had been filled, about thirty-nine were sent to the Charity Hospital, where they were received and made as comfortable as possible. In relation to yellow fever in Vera Cruz, Dr. Laub, of the U. S. Army, writes me: "We have a great many sick, and our list increasing; among them some cases of yellow fever, though as yet it cannot be said to have become epidemic. No doubt, however, in a short time we shall have it in all its virulence, at least if the accounts given of it by many here are to be depended on."

"Dr. Barton, U. S. Army, says:" 'Vomito increasing—but exactly what we are accustomed to in New Orleans—its type in some instances severe.'

"Dr. Barnes, who was employed and went from this city to Vera Cruz to assist in the military hospital, says:" 'The yellow fever prevails to a considerable extent in Dr. Porter's Hospital. There are about 350 in it and in Dr. Laub's, of the 1st infantry, and some 82 quarter-master's men. (Of course, the Doctor means, of all cases. F.) But it has not the malignancy I was led to expect it would present here. The most of the cases I have seen present more the appearance of remittent than of yellow fever; and were it not for the brilliant and red appearance of the eye, and above all, the termination of the cases in black vomit, it would be thought, in the country, remittent fever. They either run their course with great rapidity, or improve. I have seen but few cases of the typhoid fever which was seen last summer in nearly all of the patients suffering from it in the N. O. Charity Hospital, and in which it was almost universally fatal. The fever is very high for the first 24 or 36 hours; remission then takes place, of variable degree and duration, and mounts up again, and again remits, until the 5th or 8th day, when the patient either throws up black vomit or is left in a state of great exhaustion, free from fever and convalesces slowly &c.

* * * * *

* * * "Dr. Dashiell states a case of yellow fever that occurred previous to his departure from Vera Cruz, of a man in good health who was getting shaved in a barber's shop, and who commenced at once to throw up the *black vomit*, expiring (as a matter of course) shortly afterwards. This comprises the latest medical information I have received from that portion of the army in the direction of Vera Cruz.

Very Respectfully,

CHARLES McCORMICK,

Ass't. Surg. U. S. Army."

About the 1st of July an arrangement was effected by which all sick soldiers were taken to Dr. Luzenberg's private hospital, situated on the Pontchartrain railroad, about $2\frac{1}{2}$ miles from the centre of the city. At this beautiful and commodious establishment they enjoyed every comfort and attention that could be desired. There was probably an average of 500 sick soldiers at this hospital from July to December. As before stated, amongst the large number of disabled soldiers returned

from Mexico, there were *very few cases of yellow fever*. Dr. Luzenberg's hospital consists of three extensive buildings on the same lot, but a little removed from each other. The grounds are spacious and ornamented with flourishing and beautiful shrubbery. The establishment is in the suburbs, and quite retired from the populous part of the city. During the prevalence of epidemics a considerable number of yellow fever patients are generally received into this institution, and this year they were confined to one of the buildings, (the centre.) At this hospital the sick and wounded soldiers were entertained during this sickly summer and autumn, *without communicating or receiving yellow fever*, so far as I have been able to ascertain. How could this have happened, if the disease were either contagious or infectious? These soldiers were mostly from the interior of the country—probably not one in a hundred of them had ever suffered or seen yellow fever.

The vessels at this time plying between this city and Vera Cruz were chiefly engaged in the transportation of men, horses and provisions, and their place of landing was Lafayette; yet the epidemic was not declared there as soon as it was in this city.*

So much for the effect of our intercourse with the pestiferous city of Vera Cruz. From the time that city succumbed to our victorious arms, predictions were rife that a severe epidemic would follow as a natural consequence. I am aware that the impression has gone abroad that our sickness last year was mainly attributable to this source; but, from the best information I have been able to obtain in relation to it, I am compelled to think such a conclusion *altogether unwarrantable*.

Sanatory condition of the City.—The city was, perhaps, never in a more filthy condition than it was throughout this year. Notwithstanding the admonitions of the Board of Health and the remarks on the condition of the streets offered by the editors of this Journal and the newspaper press, from time to time, our city authorities took no precautions against sickness. Even the customary *very imperfect* measures for cleaning the streets were more neglected than usual. New Orleans is notoriously a dirty place, but in 1847 it may be said to have luxuriated in filth. In the month of April, the river rose very high, and for a week or two, poured through the cross streets into the swamp in the rear of the city. At the highest stage of water, a *crevasse* occurred opposite the city, in the little town of Algiers. A vast quantity of water escaped in that direction, before the *crevasse* was repaired. After this, stagnant water remained upon the Bingaman race-track until it disappeared by evaporation. Nothing more need be said about the hygienic condition of the city, as it was about as bad as it could be.

Commencement of the Epidemic.—We had the customary diseases up to the month of June, such as intermittent, remittent and typhoid fevers, diarrhœa, dysentery, and besides, an extraordinary amount of typhus or *ship fever*. This latter disease prevailed chiefly amongst the

* The distant reader should be informed that Lafayette adjoins New Orleans immediately above, and contains some 8 or 10,000 inhabitants.

European emigrants, an extraordinary number of whom came over this spring. During the months of April, May, and June, 641 cases of *ship fever* were admitted into the Charity Hospital alone. June is generally the healthiest month of the year in New Orleans. Our citizens enjoyed their customary exemption from sickness during this month, with the exception of a mild *influenza* which prevailed to some extent immediately before yellow fever made its appearance.

As usual, the epidemic fevers gradually assumed a graver type, as the season advanced. Remittent bilious fever increased considerably in June, and soon after the 1st of July, *was merged into yellow fever*. Some of the severe cases of remittent fever resembled *yellow fever* so much that they would have unquestionably been pronounced such, if they had occurred a month later. Indeed, cases much less strongly marked than some of these, were pronounced *yellow fever* during the prevalence of the epidemic. But it is customary here not to call any thing originating here *yellow fever*, early in the season, unless *black vomit* is seen, or has occurred. If the patient should have recently arrived from Vera Cruz or Havana, he will be pronounced yellow fever, no matter how light his symptoms.

Let us now see how the disease began and when it was declared to be *epidemic* by the Board of Health. The term *epidemic* is used in this city as much to designate *the amount* of yellow fever, as anything else. Hence we sometimes hear people say—"we have a good many cases of yellow fever, but it is not epidemic." Also the inquiries—"have you any yellow fever?" "Yes." "Is it epidemic?" "No, or yes"—as the case may be. From such remarks it may also be inferred that yellow fever, in its early stages, *does not always present such peculiar and decided symptoms as invariably distinguish the disease*.

So far as I could ascertain, the following is the first case of *black vomit* that occurred this season. The case was reported to me by Dr. A. Mercier.

An Irish drayman, who said he had lived in New Orleans about five months—residence near St. Mary's Market, in the upper part of the city—whilst on a visit to a friend who resided on Adeline street, three squares back of the Charity Hospital, was attacked with fever on the 21st of June. He soon became so ill that his friend would not permit him to return home, but sent for Dr. Mercier. Dr. Mercier found him laboring under high fever, with the usual pains in the head, back &c., but as it was so early in the season, he *did not suspect yellow fever*—thought it only an attack of bilious remittent. He bled him copiously and continued to treat him, but the fever proved very obstinate, and about the 11th day began to look so much like yellow fever that he invited Dr. Landraux to see the case. Dr. Landraux concurred with Dr. Mercier that it was now *a decided case of Yellow fever*, with every prospect of death by *black vomit*. He continued to get worse—turned yellow—had hemorrhage from the gums—threw up *black vomit* for ten hours before death, and died on the 3d of July, the 14th day of illness.

The following is a list of *the first ten cases* admitted into the Charity Hospital, made out by myself at the time of occur-

rence; or rather, this is an abstract from notes taken by myself and some of the attending physicians. The main object in view was to ascertain in what parts of the city they occurred and whether there was any connection between them.

First cases of yellow fever at the Charity Hospital, in 1847.

Case 1st.—Wilhelm Renners, age 28 years—a seaman, native of Prussia—arrived here from Liverpool, about 5 years ago—has lived in New Orleans ever since, mostly in the Third Municipality or lower part of the city—worked on the levee loading and unloading vessels—went to Brazos Santiago and returned about 15 weeks since. Entered the hospital on the 5th July, had then been sick with fever six days. Commenced throwing up *black vomit* on the evening of the sixth and died on the 7th. *An unquestionable case.*

Case 2d.—John Gooder, Englishman—age 19 years—a steamboat man—last from Halifax, Nova Scotia—in New Orleans 12 months. Entered the hospital on the 4th July, then sick 4 days—died with *black vomit* on the 8th. *Unquestionable Case.*

Case 3d.—Christopher Muhl, a German—age 27 years—a cooper by trade—arrived in this city on the 12th April 1847—had suffered from diarrhœa for a month before getting here. Lived in the lower part of the city. Has been admitted and discharged from the hospital on three different occasions on account of his bowel-complaint. Was admitted last on the 3d July, having fever, but complaining chiefly of diarrhœa. Took astringent medicines, which stopped the diarrhœa and he became much worse—on the morning of the 8th commenced throwing up his food and drinks, and at 7 P. M. *black vomit* appeared—this continued until the 10th at 10 o'clock A. M., when he died.

N. B. The postmortem examination of this case created some doubts as to its being genuine yellow fever. No black vomit was found in the stomach or intestines. The gastro-intestinal mucous membrane throughout was infiltrated with limpid serum—it was in a dropsical condition—that of the stomach was of a rose color—there was a slight abrasion near each of the orifices of the stomach, not larger than the finger nail, but no appearance of blood about them. There was about 2 quarts of clear serum in the peritoneal sack—lymph deposits over the surface of the liver, spleen and heart. Adhesions between the heart and pericardium, also between the lungs and ribs. The liver and spleen were very dense, the latter enlarged. The kidneys were granulated; the left greatly hypertrophied, the right atrophied. The blood coagulated readily. The body and eyes were very slightly yellow. Several experienced physicians who witnessed this examination concluded that the case was not yellow fever, but chronic gastro-enteritis terminating in hemorrhage, alias black vomit..

Case 4th.—Christopher Antoine—a German weaver—age 48 years—arrived from Havre, 6 months since. Residence on St. Phillip street—entered hospital July 10th, then sick 7 days—died on the 11th with hemorrhage from the mouth and anus, but no black vomit. The corps became yellow. *No doubts expressed about the case.*

Case 5th.—G. W. Sherman—age 25 years—has been in New Orleans the last 5 years—thought he had yellow fever three or four years ago—lived in the Third Municipality—business on the levee, loading and unloading steamboats and ships. Entered hospital on the 10th July—said he had then been sick about 10 days. Complained of great restlessness, pain in the head and back, sick stomach—eyes injected, skin yellow—died on the 11th, with convulsions. The corps was very yellow, lower part purple.

This man did not throw up black vomit, but there was no doubt as to its being yellow fever.

Case 6th.—Susan Antoine, age 49 years, the wife of case No. 4, residence same; entered hospital July 11th, then sick with fever 8 days—eyes injected, skin yellow, slight hemorrhage from gums. This patient was convalescent on the 13th and recovered.

Case 7th.—William Rose, seaman, native of Canada, aged 22 years; a cook and steward on board the schooner "Belle," at present engaged in the U. S. Service, arrived here from the mouth of the Rio Grande, on the 29th of June, and remained on board the schooner across the river at Algiers; was exposed to the sun a good deal. Was attacked with violent headache, fever and sick stomach about 11 o'clock A. M., July the 6th; says he took five or six drops of laudanum. On the 7th the captain of the vessel gave him five purgative pills which operated very severely; he continued to vomit. The captain then gave him an emetic which vomited him severely. Says he continued to vomit and have fever and headache until Friday the 9th, when the ejecta were so acid and acrid as almost to excoriate the fauces. On Saturday the 10th he commenced throwing up black vomit. He was brought to the Charity Hospital on Sunday morning, July 11th. I saw him with Dr. Cross at 5 P. M. Found him asleep, as soon as he awoke, commenced retching—pulse 84 and soft; bowels open; no urine for two days; skin yellow; eyes muddy yellow; intellect clear; surface cool and dry; tongue slightly furred; but little thirst. Continued to throw up black vomit and died about 3 o'clock at night. *An unquestionable case of yellow fever.*

Case 8th.—James Cooper, an Englishman, aged 25, a seaman. Says he came from Vera Cruz about 3 months ago; is subject to epilepsy; when asked how long he had been sick; said three weeks, when he had the last fit; never felt well since. Entered the hospital on the 17th July and died with black vomit on the 19th July. The corpse turned very yellow. No autopsy. As yellow fever was not suspected when he came in, his particular residence in the city was not ascertained.

Case 9th.—Manual Davan, Spaniard, laborer; has lived about New Orleans mostly for the last two years, but never spent a summer here. Last residence in the Third Municipality. Entered the hospital on the 15th July; then extremely low; hemorrhage from mouth, hiccup, very yellow, no urine; been sick 4 days. Died at night, with black vomit; after death the stomach was found to contain black vomit.

Case 10th.—Adam Keepferler, a German laborer, aged 24 years ; in New Orleans 6 months; lived near St. Mary's Market; worked sometimes on the levee and sometimes in the swamp back of the city. Entered hospital July 15th; said he had been sick seven or eight days. On the 16th I found him clear of fever; intellect clear; does not sleep well; has thirst, pain in the stomach, yellow skin, hemorrhage from gums; bowels open, urine free, pulse 80. July 17th, better; skin cool and moist, but very yellow; less hemorrhage; rather restless, but no particular pain; stomach quiet and easy.

This man continued very weak for some days; suffered very much from inflammation of both parotid glands, but they were relieved without suppuration; he convalesced slowly and was discharged on the 25th of July.

From this time admissions for yellow fever increased daily and soon became numerous.

On the 12th of July the Board of Health report the whole number of interments in the city, for the week ending July 10th, as having been 138, and make the first public announcement of the appearance of yellow fever. The following extract is taken from their proceedings, published in the papers of the day :

"Meeting of the Board of Health, Monday, July 12th, 1847.

* * * * * Five deaths from yellow fever have occurred in the Charity Hospital, and two or three cases are still under treatment in that institution. They appear to have originated in the city; and no facts have come to light to prove any connection between these cases and the fever prevailing at Vera Cruz, or other foreign ports.

(Signed) W. P. HORT, Chairman.

A. HESTER, Secretary."

In their report for the week ending July 17th, the Board of Health say the whole number of interments was 143; of which 6 *died of yellow fever*.

In their next weekly report, up to July 24th, the whole number of deaths reported, is 131; of which 16 were from yellow fever.

In their next report for the week ending July 31st the whole number of deaths is 177; of which 47 were from yellow fever. In this report, the Board announce the approach of the epidemic, as follows :

"Board of Health, August 2, 1847.

"It having been well established by the observations of the physicians of the city, that the yellow fever is now prevailing in nearly every part of it, and further appearing by the reports of the medical men, of the public and private hospitals, and of the cemeteries, that the cases of this disease have been numerous during the past week, and forty seven thereof fatal—it becomes the duty of this Board to apprise the public, and particularly the unacclimated, that we are on the eve of an epidemic, that the latter may prepare to absent themselves in time, and avoid such exposure and imprudence as may increase their susceptibility to the disease. (Signed) WARREN STONE, Chairman.

A. HESTER, Secretary."

From this time daily reports were published in the city newspapers, and the deaths *from yellow fever alone* during the next week were 133. The disease and the mortality continued to increase, and pervaded all

ranks of society throughout the whole extent of the city. The greatest mortality occurred during the week ending the 5th of September, when the deaths, from yellow fever alone, amounted to 435. From this time the disease gradually declined, as will appear from the following extract from the November number of this Journal, in an editorial upon the "*health of the city*," written by my then colleague, Dr. Hester. Dr. Hester says: "After much labor and great care, we have compiled from the published reports of the Board of Health, the following statement which will speak for itself.

Interments in the city of New Orleans, from the 3d of July to the 18th October, 1847, inclusive.

For the week ending	10th July—	Total	138—	of yellow fever	5
"	" 17th "	"	143	"	6
"	" 24th "	"	131	"	16
"	" 31st "	"	177	"	47
"	" 8th Aug.	"	263	"	118
"	" 15th "	"	353	"	197
"	" 22d "	"	432	"	322
"	" 29th "	"	461	"	328
"	" 5th Sept.	"	540	"	435
"	" 12th "	"	491	"	355
"	" 19th "	"	257	"	169
"	" 26th "	"	181	"	85
"	" 3d Oct.	"	149	"	61
"	" 10th "	"	126	"	44
From the 10th to 18th	"	"	148	"	53

Total, 3,990. Of yellow fever, 2,241

Interments in the city of Lafayette, from July 26th to September 21st, inclusive—total, 793; of which 498 were from yellow fever. Thus making the total deaths of all diseases in both cities, 4,873; of which 2,739 were from yellow fever."

Termination of the Epidemic.—On the 18th of October the Board of Health published the following statement:

"Meeting of the Board of Health, October 18th, 1847.

The Board of Health feels authorized to make the announcement that the yellow fever, which has been prevailing as an *epidemic*, has for some time ceased to exhibit this character, and as such has now disappeared. At the same time it is proper to state, that the *sporadic* cases, which have always been seen for one or two months after the disappearance of epidemic yellow fever, must still be expected to prevail. (Signed) WARREN STONE, Chairman.

W. T. BRENT, Secretary pro tem."

Sporadic cases did occur until very late. The number of deaths from yellow fever reported to the Board of Health, for November, was 12; and for December 10. The last death reported from yellow fever, occurred in the week ending on the 25th of December.

The weather for the first half of November was so very warm, and the city was so rapidly filled up with returning inhabitants, as to give rise to some apprehension that the epidemic would be revived. But

such was not the case; it had run its career and gradually died away. Yellow fever seldom prevails at this place as an *epidemic* longer than about *two months*. It then subsides, whether there be frost or not. The above announcement of the Board of Health was made *more than a month before the appearance of frost*, which was not seen here until the 26th of November. At the *equinox* we had rain and a pretty smart blow for a *single day*, but it then cleared off warm and continued so, with but slight deviation, for two months. So much for the rise, progress and decline of the epidemic. We have yet to speak of its general character, extent and mortality, and then to offer some special observations.

General Character of the Epidemic.—As there has been no extensive or severe epidemic (excepting this) in New Orleans, since 1841, when I came here to live, I have had to refer to physicians of much longer residence, for a comparison between this and others. All the older physicians with whom I have conversed on the subject seemed to agree that *the epidemic of 1847 was the most extensive that ever prevailed in New Orleans, but was not so malignant or severe as that of 1841 or 1839*. This conclusion appears to be strongly supported by statistics obtained from the *Charity Hospital and the Maison de Santé* at both of which institutions the mortality from yellow fever was less than was ever known before.

At the commencement of the epidemic, the attacks were generally so mild and yielded so readily to treatment, that there was considerable discussion as to whether it was *really yellow fever* that prevailed. It was not very long, however, before this point was universally conceded, and towards the 1st of September the disease became very malignant.

In previous numbers of this Journal, I have frequently alluded to the want of precision amongst the physicians of this city, as to *the particular symptoms which characterize yellow fever*. Hence the doubt and uncertainty so often witnessed when required to pronounce upon a case. Doubtless there are numerous cases so strongly marked from the onset, as to admit of no hesitation whatever; but there are others, with symptoms so mild and progress so insidious, as to elude all suspicion, until patient and physician are alike startled by the sudden appearance of some *fatal sign*. Many cases cannot be distinguished from ordinary remittent and even intermittent fever, *unless they approach a fatal termination*. Some cases admit of doubt in the earlier stages—if promptly relieved, there is doubt after they recover—but if these same doubtful cases be neglected or maltreated and terminate *fatally*, they then generally become *plain enough*. How often do we find Doctors differing about the character of a case of fever, and at last see their decision directed more by the *attendant circumstances*, than by any real value of the symptoms *per se*. If the case occur when yellow fever is common, the *slightest similitude* may influence the judgment; but if it should happen to be *the first suspicious case of the season*, or should occur *unusually late*, or out of season entirely, then the strongest evidence, such as *black vomit, hemorrhage, &c.*, will be required to settle the question. In the autumn of 1846, the members of the Board of Health (all physicians) differed so much about the character

of the prevailing fever, that a committee of three was appointed to examine the wards of the Charity Hospital and report the number of cases of yellow fever to be found there. In the performance of this duty, one member of the committee gave rather a reluctant assent to the discovery of only 6 or 8 cases, whilst another thought there were about 20 cases. The books of the hospital that year show the admission of 148 cases of yellow fever after the 1st of September, and 89 deaths.

In view of the facts just related, the term *yellow*, like the term *congestive*, as applied to *fever*, serves more properly to designate a *condition of the system or stage of disease*, than any *separate, distinct or specific form or kind of fever*.* Symptoms which do not uniformly distinguish a disease from all others *before it has run its course*, or until death is about to close the scene, are unworthy to be called *diagnostic*.

During our late epidemic, all the forms of our endemic fevers were to be seen, such as intermittent, remittent, congestive, typhoid, &c., as may be seen by reference to the Statistics of Fever published in the last number of this Journal.

At the <i>Charity Hospital</i> the Admissions for Yellow Fever were	2811.
Deaths	895.

Maison de Santé.—(Private Hospital of Drs. Stone and Carpenter.)

Admissions of Yellow Fever,	334.
Deaths,	34.

These are extraordinary results. Heretofore, at the Charity Hospital, the deaths from yellow fever have generally amounted to about one-half of the admissions. In this instance they do not amount to one-third. The great mortality that usually attends yellow fever at the Charity Hospital is not at all surprising to those who are familiar with this institution. It receives the lowest class of patients, a majority of whom perhaps enter after the third day of illness, and a large number in a *moribund state*. In addition to all this, it is impossible for patients in a large hospital like this, crowded as it necessarily is during an epidemic, to receive those minute attentions so highly important when they are in a critical state.

As the *Maison de Santé* is a private hospital and receives none but pay-patients, they must necessarily be of a superior class and have better attention. Nevertheless the success here presented is most commendable, and would be creditable in private practice. If these statistics be correct, they certainly substantiate the mild general character of this epidemic. At the commencement, the attacks were generally mild and yielded readily to treatment, but towards the last of August and first of September they were very severe.

Extent and Mortality.—The disease pervaded the whole extent of this city and Lafayette, also Carrollton, Algiers, and down the river as far as the U. S. Barracks. In the family of Mr. Davis, at the Tobacco Warehouse, about three miles below Canal street, I saw five cases at

* See the remarks of Dr. Brown, of Woodville, Miss., on the term "*congestive*," in the Third Volume of this Journal, page 443.

one time. Dr. Wood, U. S. Surgeon at the Barracks, six miles below the city, informed me that he witnessed several deaths with black vomit in October.*

As regards our own city, those quarters suffered the most which contained the *largest number of recent and unacclimated inhabitants*. Amongst these, the poorer classes, as usual, suffered the most, on account of their manner of living, exposure to the exciting causes, imprudence, &c. The Third Municipality, upper part of the Second, and back part of the city generally, having the largest number of small houses at cheap rent, necessarily contained the greatest portion of this class, and consequently suffered most. The central portion of the city, consisting of the First Municipality and lower part of the Second, having a greater number of acclimated and independent citizens, suffered least. The city of Lafayette, having fully doubled its population since 1841, was severely scourged. The localities around the two markets, St. Mary's and Poydras, in the Second Municipality, being densely inhabited by the lower order, were severely scourged. But it really appeared that the amount of sickness in different quarters was regulated *more by the character of the population* than by anything *especially pertaining to the locality*. The population about New Orleans is proverbially restless and moveable; hence many persons were taken sick on their passage from the city—some on board of steamboats going up the river; others at the summer retreats across the lake, as Covington and Mandeville, Pass Christian, Pascagoula, Biloxi, &c.

At Pass Christian several deaths occurred in persons who had recently gone over from the city; but the fever did not spread among the numerous visitors there. At Covington it did appear to spread, according to Dr. Gilpin, whose communication is annexed. On the Mississippi river, cases were taken to nearly all the landings as high as Memphis; but Rodney is the only place where the fever appeared to spread among the inhabitants, which we learn from Dr. Williams, whose paper will follow this.

It is altogether impossible to estimate with any accuracy the number of cases of yellow fever that occurred in this city during the epidemic of 1847. By many it is thought to have amounted to *twenty or twenty-five thousand*; but this calculation may be wrong. As there has been no extensive epidemic since 1841, and the population has been constantly increasing, there were doubtless more *subjects* for yellow fever than at any previous time.

Mortality.—The number of deaths from *yellow fever* reported to the Board of Health by the sextons of the New Orleans cemeteries was 2306. The number of interments from *yellow fever* in the Lafayette Cemetery, as published in the "*National*," a city newspaper, date 23d October, was 613. One of the New Orleans sextons neglected to report for two or three weeks. Many who died in this city were in-

* Dr. W. being absent from his post, Assistant Surgeon Sloan has kindly furnished me with a communication on the subject, which will be appended to this article.

terred in Lafayette and *vice versa*. I am inclined to think that 3000 would not be short of the number who died of yellow fever, in the two cities. Amongst them there were *twenty-three natives of New Orleans*—mostly children.

The newspaper above referred to contains a catalogue of 2729 interments of persons who died of yellow fever, and were buried in the cemeteries of New Orleans and Lafayette, in 1847. This catalogue gives the *date of burial, name, age and place of nativity*. The following classification of the ages at which death occurred may be interesting to the inquisitive reader.

AGE.	NO.	AGE.	NO.
Infants,	3	50 to 60 years,	103
1 to 10 years,	81	60 " 70 "	46
10 " 20 "	186	70 " 80 "	14
20 " 30 "	1098	80 " 90 "	3
30 " 40 "	671	90 " 100 "	2
40 " 50 "	250	Unknown,	269

Total, 2729; of which 1954, or *nearly three-fourths* were taken off between the ages of 20 and 40.

SPECIAL OBSERVATIONS.

Second Attacks, &c.—There were many instances of persons having the fever this year, who had had it before. Drs. Jones, Meux, Campbell, Picton, Beugnot and others have told me of cases whom they had attended themselves during previous epidemics. One of the worst cases that I saw recover this year occurred in an intelligent Irishman, who entered the Charity Hospital on the 2d of September, the fourth day of his sickness. He said he had had a raging fever for three days, attended with violent pains in the head, back, limbs, &c. The fever had then subsided; he was turning yellow, and had hemorrhage from the gums. A better marked case of yellow fever was never seen. This man told me that he had come to live in New Orleans in 1824—that he had an attack of yellow fever much like this in 1825; was attended by Dr. Ker, who pronounced it such—that he had lived here ever since, with the exception of 17 months spent in Mobile—and that he had enjoyed uniform exemption from fever during the whole intermediate time. He recovered and was discharged on the 12th of September.

It is the general opinion here and elsewhere, that if a person have yellow fever *once*, he will have it *no more*, provided he continues to reside in *yellow fever localities*. Now, that a *strong attack* of this fever does effect a *great degree of immunity* from it in future, does not admit of a doubt; but this is certainly not the case with *mild attacks*. Nor is the immunity just allowed, to be compared with that effected by an attack of small-pox, measles, whooping-cough or scarlatina, as I have heard asserted.

Attacks among Creoles or Natives.—The fever cannot be said to have prevailed as an epidemic amongst the Creole population. The adults may be said to have escaped the *decided form* of yellow fever almost entirely, though I have been informed by several extensive

practitioners that they saw a number of strongly marked cases amongst *creole children* under six years of age. Dr. J. H. Lewis, who lives in the Third Municipality, and does an extensive practice amongst the *creole* population, told me that he knew several *creole children to die with black vomit* during this epidemic. Many physicians here are of opinion that *creole children*, especially *whites*; are liable to yellow fever, but that it is generally so mild as to require but little attention, and but for the occasional appearance of black vomit amongst them, it might pass for some other form of fever. It has just been stated that there were twenty-three deaths from yellow fever amongst the natives this year.*

Negroes and Colored People.—All colored people recently settled in New Orleans are liable to have yellow fever, perhaps equally as much as white people; but from some cause or other, the disease is certainly much milder amongst them. The number of attacks amongst negroes this year was very great, yet the mortality was extremely small. Mulattoes evidently suffered much more than *blacks*.

Escapes from the Epidemic.—Notwithstanding the general prevalence of yellow fever in 1847, many persons who had but recently settled here and had never had the disease, escaped this year. On the other hand, there were instances of persons who had resided here a number of years, escaped all the epidemics subsequent to 1840, and had the fever this year. There are numerous instances of persons who have resided here 15 or 20 years, without ever having yellow fever. These facts go to prove that a person may by some years residence become just as secure against the disease as if he suffers an attack; also that neither an *attack* nor *acclimation* will effect an inviolable immunity.

Treatment.—What shall I say under this head? Such is the diversity of theory and practice pursued in *yellow fever* by the physicians of New Orleans, that it would be altogether vain for me to attempt to delineate it. Suffice it to say, that every conceivable variety of practice is pursued, from the use of the most *heroic remedies*, down to a virtual dependance on the *vis medicatrix naturæ*. The previous numbers of this Journal contain three systematic essays on the *cause, nature and treatment of yellow fever*, emanating from gentlemen who occupy the highest rank among the medical faculty of this city—I allude to the papers of Drs. J. F. Beugnot, P. A. Lambert and J. Harrison. In these essays, all the prominent remedies that have been resorted to in the treatment of this disease are ably reviewed, and their merits and defects fairly stated, after having been fully tested. They will always be referred to with interest by the student of yellow fever at this place, and are particularly valuable as illustrating the views of the leading

* Dr. Picton informs us that he really never was satisfied that yellow fever attacked *creoles*, until this year, when one of his own children, a *native of the city*, came very near dying of it. The same with respect to *second attacks*,—he attended a case this year which he had attended before with well-marked yellow fever.

French and American physicians of this city at the time. Of course many differ from these gentlemen; but the difference is in minor matters.

I deem it altogether useless to give the treatment which I pursued, but perhaps a brief allusion to a few of the most prominent methods pursued by our physicians would be acceptable to the reader.

In reflecting on the various remedies and plans of treatment in yellow fever presented to my view in the course of a pretty extensive and careful observation at the Charity Hospital, in the walks of private practice, and in conversation with my medical brethren, I think the whole may be designated under the following two general plans of theory and practice, viz., *the abortive and the rational or eclectic plan.*

1. *The Abortive Method.*—The object of this method is *to cut short the fever as soon as possible.* It was pursued by a few bold practitioners, who, however, resorted to very different means for its accomplishment: one set relied *almost exclusively upon blood-letting*, the other equally as much *upon the sulphate of quinine.* *The blood-letting plan* is as follows:—as soon as the chilly stage is passed and reaction fully established, the patient is set up in bed and bled in a full stream *to syncope*; a purgative enema and hot mustard foot bath are then administered. Reaction takes place, and when fully developed, the bleeding is repeated as before. And so on, as the reaction is strong and the patient can bear it. If the patient is unable to bear venesection, local depletion, by means of cups or leeches to the chief seat of pain, is resorted to. Some patients require to be bled four or five times, but generally not more than one or two free bleedings; with perhaps some cups or leeches. No medicine is given by the mouth,—the bowels are kept freely moved by enemata. The foot-baths and sponging the body are repeated *pro re nata*, with cold drinks and light covering, complete the treatment. This is the depleting plan *par excellence*, as practiced by the late Dr. Luzenberg, Dr. Beugnot and a few other leading physicians below Canal street. There are others who follow this plan somewhat modified—they deplete all cases and freely, but they give medicines also. If the patient be of such a temperament or in such a condition that he cannot be bled, local depletion is depended on; but these gentlemen look upon all such as have severe attacks and *cannot bear the loss of blood*, as being *very dangerous.*

The Quinine Method.—In the essay of Prof. Harrison, previously alluded to,* may be found an interesting account of the introduction of large doses of the sulph. quinine in the treatment of yellow fever in this city, in 1839 and 1841.

If I am not mistaken, the gentlemen who first adopted the practice still admit the wonderful powers of the medicine, though they have since fallen into more of an eclectic plan. Assistant Surgeon Charles McCormick and Dr. A. J. Wedderburn are the only physicians, so far

* See Dr. Harrison's paper on Yellow Fever in the Second Volume, November No. of this Journal—also Dr. McCormick's papers on the use of Quinine, &c., in the same Volume and Number.

as I know, who pursued this practice in its full extent, in the treatment of this epidemic. I shall therefore give their methods as obtained from themselves. As practised by Dr. McCormick, it is as follows :—when the fever is fully developed, a purgative enema and mustard foot-bath are first used, and from 15 to 30 grains of quinine then given to *subdue the fever*. If the pain in the head is *very violent*, he is bled from the arm, or cups are applied to the mastoids; otherwise, blood-letting is dispensed with. The large dose of quinine seldom fails to reduce the excitement in a few hours, and then he gives 15 or 20 grains of calomel with or without as much of the quinine combined. The foot-baths and enemata are repeated *pro re nata*; the bowels are freely purged; the fever vanishes, and the patient seldom requires more than the third dose of quinine.

Dr. Wedderburn first orders an enema, consisting of a table-spoonful of mustard in a quart of warm water, which he says evacuates the lower bowels more promptly and efficiently than anything else. Then comes the hot mustard foot-bath, and afterwards the following dose ;— $\mathcal{R}$ Pulv. Rhei. grs. x, Pulv. Ipecac. grs. ii, Submuriat. Hydrarg. grs. v, Sulph Quinine, $\mathfrak{D}$ i, mix in syrup and give at once. Sometimes he first gives 15 or twenty grains of quinine with 20 or 30 drops of laudanum suspended in water, and the above powder immediately afterwards. This purges freely in six or eight hours, and the quinine and laudanum are afterwards repeated according to the pain and fever. The purgative mentioned happened to be the one he mostly used last summer—he admits that some other might have done as well. This is his most general course. * if the attack be very severe, and the patient suffers violent pain in any part, he at once gives from 20 to 30 grs. of quinine combined with 40 or 50 drops Tr. Opii, or 2 or 3 grs. of opium. According to Dr. W. this dose rarely fails to extinguish both the fever and pain in a few hours. Drs. McCormick and Wedderburn both speak in the most exalted terms of their abortive methods of treating yellow fever. Dr. W. *never bleeds from the arm*, and very seldom orders either cups or leeches. Other physicians here use quinine freely in yellow fever, but not like the above named gentlemen, *to cut the fever short at once*.

There is one thing worthy of special notice in connection with this method of treating yellow fever, which is, that although the fever may be cut short, the disease is not always necessarily removed. Convalescence is not at once established; but the patient occasionally lingers in a feeble, though cool, quiet and painless state for some days, and then sometimes dies with black vomit. It would seem that the alterations in the blood, &c., produced by the morbid cause, still go on, to terminate in health or death, although what is called *the fever* is extinguished. Dr. Harrison mentions this fact in his essay before mentioned, and it is well known to many of our physicians. It has been remarked, that even when the fever *was not cut short by any potent medicine*, but spontaneously subsided in 24 or 36 hours, as it sometimes does under the use of mild remedies, the prostration would be as great and the convalescence as tedious as if the fever had run its usual course for 72 hours. Hence it is thought that *the febrile poison must be eliminated through the natural emunctories*, before healthy reaction can be estab-

lished. With some, this might constitute a serious objection to the *abortive method by quinine*; but it certainly is an important consideration to be able to relieve the painful and distressing symptoms, by a remedy which does not prevent the execution of any other indications that may arise. Nor can it be denied that when pain and febrile excitement are reduced, the system becomes much more amenable to the action of any remedies that may be indicated.

Under this view of the matter, I think the introduction of *sedative doses* of the sulphate of quinine may be considered a valuable improvement in our therapeutics of yellow fever. There evidently exists considerable prejudice against this method of using quinine, and we hear of physicians, both in this city and elsewhere, who state that they gave it a fair trial, and it did not answer their expectations. For my own part, I think it very doubtful whether they have given it a *fair trial*—they are *afraid of it*—in short, *they have not learned how to use it*.

As to the fact that patients are sometimes unexpectedly lost after all fever and pain are subdued, Dr. Wedderburn says that in all such cases as have come under his observation, it proceeded from the most *culpable imprudence*. They are so promptly relieved of all pain and fever, that they do not allow sufficient time for the system to recover from the shock it has sustained.

2. *The Rational or Eclectic Method.*—This method is founded on *experience and rational observation*, but independent of scientific induction. The object of this method is *not to cut short the disease*, or take it entirely out of the hands of nature; but rather to *guide the patient through the natural stages of the fever, and to address proper remedies to the symptoms as they are presented*. If blood-letting is plainly indicated, they bleed—if any particular organ seems to suffer most, they address their remedies chiefly to that, and so on.

A *rational eclecticism* is the *platform*, to use a phrase that recently has come much in vogue in the politics of the day, on which the great body of the Medical Profession stand at the present day, in the treatment of most diseases. The very existence of *specifics* is doubted by many, and we have to be guided by *general principles* until we learn from *experience* the *peculiarities* belonging to diseases in different regions and localities, and the adaptation of certain remedies to meet these peculiarities. That different climes and localities do give striking peculiarities to the diseases which prevail in each, is almost universally admitted by those who had extensive opportunities for observation.

Such being the object and views of the advocates of the *rational or eclectic* system, they go to work accordingly, each prescribing the remedies which he has found by experiment best adapted to meet the symptoms, and most of them falling into more or less of a *routine*. Nor, under these circumstances, is it surprising either that there should be a great diversity of practice, or that the practice should vary in different places and seasons. The general practice in yellow fever pursued in New Orleans is not always the same, because the type of fever is not always the same; and it generally differs from that pursued in Mobile, Vera Cruz, Havana, Charleston, Natchez, Vicksburg, &c., because each of those places presents some modification of the disease. It would be

almost impossible to describe all the plans of treatment pursued by the rationalists in this city, but perhaps I may succeed in giving an outline of a few of the more prominent *routines*.

I may state that *venesection* is used with great caution by the rationalists—they foresee a stage of depression or exhaustion, that will inevitably appear at the decline of the fever, and therefore endeavor to economise the powers of the system as much as possible. Cupping over the chief suffering organs is much oftener resorted to. Mustard pediluvia are universally administered. In the first stage of the fever, a purgative of some kind is invariably administered. Some give an efficient dose of castor oil; others prefer a mercurial cathartic; others a saline. The bowels having been freely evacuated, local symptoms relieved and a general perspiration established, absolute rest and quietude are rigidly enjoined, and the fever is permitted to go on to its natural termination or crisis, which generally takes place on the *third day*. This is truly the *critical stage* of the disease, for the fate of the patient will soon be decided. He must either die or begin to recover very soon. A new set of symptoms are now presented, requiring different remedies altogether from those first used. Blisters, gentle stimulants, anodynes, antacids, &c., are now called in requisition and demand the utmost skill of the physician. Many patients are doubtless saved after getting upon *the very verge of black vomit*; but after this discharge becomes established, they are generally considered beyond the reach of medicine and have to depend upon the efforts of nature and the kind attention of the nurse. A variety of medicines have been recommended for black vomit, but so far as I have been able to ascertain, not one of them possesses any *reliable virtues*. French brandy, porter and ice are depended on more than anything else in this stage, by the physicians in New Orleans. Some physicians, amongst them a few eminent practitioners, after relieving the first distressing symptoms, by means of a purgative, the mustard foot-bath and cupping, put the patient at once on the solution of the sulphate or ferro-cyanate of quinine, 5 grains a dose every two hours, until the fever completely subsides. When the critical stage arrives, they treat it pretty much like those just mentioned. I cannot descend into the minutiae of practice, but such is an imperfect outline of the practice pursued by the rationalists. As before stated, their object is *not to cut short the disease, but to guide the patient safely through all the natural stages of the fever*.

There are, doubtless, cases which will prove fatal in spite of anything that can possibly be done. From the time the disease is declared, the patient is a *doomed victim*. He is either overwhelmed irresistibly at the outset, or the poison works its ravages *so stealthily* as to escape observation until it is too late for remedies. Witness the case of Dr. Dashiell, in which black vomit unexpectedly appeared whilst a man was getting shaved in a barber's shop, at Vera Cruz. I myself have seen a man lying quietly in his bed at the Charity Hospital, *reading a book*, whilst the fatal black vomit was already upon him. He seemed to be surprised at the minute inquiries I made about his case, and as I turned away, asked if I thought *he was in any danger*? I gave him an equivocal answer, and left him in blissful ignorance. He

resumed his story, and I saw him no more. The next morning he was dead.

It has been shown that the most robust class of people and at the most vigorous age are the favorite subjects of yellow fever. Such persons do not complain at trifles ; they often pay but little attention to the first symptoms of disease, and only take to their beds when completely overpowered, thus losing the most important time for treatment and lessening their chances of recovery. Thus the hardy mechanic or outdoor laborer is often lost ; whilst the more delicate, sensitive and prudent gentleman or lady is preserved. As to the *amount of success* attained by the followers of the *rational method*, it is impossible for me to state it. So far as I have been able to ascertain from some of the most extensive practitioners, it was at least *very gratifying to themselves*.

Homœopathy.—I cannot close my remarks on the treatment of yellow fever, without a brief allusion to a novel practice which has recently been introduced into our city, and like all novelties, has captivated some of our citizens. I allude to *Homœopathy* and its practice. In the autumn of 1846, Dr. Taft, apparently a modest, intelligent and genteel young man, came from the North and settled himself in New Orleans, to practise this new system of medicine, up to that time unknown in these parts. Having gone before the Board of Examiners, presented his diploma, (I know not from what medical college,) and obtained license regularly, he soon met with great encouragement and was really established into a lucrative practice at the time the epidemic broke out in the summer of 1847. As he was unacclimated, and of course might expect to be attacked, he was asked by a friend "what physician he would employ when he should be attacked with yellow fever?" His reply was—"Dr. Taft." Sure enough, in the month of August he was severely attacked, and Dr. Taft being immediately on hand, was called on to attend Dr. Taft. He at once resorted to his infinitesimal doses of Homœopathic medicines, and continued them until the end of the second day, when he became so ill, that his friends insisted on calling in medical aid, *nolens volens*. Two of the most respectable regular practitioners of the city were called in and did everything in their power to save him, but the precious time for active treatment had been lost, if *not worse than lost*, and he fell a victim to his own folly.

With Dr. Taft died *Hamœopathy* in yellow fever for the season ; but the reports of his successful *debut* went abroad throughout the land, and the vacancy created by the disappearance of "*Yellow Jack*" and *Dr. Taft* was soon filled by a host of Homœopathic physicians, chiefly from the "land of steady habits." They seem to find much favor in the sight of the community during *the healthy season of the year*, but whether, like the martins, they will vanish when the "Dog star rages," remains to be seen.

Hydrophathy.—I heard of but one physician who pursued the hydrophatic treatment, though there may have been others. Dr. S. W. Dalton, a regular licentiate, early in the epidemic fell upon the following simple method, with which he says he was so well pleased, that he pur-

sued it throughout the season. As soon as the fever was fully developed, and when the pains in the head and back were most severe, he enveloped the whole body of the patient, from the neck to the thighs, in a thick bandage of cotton or linen cloth, and then placing him in a large tub, poured cold water upon his head until he became perfectly cool and thoroughly *soaked*. He says this never failed to remove all pain and make the patient comfortable. He was then put into bed, and the covering tucked closely about him. In a short time, he broke into a profuse perspiration and went to sleep. If the heat and pain returned, the cold water was re-applied. In the mean time, the bowels were freely opened with castor oil, cool drinks were given, and this constitutes the whole treatment. Dr. D. says he treated more than 200 cases in this way, with the most gratifying results.

As a part of the history of the epidemic, I give Dr. Dalton's statement; it may go for what it is worth.

There were some other new systems of practice tried in this epidemic by a few persons, who probably could attract attention in no other way than by some innovation. Among them was *the system of Raspail*, or the *Camphor practice*. It called forth some flaming puffis in the newspapers, but I saw nothing of it myself, and only heard of an occasional case where an intelligent physician had been called to see an unfortunate victim who had thrown away his life by trusting to such nonsense.

As to the extent of yellow fever up the river from this place, I may give the following memoranda, in addition to the papers of Drs. Cartwright, Williams and Hicks, which are to succeed.

There were several cases at the village of Plaquemine, also at Baton Rouge and Bayou Sara, but not enough to be called *epidemic*. I believe no cases occurred at Grand Gulf. There was a pretty severe *epidemic* at Alexandria, on Red River.

At Memphis, several cases were landed from the steamboats, but no person contracted the disease from them. One *original case* occurred at this place, the following notes of which were kindly furnished me by Dr. James Young.

CASE.—J. S., a silversmith, aged about 30 years, of a robust constitution and sanguine temperament. Had lived about Memphis some seven years—was taken sick on the 14th of September, soon after returning from the country, whence he had ridden 25 miles in a buggy. At first complained of something like *a cold*—felt chilly and had pain in the head, back and limbs. Dr. Young was called to see him on the 15th, and found him with a hot skin, eyes red and watery, tongue covered with a white fur and moist, pains, &c. His fever subsided on the fourth day of his illness. Then there appeared slight hemorrhage from the fauces and anus, with suppression of urine, and subsequently *black vomit*. He died in convulsions on the 21st September and 7th day of sickness. He had black vomit 36 hours before death. The corpse turned very yellow after death.

Dr. Young having practised in one or two epidemics of yellow fever at Natchez, is familiar with the disease, and had no hesitation in pro-

nouncing this a *plain case*. I see no reason to doubt the correctness of his diagnosis.

Yellow fever prevailed to a considerable extent in our squadron at Vera Cruz and on the Gulf of Mexico. Surgeon Isaac Hulse, U. S. N., who is stationed at Pensacola, informed me that some interesting reports on the subject had been made to the Bureau at Washington City, by Passed Assistant Surgeon John Hornby and Assistant Surgeon A. N. Bell.

I have finished my task. I did not take up my pen to write a disquisition on yellow fever, but to endeavor to give some historical account of the most extensive epidemic of that disease that has ever ravaged this city. I crave the indulgence of my Professional brethren in this city, towards any defects they may discover in my humble effort. If they would do their duty, they would speak for themselves and not trust to the imperfect observations and representations of another. If I have done anything towards making up the medical history of the times, the object of my labor is accomplished.

I shall here append the following communication from Drs. Sloan and Gilpin, and afterwards those received from other places. It will be recollected that Dr. Gilpin's letter was published in the November No. of this Journal, under the head of "Health of the Country;" but I think it proper to re-produce it here, by way of completing the connection.

GENERAL HOSPITAL, New Orleans Barracks,
July 27th, 1847.

DEAR SIR :—I send you, in accordance with your request, the only observations made upon the subject of the yellow fever last year, by Surgeon Wood, U. S. A., that are on record at the Hospital. You will perceive from his remarks that a separate report upon this subject was forwarded to the Surgeon General's office, at Washington. No copy of this report is on file here, except one of my reports to Dr. Wood, Sept. 30th, 1847. This I send you, but fear it will be of little service.

I would remark, that nearly all the persons attacked in Hospital were under treatment at the time for some chronic disease. The only exception, I believe, to these cases was that of a woman, an Hospital Matron, who died from black vomit. She had not been sick previously, kept away from the city, but was exceedingly fearful of an attack of the fever.

My own opinion of the fever can be given in a few words. I believed it yellow fever, modified by the less virulent condition of the cause that existed in the city. Our location seemed to be on the outer circumference of a poisoned circle, where distance, perfect ventilation, and the best internal and external police rendered less violent the effects of the predisposing cause. I do not believe that the disease as it existed here was contracted in the city.

Accompanying this, I send you a rough estimate of the number of sick in this Hospital during the months the fever prevailed in New Orleans, with the proportional number of cases of yellow fever.

Very respectfully and truly yours, &c.,

WILLIAM J. SLOAN,
Assistant Surgeon, U. S. A.

Dr. E. D. FENNER, New Orleans."

Extract from Surgeon Wood's Quarterly report of Dec. 31st, 1847.

"In September and October many cases of yellow fever occurred, some mild and some exhibiting malignant symptoms, as black vomit, &c. No fatal cases occurred before the 1st of October. The small number of cases reported and the large number of deaths, is to be referred to the fact that the fever attacked many who had been registered and under treatment for other diseases."

Report made to Surgeon R. C. Wood, U. S. A., by Assistant Surgeon Sloan, Gen. Hosp., N. O. Barracks, Sept. 30th, 1847.

"The cases of fever that have come under my observation at this post during the prevalence of yellow fever in New Orleans, have differed from the ordinary fevers of this climate. These cases were characterized by the suddenness of the attack, the short duration of the fever, and great subsequent prostration. Persons were seized, without any premonitory indisposition, with violent rigors, followed by fever, with intense pain in the head, loins and limbs; the eyes were suffused. These symptoms yielded readily to treatment in twenty-four or forty-eight hours, leaving the patient extremely debilitated. I have no hesitation in ascribing these attacks to a modified condition of the poison of yellow fever in the atmosphere—modified in a ratio corresponding to the distance from its centre of action in New Orleans, and rendered comparatively harmless by the excellent police of the hospital and garrison, and the care observed in preventing exposure to the sun and transitions of temperature.

Fever of a similar type has prevailed in other localities near the city. In my own case, the attack differed from any I ever experienced. In Capt. Fenner's family, (all of whom were attacked,) the disease was remarked to be entirely dissimilar to the fevers of North Alabama, their former residence. I should hesitate in denominating these attacks yellow fever, since they were wanting that train of violent symptoms so characteristic of the disease, that tend to the very disorganization of the body; yet, at the same time, I must believe them a type of the disease modified as before mentioned.

In regard to the treatment adopted, it was simple and effectual. Submuriat. Hyd. gr. xv, followed by Ol. Ricini, was administered at the commencement. Frequent mustard pediluviae and the free use of warm drinks were very beneficial in promoting free diaphoresis. After the violence of the fever had abated, Sulph. Quinine, grs. xx, once or sometimes twice a day, were administered with good effect, without the necessity of further repetition. In some cases, after free purgation, when the fever had not abated, where there was still pain in the head,

with a quickened pulse and dry skin, twenty grains of Quinine were beneficial and followed by perspiration during the day ; the remedy was repeated with good effect. In one or two cases, I gave the Quinine and Calomel in combination, with good results. The local symptoms were relieved by cups to the temples, sinapisms to the epigastrium, ice to the forehead, &c. Total abstinence from solid food and perfect rest were strictly enjoined.

Whole number of sick in General Hospital, N. O. Barracks, for the months of July, August, September and October, 1847, with the number of cases of Yellow Fever, and deaths therefrom :—

Months.	No. of sick in Hospital.	Cases of Yellow Fever.		Deaths f'm Yel. Fever.	
		Months.	No.	Months.	No.
July, August, September and October.	411	August, 1847	2	October 8	2
		September "	9	" 12	2
		October "	8	" 18	1
		November "	1	" 21	2
		Total	20	November 10	1
				Total	8

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Dr. Gilpin's Letter.

"COVINGTON, LA., October 22nd, 1847.

"In answer to your letter of the 18th, which I did not receive in time to acknowledge by return mail, I take the earliest opportunity of replying to.

"Several families of Germans and Dutch, *who have resided in New Orleans two or three years*, came over here the latter end of August to escape the fever in New Orleans, and took possession of some uninhabited houses. About the fifth day after their arrival, one man was taken sick ; from what I can learn, no physician saw him for two days ; he died the fourth day, with black-vomit. In the meantime another man and his wife (also emigrants) were taken down and both died in a similar way. After a day or two, two of our citizens who had been with the sick, and attending on them, were also taken down with a similar attack ; and after this the disease spread generally through the immediate neighborhood where it is thickly inhabited ; from this it spread through the town, except that portion where I reside, which is separated by a small branch from the town. We have all kept well.

"I may safely estimate the number of cases from 160 to 180; out of which, with other diseases, there have been eleven deaths, one of which was from consumption, and ten of fever.

"I have seen eleven cases from New Orleans, who were taken sick either immediately on their arrival, or within a day or two after, some at Madisonville, some at Lewisburg, and some here ; those cases have not varied in the slightest degree from the patients I have attended here. I do not think it genuine yellow fever. The persons have generally been attacked severely ; the fever has commonly been subdued in thirty-six hours, but has left the system very much exhausted. In my own practice in the place, I have lost only one patient—that from relapse. I

have not bled in any instance, have only given Calomel to one person, and only cupped one—I have relied entirely on external applications, castor oil, injections and quinine.

“There have been several instances of persons coming from the country to the town on business, several of whom soon after have been attacked with fever, and generally have died, mostly of black-vomit.

“The fever has entirely subsided here for the last five or six days. In haste. J. G.”

II—*On the Yellow Fever of Rodney, Miss., in the year 1847*, by W. G. WILLIAMS, M. D.

To E. D. FENNER, New Orleans.

DEAR SIR :—Your letter of the 13th of December last was duly received, but owing to various circumstances I have been prevented from replying sooner. As I shall be compelled to be brief, I will endeavor to confine myself to a simple reply to your interrogatories, without indulging in any speculations on the subject of yellow fever, except so far as the nature of your inquiries may demand.

“Interrog. 1st. At what time did yellow fever make its appearance at your place, and what were the prevailing diseases immediately preceding it?”

Ans. It is my opinion that the first individual who died of yellow fever was a man who returned to this place from New Orleans, sick, about the 18th or 19th of July, 1847, and who died on the 23d. As respects the health of our town it was good at this time. This applies to the surrounding country as well as to the village itself.

“Int. 2nd. What was the condition of the place at the time, in a hygienic point of view?”

Ans. I have not observed any change of importance in this respect since 1837. Our place was, I think, in as cleanly a condition as it ever is during the summer and fall seasons. As regards this locality, I refer you to a letter furnished you on this subject, and published in the first number of this Journal, (May No. 1844.)

“Int. 3d. Please describe the commencement of the disease, its progress, general features, and decline. Did it disappear before frost?”

Ans. As I have already observed, I believe the first death from the disease occurred on the 23d of July. On the 27th of this month and on the 11th of August, two individuals were taken sick in the same or an adjoining room. These were his attendants. One of them, a white man, informs me he was confined about a week; had fever, but does not know what was the matter with him. This man was taken on the 27th July. The other, a negro woman, it is said had an intermitting form of fever. I did not see either of them while sick. Between this time and the 27th of August, some whites and a number of blacks, were sick in this immediate vicinity. On the night of the 30th of August I was called to see a man in this part of the town, and found him dying of hemorrhage from the bowels. He had been complaining of indisposition a few days, took his bed on the 27th and died of yellow fever on the 30th, with hemorrhage. An individual who was in his room when

I saw him, and much about him during his illness, was the next person I was called to see, sick of this disease, and this was on the 2d of Sept. On the 6th of this month I visited a gentleman, who resides in a distant part of the town, but is confined by his business to that part of the village where the fever cases occurred.

I will here state that in 1843, the first case of yellow fever occurred at the other extremity of the place, and that the disease for some days was confined to the family, in which this case occurred and their attendants. This has been the case, I think, this season. It was very circumscribed in its operation for a length of time, and confined to the immediate vicinity of the first case. On the 8th, we concluded we were on the eve of an epidemic, and by the 15th the disease pervaded the entire place.

On the morning of the 8th or 9th, I was called to see a man who had been ailing for some time, and who was supposed to be jaundiced. He was able to walk about his room at the time I saw him. His tongue was clean. His hands were cold. His pulse thready. His countenance was expressive of both despondency and indifference. His intellect seemed clear, but when interrogated, he replied in a deliberate, and abstracted manner. He became delirious on the evening of the day I saw him, and died with black vomit the day following. The next person I was called to see, was a man who was much with the individual above alluded to and occupied the next house. The progress of this case was rapid, as black vomit made its appearance in 30 hours from the commencement of attack. Up to this period, we had much contention respecting the true nature of the disease, but the circumstances of this case, particularly the vomit (which was the unmistakeable coffee grounds) convinced the most skeptical. The disease was characterized by the same general features as in 1843. No one who had the disease here in 1843 took it this season, and very few, probably not more than five or six who never had the disease, escaped. This is as true of our negroes as of our white citizens. The disease ceased for a time before frost, the mornings became very cool, and frost was reported to have been seen. This induced a number of our citizens to return, several of whom took the disease. This occurred as late as the 9th of November.

"Int. 4. Had you any quarantine regulations? If so, were they rigidly enforced, and with what benefit?"

Ans. No quarantine regulations have ever been established at any time at this place.

"Int. 5. Do you believe the fever originated in your place, or was it brought there? Give the grounds of your belief."

Ans., I do not believe that this disease originated here from local causes exclusively; some of my reasons for this belief, you will find in my reply to your first, third and next interrogatories.

"Int. 6 Did you see any evidences of the contagiousness or infectiousness of the disease?"

Ans. I think so; and in addition to what I have already said touching this point, I will state a circumstance that occurred in a family I attended. A man with a family consisting of a wife and two children, left the place, and removed five miles into the country. He took pos-

session of a small cottage, containing two small rooms. A man who was in his employ, left the village about the same time, and took one of these rooms. The man and his wife and two boys occupied one room, and this individual the other. This man took the disease on the 5th day after leaving Rodney, and died on the 8th. The family waited on him during his illness, and four days after, the two boys took the disease. They had been twelve days from town, and must either have carried the seeds of the disease with them when they left, or contracted it from the man who died in the adjoining room. Of many who fled and where sick in the country, none took the disease after the 6th or 7th day, except these two boys. The mother had the disease before she left the village, and the father on his return to it, after the recovery of his sons.

"Int. 7. Did you see any instances of other forms of fever running into yellow fever, or the reverse?"

Ans. I saw a case that resembled in some of its features congestive fever, and I think might have been mistaken for that form of disease. It seemed to me to partake of the nature of both congestive and yellow fever, corresponding in all respects to neither. I feel confident that the two diseases were blended in this case. I saw one individual who had intermittent fever, with some of the symptoms of the prevailing epidemic, and another who during convalescence had a periodic fever. Are not intermittence and remittance attributable to a physiological condition rather than to the action of a special cause? Do not various diseases assume these types, and why may not yellow fever? The phenomena that constitute what we understand by the term *fever*, are the same in small-pox, scarlatina, measles, yellow fever and bilious fever. Fever I apprehend to be nothing more than an expression of nervous-vascular irritation. A man has a chill; reaction comes on and we have a condition that we designate by the term *fever*. After a while an eruption makes its appearance, and constitutes it a case of small-pox, measles, or scarlatina; or the distinguishing marks of yellow fever may present, and we have a case of this disease. All these may assume an intermitting or remitting type. So far as the phenomena that constitute *fever* are concerned, they are the same in all; and I have witnessed as much diversity of opinion, and seen as much difficulty in christening measles, scarlatina and small-pox, as yellow fever.

"Int. 8. What remedies or course of treatment did you find to be most efficacious?"

Ans. The only material difference in our treatment of the disease during this epidemic and that of 1843, consisted in the more liberal use of quinine; which was attended with very satisfactory results.

"Int. 9. Have you ever seen any cases of the ordinary endemic fevers of your vicinity terminate in hemorrhage and black vomit?"

Ans. I have not.

I have thus hastily endeavored to comply with your request; and I assure you, that nothing but a disposition to oblige you has prompted me to the task.

Very truly, yours,

WM. G. WILLIAMS.

Rodney, Miss., February 10, 1848.

III.—*On the Yellow Fever of Vicksburg, Miss., in the year 1847.* By
B. J. HICKS, M. D.

TO DR. FENNER, New Orleans.

DEAR SIR:—Your letter of the 20th ultimo, in which you make inquiry in regard to the history of the epidemic fever which prevailed in our city during the past autumn, has been received, and with pleasure I furnish you with such facts as came immediately under my own observation. The hygienic condition of our town was such as would most certainly produce disease. The months of July and August preceding were unusually warm, the thermometer ranging from 90 to 94° in the shade, with frequent showers of rain succeeded by sudden bursts of sunshine. In a portion of the city there were many old uninhabited wooden buildings in a state of decay, with foul confined air between the floors and in the cellars, which must necessarily have been very inimical to health. The attention of the city authorities was urgently and repeatedly called to these sources of disease; but nothing was done to correct the evil; every hope and trust being placed in the establishment of what they considered a *rigid quarantine*; and whilst all were relying upon this shadow, and sleeping as they supposed in safety, the leaven of destruction was ripening from the putrid exhalations emanating from undoubted causes existing directly amongst us.

Our diseases until the 30th of August were of a mild intermittent character, but as soon as the burning rays of a southern sun had done their work, we had presented to us a fever of a most malignant character, proving fatal in most of the cases that occurred until a northern blast stamped a milder feature upon the face of the epidemic.

It is the opinion of many of our most experienced physicians that if the condition of the atmosphere had continued as in the commencement, we should have had a most devastating epidemic, more fatal perhaps than that of 1841; and that we were indebted to the Ruler of the elements for the mild character which the disease suddenly assumed as soon as the wind changed to the north; thus modifying the poisoned air and rendering it less noxious to man.

The first case of yellow fever occurred on the 31st day of August, at the house of Terrence Owen, in the north-eastern portion of the city, remote from the river, in the person of James Trynor, a young Irishman of temperate habits and good constitution. When first called to see him, I found him complaining of violent pain in the head; he stated that he felt as if an iron band had been drawn tightly across his forehead, insupportable pain in the back, aching of the joints, universal soreness of the flesh, as if he had been beaten with sticks, eyes highly injected, constipated bowels, great irritability of stomach, rejects everything taken, also mucus with occasional flocculi resembling small particles of blood subjected to the action of muriatic acid. These symptoms continued with but slight abatement until the 4th of September, under the most prompt and energetic treatment, such as repeated cups to the nucha and epigastrium, ice to the head and hot mustard pediluvia every four hours, active enemata, aperients of *massa hydr.* and *ext. colocynth.* compound. Irritability of stomach still existing—a blister plaster 10 by 10 inches was applied over the epigastrium, and as

soon as vesication was produced, the cuticle was ordered to be removed and blister dressed morning and evening with $\frac{1}{2}$ gr. of morphine and 10 grs. sulph. quinine—free suppuration was encouraged by dressing with the unguentum resinosum. On the 6th of September a favorable change took place. The patient gave evidence of the violent concussion the system had undergone during the progress of the disease, presenting a dusky lemon hue over the whole surface, which first commenced over the chest and around the mouth, quickly extending itself over other portions of the body. About the 9th of September the patient was discharged cured, convalescence being both complete and rapid.

A few days after Trynor sickened I was invited by my valued friends Drs. Broadnax and Balfour, physicians to the city hospital, to examine the body of a patient, Hugh Wilson, who had just expired in that charitable institution; of which invitation I availed myself. About five hours after death, in the presence of Drs. Nutt and Magruder and Messrs. Ford and Brown, medical students, I proceeded to make a post mortem examination.

The body presented a surface of a dark lemon hue, interspersed with dark colored spots. A black fluid had issued from the mouth, resembling coffee grounds. The stomach was of usual size, mucous coat highly injected, many of the vessels to the extent of being ruptured—of soft consistence—contained about $\frac{3}{4}$ x of a dark colored fluid, with granulated particles resembling coffee grounds which did not color water when mixed with it, but would settle at the bottom of the vessel, leaving the water perfectly clear. The small intestines contained a dark colored fluid similar to that found in the stomach. Mucous coat inflamed, but to a less extent than that of the stomach. Large intestines contained a fluid resembling the sediment of port wine; mucous coat injected; of a more florid red than that of the stomach and small intestines. Nothing remarkable about kidneys, except presenting a yellow tinge. The liver somewhat softened in consistence, the great lobe of a spotted yellow color; left lobe of a more regular orange hue; gall bladder distended with a dark tenacious fluid, of the consistence of tar. Throat and head not examined.

Wilson had been engaged in laying brick in this city for some months previous to his illness; had no communication with any vessel ascending the Mississippi river; had not visited any sick person for some weeks; neither had he seen, or been near the quarantine depot. He was admitted into the hospital on the 3d day of September, and died on the 6th. The history of his case I have not been able to procure.

Whilst returning from this trip of investigation, my friend Dr. Magruder informed me that he had a patient laboring under a similar disease, the Rev. Dr. Teavel, whom on the following day I was called to see. I found him laboring under incipient black vomit and in a sinking condition. He expired the next morning, after throwing up large quantities of this fluid. He had been for some time engaged in the duties of his station, in holding a protracted meeting, and had not been near the quarantine depot, steamboat landing, or visited any sick person for some weeks. His residence was in the centre of the square upon which the old uninhabited buildings in a decaying condition spoken of above were

located, and immediately north of an old theatre which had been a short time previously torn down and removed by order of the city authorities, disengaging large quantities of poisoned air which had been the product of fermentation for years, which no doubt was wafted by the southern breezes directly into his bed chamber and thus in part poisoned his system. Several other cases occurred upon the same square, simultaneously with that of Dr. Teavel; a Jewess, Henry Wirtz, and Mrs. Jane Porter, all of which terminated fatally; the first and last about the 5th day of the attack, the second lingered for several weeks, having relapsed. Not having been physician to the foregoing cases, I cannot give the history and treatment, but only mention them as being interesting in connection with the origin of the disease, not one of them having had any intercourse with steam boats, quarantine depot, or even been absent from the city for months previous, and up to this period no person sick of yellow fever had been permitted to enter our city. The attentive health officer strictly and faithfully discharged his duty. The fatal tendency of the disease produced great alarm and excitement amongst our citizens, many left their homes and fled for safety to the country. But in a few days the wind shifted to the north, and the fever assumed a milder type, which caused many of our citizens to return to their homes and resume their daily occupations.

On the 19th of September I was summoned to visit Daniel Montgomery. I found him complaining of severe pain in the head, eyes highly injected, pain in the back and lumbar region, soreness of the flesh, constant aching and tired feeling in the extremities, great irritability of stomach, vomiting incessantly.

Treatment.—V. S. $\frac{3}{4}$ xx, pills of blue mass and comp. ext. colocynth, which were immediately rejected; twelve leeches applied to the epigastrium, active enema; ice to the head and hot mustard pediluvia every four hours.

10th—Patient still restless, stomach irritable; enema had acted three times; complained of violent pain in the eyes and forehead; irritability of stomach still very distressing. Blister plaster 10 by 10 ordered to epigastrium, leeches to the temple, iced gum water for drink, mustard pediluvia ordered to be repeated.

11th—Irritability of stomach still continues; distress insupportable; throws up large quantities of mucus; entire absence of bile; blister had drawn well; remove cuticle and dress with morph. and sulph. quinine.

12th—Patient passed comparatively a quiet night; pulse 90; skin soft; morph. and quinine dressing continued.

13th—Black vomit; appeared to be sinking; ordered enemas of camphor grs. v and quin. sulph. grs. x, every four hours, in mucilage. This prescription arrested the vomiting, but as soon as the vomiting ceased, the patient complained of partial loss of vision and delirium.

14th—Pulse full and regular; skin soft and moist; tongue furred in the centre, with red edges; hemorrhage from the nose and gums; total loss of sight; cannot distinguish a bright sunshine from the darkness of a clouded night; eyes had the appearance of being natural with the exception of slight dilatation of pupils and an injected condition of the conjunctiva. Blisters ordered to the temples and nucha, suppuration of

the blistered surface on the epigastrium encouraged by dressing with the unguentum resinosum, brandy toddy and arrow root.

15th—Pulse more feeble; hemorrhage from the bowels; subsultus. Camphor and sugar of lead enemas ordered every four hours; brandy toddy and arrow root continued.

From this time the patient continued to sink gradually from the exhausting hemorrhages from the nose, gums, bowels and urethra, until the 24th, when he expired, after having been sustained by the extraordinary means adopted after the appearance of black vomit—I am of opinion that the patient would have sunk on the 5th day of his attack had he not been sustained by the camphor enemas. I have never witnessed a recovery after the appearance of black vomit, except by the camphor treatment.

On the 29th day of September, I was called to see Thomas McConnel, whose residence was on the same square as that of Dr. Teavel. I found him entirely delirious; could not keep him in bed; pulse 128, full and hard; violent pain in the head and back; eyes highly injected; great restlessness; aching feeling in the extremities; universal soreness of the flesh. Ordered blood-letting $\frac{3}{4}$ xxx. R. massa hydr. comp. ext. colocynth. a a. $\mathfrak{D}$ i., make 8 pills. Take four at once, four in three hours; ice to the head; hot mustard pediluvia.

30th—Pulse 100, skin soft, medicine had acted twice; tongue red on the edges, furred in the centre; pain in the back; tired, aching feeling in the extremities. Ordered cups to the epigastrium; warm mustard bath, mucilaginous drinks.

Oct. 1st—Patient spent a restless night; suppression of urine; epistaxis; irritable stomach; rejects every thing taken. Ordered the following enema. R. spir. turpentine 3 ij, Tr. assafoet. 3 ij, mixed in $\frac{3}{4}$ iv. mucilage; blister plaster to the epigastrium.

2d—Hemorrhage from gums and nose; irritability of stomach subsiding, slight passage of urine, enema had acted once; patient restless; remove cuticle from blister; dress with $\frac{1}{4}$ gr. sulph. morph. and grs. x of sulph. quinine, morning and evening.

3d—Hemorrhage from gums and nose very much increased; considerable hemorrhage from blistered surface, so much so as to wet the patients bed with blood. Dress blister with 4 grains sulphate of iron, grs. x. sulph. quinine and $\frac{1}{2}$ gr. sulph. morph.

4th—Excessive hemorrhage from the gums, nose and bladder, universal yellowness of the skin, pulse small and frequent; nearly suffocated with the bleeding from the gums whilst sleeping; bloody froth issues from the mouth; friends suppose him to be dying. Blister dressed with sulph. quin. and sulph. ferri.; ice to the head; sinapisms to the extremities.

5th—Slight improvement; patient more rational; slept calmly several hours; hemorrhage still continues from nose and gums; morph. and quinine dressing for blister with the unguentum resinosum; this readily caused suppuration which invariably improves the gastric symptoms. The quinine and morphine dressings were continued until the 8th, when the patient was discharged cured and, as was generally the case, convalescence was rapid.

Blood letting, cupping or leeching, mustard baths, enemias and the endermic use of quinine and morphine in the advanced stage of yellow fever, have proved more successful than any other course of treatment adopted by the physicians of this city, so far as the writers observations have extended. In fact, in all cases in which the stomach, the main citadel, has been taken possession of by disease, the endermic use of remedies is the principal reliance for a successful issue of the case, and deserves the consideration of the profession. Many patients have been hurried out of existence, by crowding an already irritated stomach with nauseous drafts. I have related the three cases above, to give some idea of the general character of the disease, but the greater number of the cases were of a much milder type. All cases were initiated with the same symptoms, differing in degree; some would readily yield under the milder remedies, other cases would assume a more malignant or typhoid character. In the mild cases, there was an early appearance of a prickly heat eruption, which was a sure indication of a speedy convalescence. This eruption never occurred in the malignant or typhoid cases, but occasionally fine petechiæ, which I supposed to be an effort of nature to throw the eruption to the surface. But her powers being too feeble, she failed to accomplish her design. All cases of disease ran into the epidemic type. The yellow fever commenced at the close of the month of August. In the course of ten days it had become decidedly epidemic, prevailing to a very great extent. Nearly all of our citizens felt its influence, though the disease had become remarkably mild, not more than three per centum proving fatal.

The disease subsided about the 25th of October, at which time we had a slight frost, though a few sporadic cases occurred until the 10th of November, when all nature was clothed in ice.

Our city has been visited with epidemic yellow fever during the years 1841, '43 and '47. That of 1841 was of a most malignant typhoid character, one case out of every four proving fatal under the best treatment. The epidemic of 1843 was more limited, of decided inflammatory symptoms, and less fatal; about one death in six. The epidemic of 1847 may be properly styled *mild epidemic yellow fever*, being remarkably easily managed, and only about 25 deaths out of nearly 800 cases. In consequence of the mild character of the disease, many doubted whether it was yellow fever or not, and some difference of opinion existed amongst the physicians, as was the case in 1841, until nearly 200 of our citizens had been taken to the burying ground. Nearly all of the cases that terminated fatally, closed with black vomit, except those in which excessive hemorrhages occurred. I have never witnessed black vomit in the endemical fevers of our vicinity, but have often met with miasmatic fevers of a heavy grade in which hemorrhages from the nose, gums and bowels were not unfrequent in the closing stage of the disease.

From all the facts that we can gather from the history of yellow fever as it has heretofore prevailed in this city, we have not been able to trace a single instance in which it showed the least disposition to manifest a contagious character.

In 1837, when the city of Natchez was laid waste by its devastating influence, numerous cases in every stage of the disease, from the initiary

symptoms to the black vomit point, were landed from boats and ushered into crowded filthy boarding houses, with ten or a dozen in a room, and in not a single instance did a nurse or attendant become affected with the fever, though many of those landed died with black vomit. Such was the case in 1839—the writer attended numerous cases that were landed from boats from Natchez, our city being then crowded with paupers who were flying from disease and contagion as they supposed, as is usual in our southern cities when an epidemic breaks out; but in no instance did he witness any disposition to contagion.

In 1841 many citizens fled from Vicksburg when the epidemic was declared to exist. A large portion of them sickened at different points, and many died; Judge Anderson in Bolivar county, Dr. Weller and his son in Raymond, Messrs. Vail and Davenport in Clinton, all of whom had numerous attendants and in no instance did any of their friends or nurses sicken with the disease, notwithstanding they all died with black vomit, the ripe stage of contagion, if contagion could be.

During the past season, in vain did the advocates of contagion look for a source of disease from importation. They asserted that Dr. Teavel had visited the quarantine depot and had thus taken or contracted the disease, and that Trynor did not have yellow fever because he did not die. But it was ascertained that Dr. Teavel had not been within a mile of the quarantine landing since its establishment. The evidence of local origin was so plain that the advocates of contagion became quiet, our city authorities suspended the quarantine regulations, commerce resumed her sway, the resounding echo of the signal cannon relapsed into silence and steam boats were permitted to glide to our wharfs, all being satisfied that we were suffering the penalties of sanitary neglect.

Your friend,

B. J. HICKS.

Vicksburg, Mississippi, January 20th, 1848.

IV.—*On the Yellow Fever of Natchez, Miss., in 1847.* By SAMUEL A. CARTWRIGHT, M. D.

DR. FENNER,

DEAR SIR :—I have just returned from a three-month's tour to the North, for the benefit of my health and to pick up medical information, and I find among my letters one from you so far back as the 17th of December last, which I now proceed to answer.

In regard to your questions about yellow fever of last year, I have to inform you that we had in Natchez only a few sporadic cases.

I was called to see Mr. Haffner, a German, from the Rhine, who had been living in this city for a year or two, and had always been healthy—a tailor by trade. He had not been out of town during the summer, nor any where among the sick. He resided in a low, confined place, and the back yard was very dirty. He died on the fourth or early on the fifth day, with genuine yellow fever, on the 2d. of October. I saw another case which died on the 6th of November, on the third day. He was a stout Irishman—had not been out of town or any

where to contract the disease. He lived in a close, confined room, and dirty, ill-ventilated back-yard; his name was O'Rourke. He lived in a different part of the town from Haffner, and a month or more occurred between the two cases. These were the only two cases I saw in town; but some three or four other sporadic cases occurred. A German woman died in October of yellow fever, and my overseer, also a German, living on my plantation, 6 miles from Natchez, in La., went to see the woman with yellow fever when he was exposed to the same local causes which gave it to her. Late in October last I visited New Orleans. On my return, Mr. Grejer, my overseer, I found had died, during my absence in N. Orleans, with yellow fever. Black vomit very copious and frequent. None of my negroes nor any of the persons who attended him took the disease; nor did any of the persons living near O'Rourke and Haffner take it. The reason why so few had the disease, I attribute to the fact that most of our people are acclimated and are not susceptible to it. For instance, we had the fever in the epidemic form in 1837 and 1839, and in 1844 we had a good many straggling cases. We have had little or no accession to our population since, and consequently no subjects for it. We have had what is called a quarantine, and may attribute our exemption since 1839, (1844 excepted,) to the quarantine. Those persons trace or pretend to trace the cause in 1844 to Woodville or your city, and not being able to trace the cause of last season to any foreign origin, they doubt whether the cases above stated were yellow fever. I think there is no doubt of it.* In fact, for the last twenty-five years, I have seen cases of yellow fever almost every year, originating, as I believe, here under some strong local or predisposing causes.

In one of your questions, you ask if yellow fever disappears before frost? I have known it to continue for some weeks after frost, when it begins late in the season—and quit before frost when it begins early. I have seen many cases after several frosts. The quarantine regulations were not rigidly enforced. People would land on the other side

* In confirmation of the testimony here given by Dr. Cartwright, in regard to the effects of quarantine at Natchez, I will add the following memoranda, kindly furnished me by Dr. James Young of Memphis, Tennessee, a talented and accomplished physician, who resided in Natchez in 1844. They are given in his own words.

F.
“NATCHEZ, 30th September, 1844.

Called to see Dr. Craig this evening at 7 P. M. The Doctor informed me he had been sick three days. October 1st.—He commenced throwing up black vomit at 5 o'clock, P. M. 2d.—At 5 o'clock, A. M., threw up black vomit, again at 7 o'clock, and died at 10 o'clock, P. M. Dr. Craig was seen by Dr. Cartwright and Dr. Davis.

Samuel. R. Hammitt, attacked with *yellow fever* 19th October,—sick five days.—*Recovered.*

T. Cranch, attacked with *yellow fever* 28th October,—sick ten days.—*Recovered.*

The three cases of yellow fever occurred in Natchez, when the city was generally healthy.

Neither of the patients had been where the disease was prevailing, nor had either of them seen any individual laboring under the disease. J. Y.”

of the river and cross over in the Natchez ferry boat, or below town or above the quarantine ground. One of the landing places was humorously called the upper quarantine.

5th and 6th Interrogatory.—I believe the yellow fever originates in Natchez. From 1825 to '35 we had no quarantine, and the yellow fever was imported into Natchez almost every year, but it never spread, and no one ever took it from the patients themselves. The boats sometimes seemed to be infected.

9th Interrogatory.—“Has yellow fever prevailed at your place before?” Yes—epidemically in 1817, 1819, 1823, 1825, 1837, 1839. A few cases almost every year, and a great many cases in 1829 and 1844—but not epidemic.

I have, in answer to your 10th question, seen a few cases of the ordinary fevers terminate in hemorrhage and black vomit,* without being, as I thought, yellow fever, properly so called. Yellow fever I take to be a disease separate and distinct from bilious fever. It gives a peculiar expression of countenance to the patient. It is I believe an American disease. If contagious, it is not more so than the common bilious fevers. All fevers are more or less contagious, and all diseases for that matter, in one sense of the term, as they vitiate the atmosphere.

Very respectfully, your obedient servant,
SAMUEL A. CARTWRIGHT.

V.—*On the Yellow Fever of Houston, Texas, in 1847.* By WILLIAM McCRAVEN, M. D.

HOUSTON, TEXAS, Jan, 12, 1848.

DEAR SIR :—I did not receive yours of the 10th ultimo, till about the 1st instant, and take pleasure in furnishing in reply such information as the data in my possession will permit. In answer to your first interrogatory: The first case of yellow fever occurred here on the 4th of October. A case of rather doubtful character had occurred four days previous and was then on hand. As it excited a good deal of interest here, and, as I think, there can be little doubt that it was yellow fever, at least in the end, I will give some account of it. Mr. V. had been indisposed at Galveston for three or four days. Yellow fever was then prevailing at Galveston, though not admitted to be epidemic. He had two or three light fevers then, as he informed me, and took some purgatives and quinine, but most of the time was actively and laboriously employed. He arrived here on the evening of 30th September. He had fever that night, but was up in the morning, and took dinner with

* The fact here stated by Dr. Cartwright, is confirmed by Dr. Thomas Fearn, of Huntsville, Ala., and Mr. W. P. Hort, of this city, two distinguished physicians now retired from the practice. I obtained their testimony in recent interviews with these gentlemen. I myself have seen cases of *bilious fever* terminate in fatal hemorrhage from the mouth and bowels, though not black vomit, in Hinds Co. Miss. I might differ with Dr. Cartwright as to yellow fever being “a disease separate and distinct from bilious fever,”—but will take some other occasion to argue the point.

some appetite. In the afternoon he was taken with rigors and violent pain in the head, back and extremities, attended with fever. In this condition I was called to prescribe for him. He was alarmed and restless—thought he had yellow fever and was apprehensive of the result. His pulse was a good deal accelerated, quick and compressible—his skin hot and dry—his eyes suffused and red, tongue moderately coated with whitish fur, and red on the tip and edges. The symptoms were strongly indicative of yellow fever. But the preceeding symptoms—the decided remissions which had evidently occurred—the time which had elapsed since fever first made its appearance, threw doubt on the case, and rendered a diagnosis uncertain. It was evident to me he was not in the 4th or 5th day of yellow fever. I enquired if his eyes had been previously inflamed. He told me they had been red and somewhat inflamed, but had gotten well.

I regarded the case as doubtful, but determined to treat it as yellow fever. The fever continued with little abatement until the third day. The pains were mitigated, but not relieved entirely. On the 4th day, fever was almost gone, but I did not think him entirely free till the 6th. The skin and eyes were decidedly icterose. He was desponding and very nervous. Dozed a good deal and muttered in his sleep, which was light and interrupted. Often changed his position. For about 24 hours he appeared much improved. Tongue cleaned off—had some appetite and read a newspaper. Skin moist and heat well diffused. His gums had been bleeding a little for a day or two. On the evening of the 4th he had an operation which showed appearances of blood. He rested tolerably well and had no further movement till next morning, when his discharge consisted almost entirely of blood, but without any pain, except a little uneasy sensation just before a movement took place. The tongue became healthy in appearance. The extremities continued warm; skin moist, and pulse very little above the natural standard, ranging from 80 to 90. In despite of all remedies the hemorrhage continued, and he died on the 10th, retaining his mental faculties till a few hours before his death. Indeed, for a day or two previous, his mind appeared less clouded than in the earlier stages of his illness. I was informed by one of his attendants, that just before he died he threw up black matter, which was probably black vomit; but as I did not see it, and the attendant had never seen black vomit, I am unable to speak positively upon the subject,

This was probably a case of yellow fever supervening on ordinary remittent, and was almost an exact counterpart of a case, the first which originated here in 1844.

On the morning of the 5th Oct., I was called to see Mrs. W. She was taken the evening previous with chilliness, attended with fever and excruciating pains in the head, back and limbs. I saw her in this condition. There was but little redness in the eyes. In the evening she was better. She complained of ringing and exceedingly disagreeable feeling in her head, from the effects of quinine. Next morning she was free from fever, and continued so, notwithstanding the excitement and distress she passed through afterwards. This was a slight case, though the symptoms were severe at first. If it had

occurred alone, I should not have regarded it as yellow fever. I have seen such however.

Mr. T., her brother, was ill in the house at the same time; he was taken with the same symptoms on the morning of the 4th. Sick 24 hours when I saw him. His eyes were quite red; pulse frequent, quick and compressible; tongue fiery red on the edges and tip, partly coated and partly as though it had been scalded and the epithelium peeled off; restless and thirsty. His was a well-marked case of yellow fever of a bad type, and as his constitution was very delicate, I augured unfavorably of its termination. It did not run a regular course however. Fever declined gradually, but did not leave him till the 6th day. His tongue at one time had been very dry and almost black, especially on the denuded portions, became moist and looked well. He had become very yellow—had some appetite and no thirst. He was cheerful, and I thought almost out of danger on the 10th. But that night he grew worse. I found him on the morning of the 11th, much to my surprise, with dry hot skin, great thirst and restlessness; tongue again dry. He died on the evening of the twelfth, with all the characteristics of yellow fever, including black vomit, which occurred just before his death.

Mr. W., of very feeble constitution and nervous temperament, was taken on the evening of the 6th, two days after his wife, with severe symptoms. Fever abated on the 3d day. Got out of bed and lay on the sofa. Being a self-willed, obstinate man, with some very strange notions, he did just what he pleased. Amongst other imprudences, he ate some very indifferent water-melon. On the 10th, he dressed himself and went into the adjoining house, as his own family were all sick, and he wished to be quiet. On the 11th, he went to his store and remained till evening. This was the 6th day from his attack. He returned home and spent a restless night. In the morning I found him in bed, perspiring freely; surface warm, pulse about 60, and of pretty good strength. He appeared strange and very slow of comprehension. He gradually became more and more stupid—complained of nothing—had no fever, and extremities warm—drowsy—sleep troubled—muttering—eyes half closed. He became unconscious towards evening and very restless, often attempting to get out of bed, and requiring considerable force to restrain him. Skin and eyes yellow. He died on the night of the 13th, eighth from the attack—did not vomit after his relapse.

Miss W., daughter, and Miss T., sister of Mrs W., were taken on the 7th with same symptoms, but of milder character. Miss W.'s fever lasted two or three days, and left her languid and listless for several days. Her eyes were very yellow; her skin of a dusky reddish hue, as though the superficial capillaries were congested—a circumstance which I have frequently observed in yellow fever. And hence, probably, arises the great danger of exposing convalescents to a draught of cool air before the system has had time to recover from the shock. The power of generating heat is so feeble that the system will cool down under the influence of a current of cool air, almost like an inanimate substance. I came near losing a patient under such circumstances in 1844. All symptoms of the disease had disappeared. He was lying between two windows and had fallen asleep without cover. He was

almost as cold as a corpse and nearly pulseless, yet seemed unconscious that anything was wrong. It took an hour or two of the most active stimulation external and internal to rouse him. No serious inconvenience seemed to follow. But to return to my subject. Miss T. had very little fever after the first day, and indeed not much then, but like all the rest, was chilly and had intense pain in the head. She insisted for a day or two that nothing else was the matter. For several days her stomach was very irritable, and most that she threw up was of a grass green color. She and Miss W. both recovered, but were much debilitated for several days. Had Miss T's case occurred alone, I should not have regarded it as yellow fever; but occurring as it did with four others, all at the same time, in fact every member of the family was down at once, I could not but consider it of the same character with the rest. She was not much jaundiced. A young man who had been boarding in the family was taken about the same time—said to be yellow fever. I did not see him. They were all unacclimated.

On the 20th and 22d, two cases occurred in the adjoining house—fever with rigors at the commencement; violent pain in the head, back and limbs; red watery eyes; fever abating on the third day, with much debility; skin and eyes yellow—in fact, regular cases of yellow fever throughout. Both recovered. These are fair samples of the cases as they came under my notice. They were not numerous at any time in my practice. It could not, I think, at any time be fairly regarded as an epidemic, and the best evidence of this fact I think was, that our ordinary fevers prevailed at the same time and greatly predominated over the yellow fever cases. This was not the case in 1844. In August of that year, I scarcely met with a case of fever that did not distinctly present the features of the epidemic. From about the time of the last mentioned cases till the middle of November, a good many scattering cases were reported. Quite a number of Germans, recently arrived and settled around the suburbs of the town, died. What proportion had yellow fever I do not know, as they were mostly attended by German physicians, who I think did not regard the cases as yellow fever; but whether they were familiar with the disease or not, I am unable to say;—can only speak of two cases which I was requested by some charitable ladies to visit, as the family had not much confidence in the attending physician. I was at the time informed that one of the patients was throwing up black vomit, and it was not expected that anything could be done for him, but something it was hoped might be done for the other case. When I called one was just dead, and while I remained, the other threw up black vomit and died next day. I was informed that the other had vomited matter of precisely the same appearance, and from the signs which I saw, I have no doubt it also was black vomit. The attending physician, I believe, though I had no conference with him, called the cases *congestive fever*, at least I so understood from the father, an intelligent German, who could speak a little English. From these cases I supposed that probably a good many of the Germans had died of yellow fever; but this is only a supposition. The cases among the American part of our population were not numerous, but the deaths in proportion were considerably greater than in 1844. I saw one case, a black, on the 1st

of December, which bore a strong resemblance to yellow fever. I would observe here, that some authors, I believe, deny that the blacks are subject to the epidemic, which is undoubtedly a mistake. They were subject to it here both in 1839 and 1844. I attended a number the latter season, as well-marked cases as any I saw. The disease, however, according to my observation is seldom fatal with them. The prevailing disease at the time that yellow fever made its appearance, was ordinary *remittent* or *bilious fever*, which, as I before observed, continued to prevail throughout the season. Dysentery had been more than usually prevalent for the last year. In the preceding winter, *typhoid pneumonia* prevailed and proved fatal in a good many cases, especially among the blacks. During the past twelve months I have had some six or eight cases of *typhoid fever*, the first I have met with in Texas: all in children under 12 years. They continued from 15 to 30 days. All but one recovered. In the early part of summer, also, we had a remarkably large number of cases of *jaundice*, of which I suffered myself, much to my surprise, as I was at the time of my attack in perfect health, not having been confined to bed a day since I had yellow fever here in 1839. It has appeared to me that for some years all our diseases were becoming more and more ataxic. I seldom meet with cases in which the lancet or any other mode of depletion can be judiciously employed to any great extent. Mercury, too, perhaps for the same reason, I find a very doubtful remedy in fevers. In fact, I seldom now employ it in fever, except in combination with mild purgatives, given occasionally in small quantity.

I have already answered your 1st and 3d questions.

2d. The town was in a tolerably cleanly condition. It is situated on the border of a flat prairie, and although there is no marsh in its immediate vicinity, the prairie is so level, that with the exception of that portion of the town near the bayou, it drains slowly. It is, however, much better drained now than formerly; and the soil is of such a nature, that when well drained, it dries rapidly. Our prevailing winds blow over the open prairie about a mile before reaching town. On the north of the town, the country is timbered for three or four miles, and our north winds are always regarded as unfavorable to invalids, particularly in the fall season. If not attended with rain, they are remarkably dry, and in summer hot.

4th. We have never had any quarantine establishment.

5th. The question whether yellow fever is of foreign or domestic origin, is one on which I have never been able to satisfy myself fully. I am not a partizan of any particular theory on the subject, but have diligently sought for facts on which to base an opinion. I was until last year strongly inclined to the belief, that with us it was always of foreign origin. In 1839 I was here, but not professionally engaged. I had the disease about the 22d of October. It prevailed then in a malignant form. It commenced, I think, about the 1st of September and continued till near the last of November. Our population was then probably less than 3000, and I think there could not have been less than 200 victims to the epidemic. The town was then in a remarkably filthy condition. It was crowded with persons out of employment and destitute of means, many of them intemperate. When taken with the

fever, they had neither the necessary comforts nor the means to procure them, and sickness was so general that it was exceedingly difficult for a stranger to get proper attention even if he had money. It is not surprising that many died. I cannot speak positively respecting the first cases which occurred then, but think they came from New Orleans. Such I know was the impression on my mind at the time, but I cannot now recall the grounds on which it was based. This was the first appearance of yellow fever in our town, and it prevailed very generally, as but few of our citizens had had the disease, and most of those who had not, were attacked. It did not disappear till long after frost. Frost always I think operates as a check upon yellow fever, but if warm weather sets in afterwards and continues long, the epidemic will revive again and may be as bad as ever. A good frost I believe destroys the cause.

The second appearance of yellow fever in this place, was in July, 1844, and of this epidemic I can speak with confidence, as I was actively engaged professionally through the whole of it, and several of the first cases were under my charge. The first case arrived from Galveston, on the 10th of July, I think, and placed himself under my charge—he was then in the third or fourth day of his attack, without fever, but very nervous and greatly debilitated. He continued so for several days and recovered. I had three or four other cases under similar circumstances—all having taken the disease in Galveston and come up in the boat. About the last of July I had a case which occurred in town and ran a course precisely parallel to that of Mr. W. cited above, and terminated in hemorrhage and death. In this case yellow fever seemed to supervene on another disease. About this time I saw a case of black vomit incidentally, but as the patient was not under my care, I do not know its history. About the 1st of August cases began to appear frequent, and in a few days the epidemic became general and spread rapidly through town, the cases from Galveston appearing to act as *foci* of infection. By the 1st of September it had nearly exhausted itself in the densely populated portions of town, but lingered about the outskirts for another month and disappeared long before frost. The latter part of the fall was healthy. During its prevalence, scarcely any other form of disease manifested itself. Few unacclimated persons escaped, and none to my knowledge (and I made diligent enquiry) had the epidemic who had suffered a previous attack. The epidemic was comparatively mild, and the proportion of deaths small, though it presented a malignant aspect at Galveston. The fever ran from one to three days, and although it left the sufferers weak and exhausted, convalescence was generally rapid and complete in a few days. Black vomit was not a very common symptom even in fatal cases. Death I believe seldom ensued before the sixth or seventh day. The town was in a much better condition than in 1839, but worse drained and less cleanly than at present; and there is still great room for improvement.

As to the origin of the fever here the last season, all the facts seem to point directly to a *domestic origin*. I could trace no connection between the family of W. and any other case of yellow fever. Mr. V., it is true, had been sick four days when the two first cases of yel-

low fever occurred ; but they occurred in a distant part of the town, one of them in a female who seldom went out, and I can trace not the slightest connection between them and V. The only plausible mode by which it could be traced to a foreign origin, would be to ascribe it to goods received from abroad, (W. and T. being merchants,) but this I think extremely improbable. On a careful review of all the circumstances, I can come to no other conclusion than that it was local in its origin.

6th. I have never believed yellow fever to be strictly speaking contagious. But I have frequently thought I could trace evidences of infection. My experience the last season has added to these evidences, yet I will not pretend to aver that they are not all deceptive. It is certain, however, that in nearly all the cases that occurred for two or three weeks, a connection either direct or indirect with the W. family could be traced ; for instance, at least six of the attendants had the fever and another who visited the family once or twice, had it. A young man and his two sisters visited and watched with them regularly. He was present when T. died, and asked me if the attendants were liable to take the disease ? I told him I thought they were more likely to do so than others, but that if he was taken ill and would take medical advice at once, I thought he need not feel any alarm, as he was accustomed to a hot climate. In a few days, he and both his sisters passed safely through the fever. The physician who attended these cases was unacclimated ; he was soon after taken with the fever and died. Two persons at least, who visited the house while he was ill, had the fever and one died. The lady in whose house Mr. W. died and her daughter were taken about a week after his death ;—both recovered. A woman who assisted in nursing the W. family soon after died with black vomit. Not long afterwards, her husband also died of yellow fever, as was said ; but I do not know whether he had black vomit or not. It is at least remarkable in a season, when so few cases occurred, that so many of them should have had intercourse with yellow fever patients, and in fact I could trace such intercourse in every case which came to my knowledge for several weeks. It is true, that all the unacclimated who had intercourse with the sick were not attacked ; but I believe most of them were. The evidence which I have been able to collect is strong in favor of some kind of infection, growing out of intercourse with the sick. The more strictly I have scrutinized the matter, the stronger has the proof appeared.

I give you the facts for what they are worth.

7th. The case of V. I regarded as a different form of fever, terminating in yellow fever. I also met with a case, the first I saw originating in town in 1844, and to which I have already referred, as terminating in the same way. I have also met with some cases in which yellow fever ended in a slow remittent form of fever, which lasted 8 or 10 days. These cases, however, have been rare.

8th. In reference to the treatment of yellow fever, the cases which occurred in my practice last year were so few that I would be unwilling to deduce any general rules of practice from them. Therefore, most of what I have to say upon this subject will be based upon my experience in 1844, which was ample, and I think successful, as I

did not lose more than two patients, whom I visited the first day; and I attributed most of the deaths which I witnessed to too active purgation with drastic medicine. There is a strong tendency in the epidemic to localize itself on the bowels. Strong purgatives seemed greatly to increase this tendency. Such cases I always found very intractable. I doubt the efficacy of the mildest purgatives in yellow fever, and am strongly inclined to believe we would be more successful without them. I have seldom used the lancet in yellow fever—not in any case last year—though in 1844 I bled two or three patients with manifest benefit.

I have a good deal of faith in quinine in the incipient stage, and prefer giving it freely during the first 24 hours. I have little faith in mercury. I give it in small quantity in the commencement of the attack—usually a few grains of blue-mass; I do not, however, rely much upon it. I order a warm bath; general if convenient, if not, a hot mustard bath to the feet—followed by flying sinapisms, often repeated. Cups dry or wet *pro re nata* to the nucha, temples, epigastrium and spine—wherever in fact there is much local pain. I generally give a mild aperient in the beginning, assisted by an enema, if the bowels are costive. I am not, however, satisfied, as I before remarked, that this purgative plan, however mild the means, avails anything in most cases of yellow fever. It is hard, however, to overcome the prejudices of people in its favor. I usually find the bowels easily moved. If the skin is dry, I give some diaphoretic, as liquor ammoniæ acetatis with spt. nitre, nitrate potass, and a little sulphate of magnesia in mint water as a menstruum, and if the stomach is not irritable and pulse rather full, add a little antimony or ipecacuanha, to reduce its action and determine to the surface. I have the stimulating pediluvium often repeated and also the sinapisms and stimulating frictions, and if there is much heat about the head, ice or cold water constantly applied till the action of the heart is reduced. If there is tenderness of the epigastrium or abdomen, after cupping I apply a large warm poultice, which I think generally far preferable to blistering. I prefer warm drinks, generally, but there are some cases in which cold drinks are perhaps preferable, and I allow patients frequently to swallow ice in small quantities, if the thirst is urgent, and especially if there is much irritability of stomach. My main reliance is in a good active intelligent nurse—full doses of quinine till the system is brought well under its influence—the frequent and diligent use of external stimulants and cupping according to circumstances. When a remission takes place, if the patient takes it with a relish, I generally give brandy *ad libitum*, till some strength returns. I believe brandy is the best stimulant we can give and most agreeable to patients generally. But I think I have seldom found benefit from its use unless the patient took it cheerfully—and on the other hand, when he took it with a relish I have almost universally found it serviceable.

I have some confidence in small doses of saline medicines, and frequently give them more as an alterative than to act on the bowels. The blood is undoubtedly in an altered state in yellow fever. The saline principles are said to be in defective quantity. I think it extremely probable that in this and other fevers where that condition of the blood exists, the neutral salts are of much more value than is generally supposed among us. It is certain that they are much more extensively

used abroad than among ourselves. The pathology of yellow fever, (perhaps of all fevers,) is I think involved in a great deal of obscurity. The gastro-enteritic theory is to me an absurdity. It is evidently mistaking an occasional effect for a general cause. I do not believe that either gastritis or enteritis generally exists in yellow fever, and if any case can exist without either, it is unphilosophic to regard them as the essential cause. Whether the change in the blood before referred to, is antecedent to the commencement of fever, I am unable to say. Dr. Stephens, I believe, contends that it is. If this be true, (and I think it might be very satisfactorily tested in your city,) it will throw a great deal of light on the pathology of fevers. I have been fully satisfied ever since I had the disease myself, that the first sensible lesion was in the brain and nervous system, and it is probable that the lesion of the ganglionic nerves is owing to that softening of the mucous membrane of the stomach which has been so generally, and as I think, erroneously regarded as the sign of inflammation. The black vomit I am satisfied, is not an effect of inflammation of the stomach, but is the dissolved blood which has fermented the softened tissues of the stomach and been blackened by the chemical action of the acid there encountered.

The cause of yellow fever then, whatever it be, must act in one of two ways—either when absorbed into the system it changes chemically the qualities of the blood and thus contaminates all the different organs, the most sensitive, to wit: the brain and nervous system, suffering first, or it acts directly on the brain and nerves in the first instance and from this primary lesion results the derangement which follows in the other organs. But I must bring this long letter to a close, which has been written in moments snatched from other engagements and often interrupted, so that I fear you will find it very desultory and unsatisfactory. If you find anything in it which shall aid you in the laudable task you have imposed upon yourself, or if at any time I can furnish you with facts or information that may assist you in your literary pursuits, it will be a source of much gratification to give you all the aid in my power.

Yours, very respectfully,

WILLIAM McCRAVEN.

DR. E. D. FENNER.

Part Second.

REVIEWS AND NOTICES OF NEW WORKS.

I—Report of the Select Committee of the House of Representatives, to whom was referred the subject of imported adulterated drugs, medicines, and chemical preparations.

It is notorious that for a number of years past a most iniquitous traffic has been carried on both by home and foreign dealers, in our most valuable drugs and medicines; adulteration to an extent hardly credible, were it not for the authority attesting it—the most unblushing frauds—the most scoundrelly impositions—the most murderous and reckless disregard of human life, have characterized the conduct of these gentry, who in the language of one of their agents, “manufacture to meet price.”

Thanks to Dr. Edwards, the chairman of the committee—thanks to the American Medical Association, this subject has engaged the attention of Congress at last, and the proper remedy, it is to be hoped, will be applied.

It seems from this report, that we are indebted to the stringent laws of Europe for so many of these good things on our side of the Atlantic:

“In consequence of the stringent laws now in force in most parts of Europe, regulating the trade in drugs and the dispensing of medicine, none but genuine articles, and those of acknowledged strength and purity, are allowed to be used or purchased. All inferior and deteriorated drugs in a crude state, as well as adulterated medicinal and chemical preparations, must therefore, as a matter of necessity, find a market elsewhere; and that market, unfortunately for the people of this country, has long been and still is found in these United States.

For a long series of years this base traffic has been constantly increasing, until it has become frightfully enormous. It would be presumed, from the immense quantities, and the great variety of inferior drugs that pass our customhouses, and particularly the customhouse at New York, in the course of a single year, that this country had become the grand mart and receptacle of all the refuse merchandise of that description, not only from the European warehouses, but from the whole Eastern world.”

Great praise is given in the report to Dr. M. J. Bailly, examiner of drugs &c., at the New York Customhouse. We copy what relates to this gentleman:

“Your committee embrace with pleasure this opportunity to present the name and services of Dr. M. J. Bailly, examiner of drugs, &c., at the New York cus-

tomhouse, as one to whom the country, and especially the Medical Profession, are deeply indebted for the firm and faithful stand he has taken in exposing these frauds. Enjoying the advantage of a thorough medical education, together with a ready and able pen, he has been industrious and successful, through the various journals of medicine and pharmacy, in calling the attention of both physicians and importers to these nefarious impositions. His communications with us have been frequent and important. Without awaiting a regular summons, Mr. Baily repaired at our suggestion to meet us, and we will subjoin the result of his examination before the committee. The activity and frankness of this gentleman deserves the highest commendation; we give an extract of a letter received from him, dated April 29th, in which he says, "I am sure such action (referring to a memorial of the national medical convention) will have great weight with the members of Congress generally, and cause them to act with more promptness than they otherwise would, whilst at the same time it will be exceedingly gratifying to me, as I have from the first, in aiding the college of pharmacy and my Profession in their appeal, used the knowledge acquired in my present position with a single desire to advance the general good. Motives of self or pecuniary interest (had I listened to the prayers of those interested) would have prompted me to withhold from the public the facts I have willingly disseminated, in order that permanent benefit might result therefrom. Many an argument have I held with those who professed to think it no moral wrong, while it was more profitable to themselves to impose such worthless and dangerous trash upon the community as we complain of."

"For many years a considerable proportion of the foreign chemical preparations, medicinal extracts, &c., have come to us more or less adulterated; but the base fraud is no longer confined to that class of medicines. Opium is now adulterated to a most fearful extent, and so adroitly as almost to defy detection by the unsuspecting and confiding purchaser. I have lately passed three invoices of opium from London, which, on opening the cases, seemed to all external appearance to be as it should, but a closer examination proved it a base compound of that drug with various vegetable extracts—the mass not affording more than about one-third part of pure opium. When I questioned the consignee, (and to the credit of our regular importers, most of these adulterated and deteriorated drugs and medicines are consignments from speculators abroad,) they admitted their private advices gave them to understand that the article 'was not quite pure;' yet, as the law now is, I must pass all such dangerous and rascally imitations, if they are found to be charged at their full value, and in commercial language, to be the article specified in the invoice. In fact, I have no authority to examine into the purity, &c., of any article further than to enable me to judge as to the correctness of the value expressed in the invoice." We subjoin also a quotation from the Boston Traveller of last week, entitled "frauds in opium." "About twenty cases of opium were sold at auction yesterday by John Tyler. It was imported from Smyrna and Liverpool in various vessels, and to all appearances was of equally good quality. Notwithstanding this, however, its price varied from \$3 45, \$1 35, six cents to three cents per pound. An exposition of the fraud was made at the sale. It is said to consist in the extraction of the morphine or vital principle of the drug, before exportation. This fraudulent opium was invoiced at a lower price than that of the first quality, but still greatly above its real value."

Of the articles most commonly adulterated, the following is a list: it will be seen that it contains most of the medicines in daily use.

Opium—Adulterated with various vegetable extracts and frequently deprived of two-thirds of its active principles in the manufacture of morphine.

Scammony—Adulterated with vegetable extract and clay.

Blue pill Mass—With Prussian Blue, blue clay &c.

Sulphate of Quinine—with chalk, Plaster of Paris, Starch, Salicine, &c.

Calomel—Starch, Salicine, &c., adulterated with white clay &c., and besides crudely prepared.

Iodine—Imperfectly manufactured.

Iodide of Potassium—Adulterated with Nitrate of Potassa.

Red Precipitate—With red lead.

Almost all the Extracts—adulterated, or imitation compounds passed off as such.

Myrrh—Imitation compound manufactured in Canton.

Castor—An imitation compound of *dried blood*, gum ammoniac, and a little real *castor*, put up in artificial bags, is the article generally met with. (*Extract from Report.*)

Gum Ammoniac—Adulterated with common rosin and earthy substances.

Gum Assafoetida—Adulterated with inferior gums, chalk, clay, &c.

Peruvian Bark—Mixed with inferior and worthless qualities, in some cases, active portions previously extracted.

Rhubarb—As in the cases of Opium and Peruvian bark the active ingredients have been extracted, and an article sold as rhubarb altogether worthless.

And so on with many other articles. That a stop should be put to this infamous trade, no one can deny; the wonder is that it has not been done long before. We learn from the Charleston Medical Journal, that a bill passed the House of Representatives on the second of June, providing “for the appointment of examiners at the ports of Boston, New York, Philadelphia, Baltimore, Charleston and New Orleans, who shall examine all drugs, &c., imported, and shall condemn such as are adulterated or spurious. The drugs so condemned, may not enter the country, but must be re-exported or destroyed.”

But whence are derived these adulterated drugs? We have an answer to this question from Dr. Bailey in his replies to the questions of the Committee.

“Whence do we derive the largest proportion of these adulterated and deteriorated medicines?”

Answer. The largest quantity comes from England; but other portions of Europe furnish more or less of these base compounds and worthless drugs.

Is this traffic on the increase, proportionate to the increase in the trade of drugs, medicines, &c.?

Answer. It is.

What proportion of the importers in New York are engaged in this traffic, to any extent, with a full knowledge of the article imported?

Answer. I know of but two or three of our regular and otherwise respectable houses, who order these vitiated articles from abroad. The business is more generally in the hands of commission houses, where ‘good, bad and indifferent’ can be found, ‘in quantities to suit the purchasers.’ A great proportion of these adulterated articles, I have reason to believe, are consignments.

From your knowledge of medicine and the information acquired in your present position, are not the deceptions, in many instances, so great as to deceive, not only the people generally, but the profession at large?

Answer. Such is unfortunately too true, and what is more to be regretted,

these base imitations are rapidly multiplying; giving, at the same time evidence on the part of the manufacturer of increased proficiency in the deceptive art, as applied to the preparation of vitiated medicines.

Are you acquainted with any agents of foreign manufacturing chemists who travel in this country, for the purpose of collecting orders and effecting sales of adulterated medicines, &c.?

Answer. I am acquainted with persons of that description, and they have been among us for the past twelve months."

With most of the sentiments expressed in the Report, we cordially concur; but there is one passage which not only does not meet our concurrence, but against which we most decidedly protest. Here is the passage.

"The Medical Profession in this country have established, and well sustained, a number of able medical journals; but, owing to the same crying evils, these useful and necessary aids in promulgating knowledge have greatly disappointed their friends, by exhibiting discrepancies almost too great for credulity. Examine the catalogue of adulterated medicines, and you have the key to the secret. Excessive doses of medicine are prescribed in some sections of the country, particularly the south and west; portions which, if pure, might well startle the eastern and northern practitioner, and used anywhere would endanger life. Quinine is used, in many cases, in incredible quantities; but this is accounted for most rationally, by the admixture of this valuable and necessary agent with salicine, chalk, &c. In acute diseases, dependent upon locality and climate, a difference in medical agents may be expected; but in chronic affections, having a set of regular phenomena in every clime, these discrepancies and contradictions would be perplexing, could they not be traced to the adulterated agents prepared expressly for 'southern and western trade.'"

Here is a slap in our faces, among others, with regard to large doses of sulphate of quinine; our country contributors must answer themselves for the purity of the article used by them—we shall only speak for the city of New Orleans.

The Sulphate of Quinine which we obtain from our best apothecaries with most of their finer medicines, is imported direct, and scarcely a case has occurred to our knowledge in which the purity of the article was suspected. But we have better proof of its purity. When the Mexican war broke out it was all important that the drugs furnished the medical officers should be pure, and it was particularly so with respect to the article of sulph. quinine, so highly regarded in the South. An elaborate examination of all the different specimens, (French, English, Spanish and Italian) that could be procured was made conjointly by Professor Carpenter of the University of Louisiana, and by Dr. McCormick, Medical Purveyor of the Army. In none was adulteration detected—the Italian only, we are informed by Dr. McCormick, contained a portion of Cinchonine.

Though we cannot hope to have escaped the deluge of trash which has swept over the United States, we believe our city is as little afflicted as any in the Union—our apothecaries (those best known we speak of) are in general gentlemen of liberal education, thoroughly versed in their art, and we believe utterly incapable of perpetrating a fraud upon the public either directly or by collusion.

But is it not funny to hear our Northern brethren asserting over and over again, that these adulterated drugs are intended for the "Southern and Western markets?" What, do none remain at home? Has the Examiner been through the thousand and one shops which nightly illuminate with their blue and red vases each of the cities of Boston, New York, Philadelphia, and Baltimore? Doubtless, from the fashionable apothecaries of these cities you can procure good drugs—as you can anywhere from the same class—it is their interest to sell good medicines, and their destruction to sell bad. But with regard to those which stud the outskirts and purlieux of all great cities! believe us, gentlemen, should the examination be made you will find a vast quantity of medicines "manufactured to meet price" much nearer home than you ever dreamed of. Providence is not so unjust as to stop all good things at the North, while the trash, and worse still, *all* the trash, is swept onward to the South and West. The laws of nature and of commerce forbid such a result. The North and the East must take their chance of good and evil with the rest of the human family.

Few men like to have their opinions distrusted, particularly if they have been so long entertained as to have become almost matters of faith—I have made up my faggot, says Dr. Johnson, and he who takes a stick out of it, looses the whole"—sulphate of quinine seems to be the stick which, if it has not been withdrawn from the faggot of Northern opinions, yet not long since kept up such a disturbance therein, as seriously to trouble and alarm the proprietors.

In fact, a portion, and not a very small one, of the Medical Profession at the North, have never been able to divest themselves of the shackles of Broussais. They will deny they are Broussaists in words, but betray themselves whenever they attempt argument either in conversation or in writing. This is manifest in their everlasting harpings upon "irritation" and "excitement," and in their eternal iteration of the words 'stimulant' and 'sedative.' To hear them, one would think that all medicines act upon the system by the rule of plus and minus, and that the human economy was a sort of pump-handle which stimulants push up and sedatives bring down. The alterative effects of medicines seem to be quite overlooked. That each medicine, enumerated in our books, has a *peculiar* action, that whether excitement or depression follow their administration, these effects are contingent and not constant; in short, that the virtues of every medicine lie in its chemical character, is disregarded in their reasonings, however much they may be conversant with the facts. Hence the sulky scepticism with regard to the action of large doses of sulph. quinine, when facts had so multiplied, that small logic was no longer of avail. Thus ran the argument:

"Sulph. of quinine is a stimulant, and in fevers when the system, and particularly the stomach, is in a high state of irritation—to give quinine, therefore, is to add fuel to the flame—the practice is murderous, etc., etc."

The administration of large doses of sulph. quinine in our southern epidemics and the effects therefrom resulting are matters of fact, not of opinion. These facts accumulated from so many different quarters that opposition would have been reduced to silence, if not to acquiescence,

had not some happy genius suddenly cried out "their quinine is adulterated, and hence the large quantities they can administer with impunity"—and forthwith the unbelievers stretched themselves upon this pleasant bed of hypothesis.

Before taking leave of them we must point out that Coutanceau (and we might mention others) used bark in the commencement of idiopathic fevers as long ago as 1805 and with success. Maillot in 1835, during the Bona epidemic, also employed quinine from the beginning, and in doses from 24 to 40 grs., and also with success. Were these medicines also adulterated?

But there is an absurdity involved in their supposition which it is surprising they themselves have never discovered. So far as the records go, the administration of sulph. quinine in large doses and early in the stages of our southern fevers, is attended with results highly satisfactorily. These results, then, if their hypothesis be true, must have been produced by adulterated medicines.

The report says in the extract last quoted, that "excessive doses of medicine are prescribed in some sections of our country, particularly the south and west; portions which, if pure, might well startle the eastern and northern practitioner, and *used anywhere would endanger life*. Quinine is used, in many cases, in incredible quantities; but this is *accounted for most rationally*, by the admixture of this valuable and necessary agent with salicine, chalk, etc." The italics are ours. Now with all due deference, we cannot agree in the rationality of any such inference. It smacks of a foregone conclusion—it is reasoning a priori—and besides, the conclusion is built upon a gratuitous hypothesis. A Baconian would have commenced his inquiry by ascertaining the fact whether the quinine was, or was not, adulterated; the mere supposition is sufficient for our philosophic reporter.

We humbly think that the proper way to test a matter of fact is to repeat the experiment; or if timidity should interpose her hand, to get others less nervous to do it. This can easily be done by sending south some of the real, dangerous, true, simon pure sulphate of quinine—should any be sent, we promise most solemnly to render a conscientious and faithful account of its effects.

J. H.

II.—*On the Blood and Urine in Health and Disease*. By JOHN WILLIAM GRIFFITH, M. D., F. L. S. &c.; G. OWEN REES, M. D., F. R. S. F. G. S. &c., and ALFRED MARKWICK, M. D., &c. In one volume, Lea & Blanchard, Philadelphia, 1848.

The three gentlemen above named, wrote and published separate works on the blood and urine in health and disease. They were brought out first in London, and the American publishers, for convenience and easy reference, with much good judgment, embraced the three papers in one handsome volume.

Until chemical analysis, aided by the microscope, began to be applied to the investigation of both healthy and morbid phenomena—of physiological and pathological science, their progress had become tardy and

uncertain, but with the helps with which genius has supplied us, a new impulse has been imparted to these sciences, and we even now imagine that the day is near at hand when medicine shall take rank among the exact sciences.

It is true that in the early days of our profession some attention was bestowed upon the blood and some of the secretions and excretions of the body; but it did not extend beyond their mere *physical* appearance, such as their consistency, color, odor, &c.

Now, however, by rigid analysis, both quantitative and qualitative, we can specify the exact composition both in health and disease of all the fluids of the human system.

Knowing this, we are daily striving to apply such remedies as are most likely, from their well ascertained habitudes, to correct the aberrations of chemical compositions. Having premised thus much, we shall endeavor, as briefly as possible, to give the reader some idea of the character of the three papers, included in one volume, under the above title. We shall speak of this *trio* as one book, and regard the authors but as one, treating of subjects closely connected with each other.

The first part, then, is devoted to the study of the urine; it points out, in precise language, the steps to be taken, in analysing this fluid. This is, we doubt not, already familiar to the reader, and we therefore proceed to notice that portion of the work which investigates the "*general chemical and microscopical characters of the blood &c.*" "Before," says the author, treating of the characters of the compound fluids; those proximate principles, &c., which are generally diffused through them will be described; those which are found in peculiar fluids only will be treated of with the fluids themselves."

The "proteine compounds" are made up of combinations of an organic principle with sulphur and phosphorus in various proportions; thus combined, the organic principle is called "proteine." Like other organic principles, they may be either in a fluid or a solid state; but it does not exist in the system, either as a fluid or solid, in an uncombined state. As this proximate principle plays such an important part in all works on organic chemistry, we refer the reader to the author's account of its chemical properties.—[page 60.]

The book contains an account of the other proteine compounds, and also their behavior with the various acids, metallic oxides and alkaline salts. For full details on this subject we refer to the work itself.

As it is of the first importance that the student should be familiar with the properties and constituents of the blood in health, before he can be prepared to appreciate the changes wrought in it by disease, the author gives an analysis of healthy blood, and having done this, in a lucid and plain manner, and according to the latest methods, he then proceeds to an analysis of the same fluid in disease. As already remarked, it was in the early stages of medicine known that the blood was altered by disease; but in what this alteration consisted, little or nothing was really known.

Thanks to the progress of modern chemistry, we can now speak on

this subject as of matters of fact, capable of demonstration and analytical proof.

The blood is said to be diseased, when any of its constituents or proximate elements are either deficient or in excess. This excess or deficiency may be produced by various causes; and among the most fruitful, is the retention in the blood of certain constituents which are destined in a state of health to be eliminated by the excreting organs of the body.

Thus in some forms of diseased kidney, the urea, failing to pass off through this excreting organ, is retained in the blood, and if very abundant, may embarrass the functions of several organs.

So in Asiatic Cholera, we have a deficiency of the salts and the serum of blood, caused by the abundant thin rice water discharges, as they are popularly called.

The elements of the blood may be relatively or absolutely deficient or in excess, in respect to certain other constituents of the same fluid. In either case, it is in an abnormal condition, but whether this abnormality (!) entails serious consequences upon the organism or not, depends greatly upon its extent and the nature of the proximate principles thus modified.

But let us return to experimental knowledge. M. Lecanu, in some researches on the *variable proportion of the constituents of the blood*, in the different sexes, found the amount of blood to vary in the two sexes; thus in 1000 parts of blood, the maximum quantity of water was 853,135; and the minimum 773,625, mean 821,7645, making a difference of 62,741. Age also in the same sex, exercised a modifying influence in the relative constituents of the blood. The blood of the female contains a larger proportion of water than that of the male; being an excess in favor of the former of 29,8205. The lymphatic temperament, it is well known has more water in the blood than the sanguine; the mean of the water in sanguine temperament being 793,007; whilst that of the lymphatic reaches as high as 803,710; leaving a difference of 10,703 in favor of the latter temperament. This fact may, perhaps, enable us to explain why it is that females will bear saline purgatives much better than the male; for such we believe to be the fact, whatever the explanation.

The quantity of albumen in the blood, according to M. Lecanu, does not vary as much as the water, even under similar circumstances; yet the excess is in favor of the sanguine temperament in both sexes.

The same author has demonstrated by quantitative analysis that the globules of the blood form no exception to the predominance of one element of the blood over that of another, not only in the two sexes but likewise in the different temperaments of the same sex.

Thus the proportion of globules is greater in men's blood than in that of woman, and of course, in the sanguine than lymphatic temperament. It must be remembered that the periodical evacuations to which females, at a certain age, and in good health, are doomed, will vary the proportion of globules. Hence in some experiments performed by M. Lecanu, on the blood of the female, both before and after the menstrual discharge, he found the proportion of globules diminished nearly one-half in those who had just passed through their monthly evacuations.

With regard to the relative constituents of venous and arterial blood, and the blood of the hepatic, renal veins, capillaries, and venæ, portæ, &c., so great is the difference of opinion that among those who have devoted their time to this subject, nothing positive can be asserted. Whilst on the subject of the comparative elements of the blood of different vessels, we may "*en passant*" state that Denis found the blood of the fœtus to contain more solid matter and corpuscles than that of the mother. The proportion of iron in the blood taken from the umbilical artery, compared to the venous blood of the mother was as 2.5 to 1. This result could not from *a priori* reasoning have been expected; yet the author is too respectable to mislead. Besides some excellent observations on the composition of the blood both in health and disease, this volume contains many valuable remarks upon morbid and healthy urine; it also points out the appropriate treatment for each particular disease, as indicated by the chemical state of this fluid.

It is in many respects and for a variety of reasons, one of the best manuals recently published, on the different subjects of which it treats. The *American* publishers are entitled to the thanks of the profession for their enterprise. A. H.

III—*A practical Treatise on Poisons: their Symptoms, Antidotes and mode of treatment.* By O. H. COSTILL, M. D. CRIGG, ELLIOTT & Co., Philadelphia, 1848—p. p. 160.

Toxicology has become a separate branch of medicine, and not, by any means, the least important. Until the works of Orfila, Christison, Taylor and Anglada appeared, this branch of medicine had not assumed the shape of a science; it existed only in isolated facts, scattered through the periodicals of the day and other ephemeral productions. To collect and arrange—to collate and systematize these facts and observations, and to add others more precise and positive, founded upon experiments, was the work of the highly gifted Orfila; and ably did he perform the task which he assumed.

Since the first edition of M. Orfila's work, many valuable additions have been made to toxicological science, and with our present amount of knowledge of this subject, we are better prepared to appreciate its importance both to the profession and the public. Imperfect as it undoubtedly is, yet no man is competent to pursue safely his profession, without some acquaintance with this branch of medicine.

Dr. Costill lays no other claim to originality but in the plan of this little work. He says he has attempted to collect and arrange the symptoms—post mortem appearance and the treatment of poisoning from the best writers on the subject. This is all very well; but such knowledge will scarcely satisfy an ambitious mind; it seeks to go further, to understand the chemical as well as therapeutic habitudes of medicinal agents, whether of vegetable, animal or mineral origin. Hence, although toxicology may be regarded as a separate branch of science, yet if studied alone—as an independent department, little good would result from it in a practical sense.

A familiar acquaintance with chemistry together with sound views on the physiological action of medicines, must form the basis for the study of toxicology. In possession of this knowledge, the practitioner will readily apply it in its appropriate place and time, with far more certainty and effect, than he who, ignorant of such things, relies upon his information acquired from works, written exclusively upon toxicological medicine. These facts should urge us to study chemistry and all that is comprehended in the adective *physiological*, with a view to their easy practical application.

The little work, by Dr. Costill, may be useful for reference in emergencies, when time presses, when large and more elaborate works on the subject are not at hand.

A. H.

III.—*Notes on the Theory of Human Existence, comprising remarks on Vitality, the Mind and, incidentally, the Soul; the whole being an Exposition of the Nature, Powers and Destiny of Man.* By THOMAS L. WRIGHT, M D., Cincinnati, Ohio. 1848.

We are indebted to the author for this small pamphlet, numbering thirty-seven pages. A large portion of it must necessarily be speculative, treating as it does of the nature of mind, the soul and of vitality. Whoever undertakes to explain the connection between mind and matter—the soul and the intellect—life and death—must meet at every step of his progress difficulties which nothing but a disembodied spirit can ever surmount. Since the dawn of man's existence—when the morning stars sang together for joy, over the birth of our race, the human mind has striven in vain to explain its own existence—its powers—its capacities, and its connection with matter. And yet but little progress has been made in mental or sycological science.

Now, as we know nothing of matter but by its properties, so, in like manner, are we equally ignorant of the nature of mind and vitality, although we see their wondrous *effects* at every step of our lives. After floundering through the mud and mire of transcendental nonsense, Dr. Wright, in his 2d Chapter, when speaking of life—the process of organization—the *vis vitæ*, etc., etc., holds forth in the following philosophic strain:—

“Vitality, therefore, in its special molecular actions, operates in the fulfilment of two offices:—1st. An immediate and primary office, having for its object the production of organized matter. Vital actions are nothing more, than the selection from *inorganic* matter of crude material, and their conversion into organized compounds.” If we understand Dr. Wright, he means to say, that organized beings are nourished by inorganic matter—a statement entirely new to us, and at variance with all medical philosophy. Even the creeping earth-worm, which crams his straight alimentary canal with dust and mud, derives his nutriment from the *organized* vegetable and animal matter that it contains!

Organized beings can only be supported by *organic* matter; and

and the meanest and most lowly organized of creeping things would starve amidst a mountain of gold-dust did it not contain organized vegetable or animal matter! So much for Dr. Wright's views on the subject of organization and nutrition.

Much of his reasoning is equally fallacious and unfounded; yet we must admit that *some* of his views are quite plausible and ingenious. We regret he did not separate the latter from the former, as it is much easier to condemn than to applaud. In his chapter on the "*Duration of Mind—its Immateriality and Infinite Capacity*," he says, "the evident tastes and longings of the mind are also collateral evidence of its character. It loves to dwell on conceptions which stimulate the infinitude of immortal topics."

We think the author must needs write another pamphlet to explain "*in extenso*," this last paragraph; for, to our mind, it is the climax of nonsense. Again, he says, speaking of the mind,—“It loves to dwell on *vagueness*, conceptions of things without *knowing* their bounds, or rather without the ability to know them—in *indefiniteness*, things which may be seen, heard or felt, in fact or in imagination, but whose beginning or termination, it cannot comprehend. Ideas dimly seen, yet not seized by the mind—and *inapplicable*—are viewed by it with satisfaction, as *possibly* containing something of unearthly meaning and power.” (page 31.) Did we not tell the reader that the above is the exact language, punctuation, italics, &c., of the author, he might well be in doubt. The vague conceptions of our author's mind, in framing the above paragraphs, is a happy illustration of “ideas dimly seen,” and an effort to understand things “without *knowing* their bounds, or rather without the ability to know them.”

Dr. Wright contends, for instance, that all minds are, *de facto*, equal in original power and capacity; and that the apparent difference is due entirely to the superior organization of its instrument—the brain! This may be correct, yet we doubt it. Neither the positive nor negative can be disproved.

After laboring to prove that the mind is immaterial, and therefore immortal, he says, at page 32,—“that there are reasons for supposing, however, that mind, in original capacity, is a unit—of equal power everywhere,”—and, *per contra*, “matter is a unit in its operations, without resource or intelligence;” consequently mind is nothing more nor less than matter in operation. Q—E—D.

Such are a few of the contradictions and, we were about to say absurdities, with which our author is chargeable.

Now, whether the mind—the soul—the intellect, or by whatever term the memory or the reasoning faculties of man may be designated, is material or immaterial, the conclusion as to its duration is the same. *Matter per se* is as indestructible as *mind*; for where there is no matter there is no need of mind—the existence of the one pre-supposes the presence of the other.

Did we not believe firmly in the immortality of the soul—the mind—this pamphlet might well shake the foundation of our faith, although the author evidently aimed to establish this important fact.

V.—*On Bandaging and other Operations of Minor Surgery.*—By F. W. SARGENT, M. D. Philadelphia: Lea & Blanchard. 1848. pp. 379.

It has, we regret to say, fallen to our lot to notice in condemnatory language, several American and foreign works claiming to give a brief synoptical view of some one or all the various branches of medicine. We are pleased, however, to be enabled to adopt a far different tone in relation to the elegant little volume on *Minor Surgery*, by Dr. Sargent. The manner in which the work has been brought out, no less than the matter it contains, reflects much credit both upon the taste of the publisher and the judgment and practical good sense of the author. It contains a number of very well executed engravings, illustrating many interesting points in surgical science. The author lays no claim to originality; but says he has availed himself of the writings and opinions of men who are acknowledged to be good authority on these subjects. Many of our best works on surgery do not enter into such details as the young surgeon, at his outset, requires to enable him always to acquit himself with satisfaction. True, they tell us all about dislocations, fractures, aneurism, &c., &c., yet, they being too much occupied with principles and important lesions, rarely seen in routine practice, seldom condescend to give minute and accurate directions for one not already familiar with clinical surgery.

As supplying this necessary and truly useful information, we recommend Dr. Sargent's book on *Minor Surgery*. It embraces everything in relation to bandagings, surgical dressings, for fractures of all the bones, and indeed a large amount of practical information.

It is written in a plain and intelligible style; omitting nothing that is useful to the young practitioner; nor yet does it enter into useless and tedious minutiae.

The Book is worth buying.

A. H.

Part Third.

EXCERPTA.

1.—*On the Use of the Peruvian Bark, given in very large doses, in the Malignant Yellow Fever.*

We give below a curious and interesting paper on the use of large doses of the Peruvian Bark in the treatment of Yellow Fever. Doubtless had the sulphate of quinine been known at that date, (about the year 1800,) it would have been used for the same purpose by the sagacious Dr. Lafuente. It certainly anticipates the present quinine practice, as it is called. The paper is taken from the *Philadelphia Medical and Physical Journal* for 1808.

(ED'RS.)

The Consulting Physician of the Royal Armies, Dr. Tadeo Lafuente, frequently commissioned as inspector of public health, in the camp of Gibraltar, has composed a paper, entitled, "*Well-proved and decisive Observations, showing that the Yellow Fever loses all its contagious force in a Cottage,*" as also that it may be kept off, or cured, by bark, used in a manner entirely new and distinct from any commonly practised.

This dissertation, which was, by order of his Majesty, examined by the superior governing Medical College, was found to merit his royal approbation, and was, consequently, by his directions, printed and published; but as the season, in which this new method might be eminently useful, is at hand, and the operations of the press would not allow of the publication as early as could be wished, the following extract is given, drawn up by the author himself.

The above-named physician is well aware of the general discredit into which bark has fallen, in this Peninsula, (Spain) as a remedy for the Yellow Fever, and is himself inclined to admit many of the objections made to it; but he decidedly and firmly declares, that none of those who have written or spoken against its use, have either given it or seen it given, in a proper manner; and that, if administered as it ought to be, it will as certainly cure the Yellow Fever as the Tertian.

The method he has invented is most simple, and consists in obliging the patient, within the first forty-eight or fifty hours of his fever, to take, at least, from six to eight ounces of bark, and it is absolutely necessary that he should retain this quantity in his stomach.

In order to effect this in so short a period, which is necessary, minute attention to circumstances is requisite, and not a single moment to be neglected. For this reason, the taking of the bark must commence instantly that the chill or any other symptom is felt of this subtle and terrible disorder; and if any time is lost, it ought not to exceed six or eight hours, from the first attack, because in the three or four first days of this sickness, all the destructive qualities of it show their effects; and the only confidence in a cure must arise from boldly attacking these, before they can unite, and without losing one instant of time.

It appears from his observations and proofs, that the efficacy of the cure may be totally and wantonly destroyed, if some of the doses are omitted, or the first hours neglected in which it should be administered. Of ninety-seven persons who were attacked with this fever, in the village of Barreos, and who took these six or eight ounces of bark, within forty-eight hours of the attack, commencing from the first to the eighth hour, all quelled the disorder in its origin, one excepted, who perished, having at the same time an attack of the gout. Of eight who took the same quantity, commencing from the eighth to the tenth hour, all were cured. Of five, who began to take it from the tenth to the twenty-fourth hour, three recovered and two died. Of twenty, who began the second day, thirteen recovered and seven died. Of seventeen, who began the third and fourth days, eight recovered and nine died; and, finally, of eighty-nine, who were treated as they chose, but in a different manner, (including the sudorific plan,) only twenty-two recovered. It is further to be observed, that when no time was lost in beginning the bark, within the first ten hours, and taking the six or eight ounces, the disorder was so completely suffocated and eradicated, that the cure was effected by the fourth day, and they were able to leave the house, as if they had not been sick, and before the second and most terrible period of this disorder, which usually commences the third day. But when there was delay as to time in the commencement, or neglect as to the quantity, they felt proportionably more or less of the symptoms of the second period, and risked more or less from their violence, although they were finally cured.

The above-named physician, as well as the one who practised at Barreos, Dr. Joaquin de Bobadilla, to whose zeal and exactness we are indebted for a great part of the important observations in this dissertation, were both careful to clear the stomach and intestines by a slight vomit, before they commenced the use of the specific, or added v. g. two drams of cream of tartar, or cathartic salt, to the first dose of bark, to the second, and even to the third, if necessary, or even causing them to vomit, by means of warm water, without medicine; but females and others, who undertook their own cure clandestinely, as soon as they knew the singular efficacy of this remedy, learnt (as they say themselves) the importance of not losing an instant of time, by these, or other usually preparatory steps; they found that cures were equally performed without as with them, that it was necessary to be careful with the vomits, because much time was lost by them, and they left the stomach so irritable, that they could not so well retain the bark. In short, (says Lafuente,) when the more sensible part of this village was satisfied of its efficacy, an emulation was excited amongst those who were attacked, who could take the most bark within the forty-eight hours, and at the earliest period.

The greater part of the sick, therefore, had neither vomit, purgative, nor other receipt than a packet of half a pound or more in bark powder, (and even this not divided into doses,) and the moment they felt the first chill, they began rapidly to take the bark, without sleep or remission, night and day, every two hours, a large spoonful at a time, sometimes equal to an ounce, sometimes half an ounce, and at least three drams. Some of them, either from having suffered in their reason, or from some excess, or inattention, and sometimes unnecessarily but for greater certainty, took from sixteen to twenty, or even thirty-eight ounces within a few days, without any ill effect.

Nevertheless, those who wish to use it by measure, may divide each ounce into three papers, and take one every three hours, taking a little broth each of the two intermediate hours; by which means they will take in the first forty-eight hours, eight ounces of bark in twenty-four papers. If the stomach rejects a dose, another must be taken immediately, without reckoning on what is rejected as a part of the eight ounces, and the patient must not be indulged in his natural repugnance to the repetition more than half a quarter, or, at most, a quarter of an hour, to quiet his stomach; and if, from any inattention or accident, a dose has been omitted, two doses must be taken each succeeding hour, and the

broth taken the third, following up and compelling, as it were, the patient, until the lost time is made up, and the risk got over that may have arisen from carelessness or delay.

The vomiting, which is peculiar to this disorder, seldom begins before the third day, and this is another motive not to lose a moment during the two first days; it is true, that there are some who throw up the bark from the nausea it naturally creates, and this will, in those cases, lessen much its good effects. Yet in the village of Barreos, only one person suffered from this circumstance, from not knowing at the time how to remedy it, as it was remedied in every other case.

This advantage was obtained in two ways. *First*, it was mixed with water, so as to form a paste which could be handled and made into pills, lengthened so as to divide the dose into four or five. These were folded up in wafers, a little moistened with water, and they soon learnt to swallow these without perceiving the taste of the bark, and without any trouble. At first the patients may conceive that they cannot swallow such large pills, and may wish to have them smaller; but they are really mistaken, as in the way they are made and softened, they easily assume the shape the throat gives them in its passage, and have neither points nor hardness to injure. A little firmness and management soon undeceives them, and they find it less troublesome than to take smaller pills, and more frequently for each dose. It may be supposed that they will not immediately decompose themselves in the stomach, but this can only happen when they are too long made, and too hard and dry.

The *second* method of stopping the vomiting consisted in taking before and after each dose, or for some time every quarter of an hour, one or two spoonfuls of the following mixture:

Take of Jarabic de Meconed,	-	-	-	-	-	one ounce,
Spirit of Canela,	-	-	-	-	-	one dram,
Of wine, or water, as the patient pleases,	-	-	-	-	-	six ounces:

mix it for use.

Dr. Lafuente has not confined himself, in his Memoir, to proving incontestibly the truth of his assertions, by twelve signatures, accompanied by legal attestations; but he has detailed his professional reasons, referring also to his first publication, and anticipating answers to all the objections which can be made against his method. He notices, for instance, the difficulty or facility of knowing at once, according to the cases, whether it is the fever, so that the most delicate persons, once convinced, and those in lower stations will follow, they will swallow and retain as much bark as is necessary.

Again, he shows that the fever is not inflammatory in its first stage, as some suppose, nor originally gastrical, nor hepatic, as others assert; treats of the degree of risk of its being joined by other inflammatory disorders; shows that if, in periods of greater heat than when he made his observations, it is more active, the effect of the bark is also more powerful than in more temperate months, and that all that is necessary is to be more watchful not to lose a moment of time, &c.

He also remarks that such abundant and precipitate doses of bark sometimes produce a retention of urine, but this evil is momentary, and ought not to give any alarm, as it is instantly dissipated by embrocation with certain simples, and oily frictions of the groins, and has no connection with the suppression of urine in the second stage of the disorder. But the little room allowed for this analysis does not admit of a more ample detail, and, indeed, on the contrary, some inconvenience to the public good might arise from giving merely an abstract of these important observations.

For, (observes Dr. Lafuente,) it being absolutely necessary to convince the minds of the medical and other attendants, in order to execute a plan, which, although simple, requires a certain species of boldness to conquer obstacles

and prejudices, it will be indispensably necessary to read the whole work, (which will be speedily published,) in order to be fully convinced of the correctness, truth and irresistible conviction it carries with it. In the mean time, he informs the public that he has preserved himself from the yellow fever, during the execution of his public medical commission, in the years 1800-1-3, in which he was more or less exposed to its contagious effects, by merely taking half an ounce of bark every morning in one or two doses, and that having begun their rounds more steadily, and been more exposed to danger in the beginning of October, 1804, he guaranteed himself in the same manner during that month and November.

This was likewise practised by Dr. J. de Bobadilla, and the Rev. Father Juan, of the order of San Miguel Mendicant and Barefooted, and Chaplain to the sick in the village of Barreos, who were constantly living within its vortex, until tired of taking bark as a preservative, and convinced that they should run but little risk, in case they were attacked by the fever, they agreed to discontinue it, and follow their rounds, determined, in case of attack from the sickness, to destroy it by bark at its first appearance, and see if in this manner they could escape, or even overcome the attack, in those circumstances which were less favorable; and in effect Lafuente, who quitted the bark the 20th of November, was on the 4th of December seized with a suspicious fever, which was dissipated instantly by taking six ounces of bark in forty-eight hours from the chill. The chaplain and physician who had attended the sick, and omitted the preservative, on the 15th and 16th of December, were attacked in the beginning of January, and both immediately stopped it; the first by a pound of bark in forty-eight hours, because his fever was very violent, and he was determined to be secure; the other having been more slightly attacked, was cured with six ounces in the forty-eight hours, and one ounce the following day.

Lastly, it is fully proved by the same documents, that a person having the yellow fever, who is with his attendants in a cottage or barrack in the country, however small it may be, does not communicate the infection to any one. At the time that the sickness prevailed in Barreos, the families that remained in the houses lost in some of them two or three persons; the contagion also prevailed in a fine hospital that was obliged to be opened in a corner of the town, on account of the prejudices of some of the sick, and from which they were with their assistants ultimately obliged to fly, finding a safer asylum in the country cottages; at the very same time there was not a single instance of one assistant falling sick amongst a hundred which attended the sick in the Cottage-Lazarettoes, which were established at cannon-shot distance from the village, where each cottage or tent had only one sick, with their friends who chose to accompany them, and each cottage (or tent) was isolated and separated eighteen yards each way from every other one.

The rains and hurricanes, which prevailed the beginning of December, obliged the officers of health to offer to the sick in these cottages or tents that chose to return to the hospital, but they unanimously determined, unless obliged by violence to do otherwise, to remain where they had experienced the good effect of their situation, both for themselves and their assistants, not one of whom had fallen sick. Such is the enormous difference between the two degrees of ventilation.

The imperfect construction, the tumultuous and ever continued interruptions, of the best of these great buildings, has not a single point of resemblance with the natural and tranquil circulation, which, without ceasing night and day, pierces the innumerable cracks and pores of a tent or cottage. In buildings we continually breathe the very slightest offensive vapors, for at least eight or ten hours every night, when we are obliged to close our doors and windows; but in a cottage, always sufficient porous and open, the very laws of the circulation of air make it impossible to retain one moment any of the miasma

which escape from the contagious persons or effects, even although the door should be shut, and there should be no window. A chamber, in which a single healthy person sleeps one night, smells offensively to any person who from without opens the door suddenly, until it is ventilated; and the smoke and smell of a single cigar is easily preserved, and for a long time, in an apartment plastered with lime and mortar; but in a cottage, the most fætid excremental smell scarcely remains an instant, and even the smoke of a large portion of burning wood is scarcely retained. The smell of sulphur which they had just burnt at the moment Dr Lafuente was going to occupy the cottage in the Lazaretto when he was ill, did not last longer than the time taken to burn it.

What a consolation for a family to know, that by going to a cottage or tent with a person sick of the yellow fever, not only the sick person will be in a better situation, which is instantly felt by those who are convalescents, as well as those who are half infected, and on the point of having the fever, as they quickly dissipate all the miasma attached to themselves or clothing; but more especially they are certain, that all who may be inmates there will never be infected, although they may sleep, as it were, in the same bed with the sick. But the reading of the work will satisfactorily show, even to the very foundation, all the incomparable advantages which are promised by this discovery.

Note in the Mexican Gazette, from whence this account was extracted.

The Editor and a physician residing in this capital, desirous of propagating the knowledge of this work in the whole kingdom, and particularly that Vera Cruz may be benefitted by it, they have subscribed that it shall be reprinted in this our metropolis.

2.—*Miscellaneous Medical Facts and Observations. In a letter to the EDITOR, from Dr. SAMUEL BROWN, of New Orleans.*

DEAR SIR:—For three years past, this city has escaped the ravages of the yellow fever; and I can assure you, that the two last years have been as healthy as I have ever known any country to be, even in a Northern climate. Consumptions and colds are rare: we seldom have intermittent fevers. Indeed the remittent fever is almost the only disease which can be considered endemic. Hypochondriac and hysterical complaints are astonishingly rare. The almost universal use of claret, and the influence of a fine sky, produce habitual good spirits. Dyspepsia is so uncommon, that in twelve months I have not been consulted on a single case of it. I am inclined to think, that a voyage to this city would be one of the most salutary prescriptions for your hypochondriac patients, which, in most parts of the United States, are a pest to physicians and to their families.

Mercury, as a cure of fever is going fast into discredit here. I never used it to the extent of salivation in fever, but have seen much mischief result from it, whilst I thought its use very equivocal in every instance where I have known it employed. I hope a few years will consign this remedy to a humbler rank than that of a panacea.

I am daily more and more pleased with the effects of the tourniquet, when applied to the stomach for the cure of convulsions. I wish most sincerely that some of you would try its effect in your hospitals, where you have so many opportunities for experimenting upon inveterate disorders. In true epilepsy, I have not yet had occasion for using it, but from its effects

in hysteric convulsions, I should expect much benefit from it. I am even inclined to think, that in *habitual* intermittent fevers, strong compression upon the stomach may prevent paroxysms which would resist the bark.

The natives of this country, and the negroes, cure the gonorrhœa by a decoction of the Palmetha, (Litanie,) and it is believed by them, that a decoction of the leaves and roots of the Nymphæa Nelumbo is capable of producing *temporary impotence*. The experiment is worth the trial.

I am yours, &c.,

SAMUEL BROWN.

New Orleans, November 7th, 1807.

Part Fourth.

MEDICAL INTELLIGENCE.

FOREIGN.

I.—*Excerpta from "A Treatise on Fractures in the Vicinity of Joints,"* &c. By Dr. R. W. SMITH, Dublin.

1. *Fractures of the neck of the femur.*—*Conclusions on the diagnosis and pathology.*—1. A slight degree of shortening, removable by a moderate extension of the limb, indicates a fracture *within* the capsule.

2. The amount of *immediate* shortening, when the fracture is within the capsule, varies from a quarter of an inch to one inch.

3. The degree of shortening when the fracture is within the capsule, varies chiefly according to the extent of laceration of the cervical ligament.

4. It also varies according as the fracture is impacted or otherwise.

5. In some cases of intracapsular fractures, the injury is not immediately followed by shortening of the limb.

6. This is generally to be ascribed to the integrity of the cervical ligament.

7. In such cases, shortening may occur suddenly, at a period more or less remote from the receipt of the injury.

8. This sudden shortening of the limb is, in general, to be ascribed to the accidental laceration of the cervical ligament, previously entire, and is indicative of a fracture within the capsule.

9. The deposition of callus around fragments is not necessary for the union of the intracapsular fracture.

10. When osseous consolidation occurs in the intracapsular fracture, it is effected by the direct union of the broken surfaces, which are confronted to each other.

11. The osseous union of the intracapsular fracture is most likely to occur when the fracture is of the variety termed "impacted."

12. In the intracapsular fracture the mode of impaction is different from that which obtains in the extracapsular.

13. The degree of shortening, when the fracture is external to the capsule, and does not remain impacted, varies from one inch to two inches and a half.

14. When a great degree of shortening occurs immediately after the

receipt of the injury, we usually find a comminuted fracture external to the capsule.

15. The extracapsular fracture is accompanied by fracture with displacement of one or both trochanters.

16. The extracapsular *impacted* fracture is accompanied by fracture without displacement of one or both trochanters.

17. In such cases the fracture of the trochanters unites more readily than that of the neck of the bone.

18. The degree of shortening in the extracapsular impacted fracture, varies from a quarter of an inch to an inch and a half.

19. The exuberant growths of bone met with in these cases have been erroneously considered to be merely for the purpose of supporting the acetabulum and the neck of the femur.

20. The final cause of their formation is the union of the fracture through the posterior intertrochanteric space.

21. The difficulty of producing crepitus, and of restoring the limb to its normal length, are the chief diagnostic signs of the impacted fracture.

22. The position of the foot is influenced principally by the obliquity of the fracture, and the relative position of the fragments.

23. Inversion of the foot may occur in any of the varieties of fracture of the neck of the femur.

24. When the foot is inverted we usually find that either a portion or the entire extremity of the lower is placed in front of the superior fragment.

25. In cases of comminuted extracapsular fractures, with fracture and displacement of the trochanters, the foot will generally remain in whatever position it has been accidentally placed; it may be turned either inwards or outwards, or there may be inversion at one time, and eversion at another.

26. Severe contusion of the hip-joint, causing paralysis of the muscles, which surround the articulation, is liable to be confounded with fracture of the neck of the femur.

27. Severe contusion of the hip-joint may be followed at a remote period, by shortening of the limb, and eversion of the foot.

28. The presence of chronic rheumatic arthritis may not only lead us to suppose that a fracture exists when the bone is entire, but also, when there is no doubt as to the existence of fracture, may render the diagnosis difficult, as to the seat of the injury with respect to the capsule.

29. Severe contusion of the hip-joint, previously the seat of chronic rheumatic arthritis, and the impacted fracture of the neck of the femur, are the two cases most likely to be confounded with each other.

30. Each particular symptom of fracture of the neck of the femur, separately considered, must be looked upon as equivocal. The union of all can alone lead to the formation of a correct opinion as to the nature and seat of the injury. (p. 110.) *Ranking's Abstract*, June, 1843,

AMERICAN MEDICAL INTELLIGENCE.

1.—*Remarks connected with the sanitary condition of the city of New Orleans.* By WILLIAM P. HORT, M. D., of New Orleans.

In an article on the subject of Quarantine Laws, published in the New Orleans Medical Journal, July, 1845, I gave my reasons at some length for believing that the yellow fever originated in this city, it being still included within the northern limit of the yellow fever region in the Western Hemisphere; that the causes of its production existing here, it was useless to look to other places for its paternity, and worse than useless to attempt to exclude it by quarantine laws, which had been tried here and elsewhere over and over again, and proved to be of no avail.

Since that time, all the facts that have been observed in relation to this subject have only tended to strengthen the opinion then expressed.

In 1846, there were sporadic cases of yellow fever, though many persons contended that the disease was epidemic, and efforts as usual were made to trace it to some foreign port in the Gulf where yellow fever was known to prevail; they failed, however, to establish the doctrine of importation, while proof of the domestic origin of the fever was abundant. Dr. Fenner investigated this question diligently and impartially, and thus concludes a very interesting and able article on the yellow fever of 1846. "From all the facts here stated, I think the following conclusions may be fairly deduced, viz.:

"1.—That the fever I have attempted to describe is genuine yellow fever.

"2.—That it originated spontaneously in the city."—(N. O. Med. & Surg. Journal, Jan., 1847.)

By referring to the Report of the Board of Health for the year 1846, published in the same number of that Journal, and which was unanimously adopted by the Board, we shall find that they fully concurred in opinion with Dr. Fenner.

"When these circumstances (the filthy state of the city generally) are duly considered, and that moreover disbanded volunteers already debilitated by disease on the Rio Grande were returning every week during the summer and fall; that most of them were unacclimated men from the Western States, and compelled to remain some time in the city, it is only surprising that New Orleans suffered comparatively so little this last season with malignant and yellow fevers. It has been estimated that more than twenty thousand Europeans have settled in this city in the course of the four or five last years, none of whom have been acclimated by yellow fever, and they would necessarily have been exposed to its attacks, and amongst the first victims, had the fever been epidemic in its character. Every fact tends to prove that it was local in its origin, and confined to the locality where it originated. Its form therefore was *sporadic*."

Notwithstanding the remonstrances of the Board of Health, the Com-

missaries, in 1847, continued to encumber the bank of the river and fill up docks with every kind of decaying vegetable and animal matter, the garbage of the city and of the markets, producing most intolerable effluvia as the river receded, leaving the mass of corruption to be acted on by the sun's rays. Nor was the condition of the streets, excepting in the First Municipality, any better than in 1846. There was a considerable number of Irish immigrants who had arrived during the winter and spring, most of whom were in a state of great debility after passing through the ordeal of the ship fever; besides these, discharged soldiers arrived in almost every vessel from Tampico and Vera Cruz, whose pale countenances and attenuated frames gave evidence of long continued attacks of dysentery and diarrhœa.

Under such circumstances, what ensued might reasonably have been expected. The yellow fever appeared early in July, and about the same time in the three Municipalities, showing that the foci of disease were unusually abundant. On the 2d August, the Board of Health announced to the public the existence of an epidemic, which for about thirty days, (one-third of its duration,) from the early part of August to the early part of September, was as severe and fatal as any previous visitation. On the 18th October, the Board decided that the epidemic was at an end. No frost had occurred at the time, nor was there any until towards the end of November. Thousands of persons immediately returned to the city from the watering places across the lake and from the northern cities while the weather was still very warm, not one of whom contracted the disease: the fever gradually died away and finally disappeared.

These facts are recorded by Dr. B. Dowler, in an article on meteorology, published in the New Orleans Medical and Surgical Journal, January 1848, as follows:—

"About the middle of October frost was announced—the wish was father to the fact—frost there was none—the mercury had only descended to 57°. Even a month later, when Dr. Lindsay of this city had returned from his plantation over the lake, nearly one degree north of New Orleans, no frost had yet occurred.

"And now, in the last week of November, nearly seven weeks after the Board of Health announced the extinction of the *epidemic*—no frost arrived, to the great scandal of all faithful theorists; the miasm—the contagion, if any, was not frozen into substance. The summer heat was continued in autumn, and yet the city, overflowing with *non-acclimates*, continued to improve in its sanitary condition. Strangers rushed into houses, where, in some instances, every person had died a few days before, without being infected. Think of this miasmatists, contagionists, quarantinists!"

Such are the facts, so far as this city is concerned; they do not afford the slightest evidence of contagion or importation.

The following remarks from the New York Commercial Advertiser are, I believe, strictly correct.—

"We hope that the men, whose old-fashioned opinions bind upon our commerce the vexatious burthens of our present quarantine, will be on the look out for wisdom. Yellow fever is rife at New Orleans and all around the Western side of the Gulf of Mexico. Our vessels

of war are cruising to Pensacola and other ports, and ships with the sick and dying are putting in at all the ports of the Gulf and of the Atlantic, and the sick ships are visited and assisted at sea, and yet contagion is silent. We have no notice of contagion anywhere, not even those semblances of it which are often seen."

The exemption of the city of Natchez from yellow fever for some years past, has been attributed to the quarantine laws established there. I endeavored to show on a former occasion how easy it was to evade those laws; as citizens flying from the yellow fever in New Orleans had only to pass higher up the river to the next turn beyond Natchez, and then return in the next steamer descending the river and land at Natchez. And the same plan might be pursued by persons coming from Vicksburg, where there might be yellow fever.

But there is direct proof in the New Orleans Medical and Surgical Journal for May, 1848, in the following letter to the editors.

"NATCHEZ, April 5th, 1848.

"GENTLEMEN:—Observing in the January number the statement that quarantine was maintained at Natchez and Vicksburg, that the former escaped and the latter had the (yellow) fever, and that no correction of an error therein contained has appeared in the March number, I desire you to publish in the May number that *several* cases of yellow fever occurred here during the fall of 1847.

"These cases were of persons having no intercourse with boats, or with persons ill of or convalescent from yellow fever, and who had not received goods during the summer and fall, and were therefore from local cause, be that what it may.

"In evidence that they were yellow fever cases, I am authorized to cite in addition to my own, the unhesitating opinions of Drs. Cartwright, Jones, Lyle and McPheeters.

"The quarantine was established soon after the announcement of the epidemic in New Orleans, and maintained with unusual rigor, till the official declaration that it no longer existed in your city. People were restrained from entrance into the city from one to five days; goods, if from Boston or New York, remained 24 hours at quarantine ground—if from New Orleans, 48 hours, exposed to the sun 24 hours of this time. The above, I am informed, were the rules with very few exceptions.

Yours, &c.,

C. H. S."

In the January number of the same Journal for 1848, I find in an article signed F. this remark:—"Quarantine was maintained at Natchez and Vicksburg. The former escaped and the latter had the fever, whereas of the intermediate towns between those places, Rodney, next to Natchez, was severely scourged, while Grand Gulf, next to Vicksburg, escaped."

The writer was evidently not aware at that time that the yellow fever had occurred at Natchez as well as at Vicksburg.

These facts furnish strong presumptive evidence of the animalcular origin of yellow fever, an opinion which I advanced in an article pub-

lished in our Medical Journal in January, 1846, entitled "An Inquiry into the causes of disease." (p. 464.) And I was pleased to see the same doctrine more fully discussed and maintained by Dr. Nott, of Mobile, in the March number, 1848.

I may here remark that there was no yellow fever in Savannah (Ga.) before 1817. It was produced that year by the opening of a large rice field in the north-east; five days after the field was drained, the wind changed from S. W. to N. E., when the yellow fever immediately broke out and was very fatal. I was aware of this fact before I saw it stated in the Botanical Medical Reporter, published at Cincinnati.

The interesting fever statistics taken from the books of the Charity Hospital and reported by Dr. Fenner, in the last number of this Journal have tended to confirm me in the opinion that yellow fever belongs to the great class of Southern fevers characterized by intermittent or remittent.

It is a general rule that all fevers give way before a great epidemic, or that the epidemic impresses its character on all other forms of febrile disease; but intermittent or remittent fevers do not give way entirely to yellow fever. Dr. Fenner observes that it is curious to note the gradual increase and decline of intermittent fever, at the Charity Hospital; and also the remarkable fact that this form of fever *is never entirely absent not even at the zenith of the worst epidemics of yellow fever.* (p. 52.)

In 1841, when the fever was epidemic, it appears from these statistics that there occurred at the Charity Hospital 1113 cases of yellow fever, 323 of intermittent, and 42 of remittent fever in four months—August, September, October and November.

In 1842, during the same four months, there occurred 410 cases of yellow fever, 563 intermittents, and 121 remittents. It is more probable that the disease was sporadic and not epidemic that year.

In 1843, when it was again epidemic, there were 1053 cases of yellow fever in six months, beginning with July and ending with December; during the same time, 587 cases of intermittent and 194 of remittent fever.

In 1844, it was evidently sporadic, there being but 152 cases of yellow fever against 990 of intermittent, and 174 of remittent fever.

In 1845, there was but one case of yellow fever in the Charity Hospital.

In 1846, it was again sporadic; in four months, September, October, November, and December, there were 148 cases of yellow fever in the Hospital, 1126 of intermittent, and 68 of remittent fever.

In 1847, commencing with July and ending in November, there occurred at the Charity Hospital 2811 cases of yellow fever, 983 of intermittent and 188 of remittent fever. This epidemic is considered by many quite as severe as any that ever occurred here.

From the foregoing facts, I think at least three inferences may be drawn. 1.—That when the intermittents predominate to a considerable extent, throughout the summer and fall months, over the yellow fever cases, as in 1844 and 1846, the disease may be set down as sporadic.

2.—When in August and September the intermittents suddenly decreased, with an immense preponderance of yellow fever, as in 1847, then there can be no doubt of the existence of an epidemic, as also in 1841 and 1843.

3.—That the intermittents never being entirely suppressed during the climax of the epidemic yellow fever, and increasing so rapidly as the epidemic declines, considerable affinity would appear to exist between the two descriptions of fever.

If this be true, which for many years past I have never doubted, then it would forever settle the question of quarantine in the case of yellow fever.

Belonging to the extensive family of fevers of intermittence, it would no more be considered contagious than they are, and the question of importation from some foreign seaport, in the Gulf or in the West Indies, would cease to be agitated. All our attention would be directed to the local causes of fever, and a system of sanitary regulations would be adopted and persevered in, the beneficial effects of which would soon become apparent.

In the report made to the Royal Academy of Medicine, on the subject of the Plague, &c., by Dr. Prus, it is stated that when Dupuytren inquired of the young Egyptian student, who had been brought by Clot-Bey to Paris for medical education, what was the opinion of the most enlightened men in Egypt respecting the origin of the Plague, the answer he gave was, "*la peste vient de la terre.*"

The accumulated filth amongst the lowest class of Egyptians, who inhabit the alluvial banks of the Nile, generates the plague. The same cause somewhat modified produces the yellow and intermittent fevers in New Orleans, which is located on the alluvial bank of the Mississippi, and in the same latitude as Grand Cairo.

If, then, our enterprising citizens really desire that this city should become exempt from that terrible scourge, the yellow fever, they must elect men to the different Councils who will as far as possible carry out those hygienic measures, based on the experience of ages, which have heretofore been repeatedly recommended to the City Authorities,

I am aware that, owing to local circumstances, great difficulties exist in the way of efficient sanitary arrangements; this, however, should only prompt to more energetic and persevering efforts.

The streets of the First Municipality being laid off at right angles, and there being great facility of draining by means of the Bayou St. John, it is much more easily kept clean than the other two Municipalities. The Draining Company have rendered good service, and the water-works on the Levee have no doubt been beneficial; but much remains to be done. The offal of the markets and other trash should not be thrown on the bank of the river to undergo decomposition and pollute the atmosphere. It should either be thrown into the current of the river, or be carried back and deposited in the swamp. The streets running parallel with the river derive no benefit from the water-works; an attempt is sometimes made to cleanse the gutters in those streets by using the hydrant water, but ten times as much as is now used would be required to accomplish the object thoroughly.

The offal from dwelling houses should be promptly removed, and not

suffered to remain for hours in the hot sun. Commissaries should be compelled, under pain of fine or dismissal from office, to examine back-yards and to cause any nuisance that might be found therein, to be immediately removed. Lime may be used to great advantage in privies and other back places to correct offensive smells, and purify the atmosphere, and Commissaries and Health Wardens should be authorised by law to direct its application whenever necessary.

The gutters in many parts of the city, particularly in the Second and Third Municipalities, are exceedingly offensive; they should be particularly attended to and kept clean, for a focus of disease may be produced by a very slight cause.

No stagnant water should be tolerated in any part of the city, neither in gutters, nor in lots; and the laboring class all over the city should be supplied, free of expense, with an abundance of wholesome river water.

Bathing in warm climates is not merely a luxury, for there is really nothing more conducive to health. In such climates, it has been practised by all nations, in all ages.

There should be floating baths alongside of the Levee in each Municipality, provided for the laboring class, free of expense; such baths would probably be the means of saving the lives of many boys, more or less of whom are drowned every year when bathing in the Mississippi.

Such measures have been repeatedly recommended, but without producing any important result. We must suppose that the city authorities have been, and still are, aware of their importance and practicability. Why, then, has so little been done? How is it that we hear every season the same complaints of the filthy condition of the gutters and streets in many parts of the city—of the offensive garbage thrown on the bank of the river and into the docks, and of stagnant water in unoccupied lots, annoying the whole neighborhood? Are the members of the city Councils afraid of the expense? Suppose a hundred thousand dollars were annually appropriated to improve the sanitary condition of the city,—I believe there is not an intelligent man in the city who would not heartily sustain the Councils in such an enlightened and benevolent policy.

It is impossible to calculate what the city loses by a severe epidemic, such as occurred last year. In 1845, when a false alarm of yellow fever was raised, Mr. Jones, who was at that time Editor of the *Bulletin*, and who was travelling in the Western country when the report reached there, stated on his arrival in this city, that produce of the value of at least two millions of dollars, which was on its way here, was immediately diverted to the Eastern cities. From thirty to fifty thousand dollars are expended during the existence of an epidemic, in private charities by the Howard and other benevolent societies, and by individuals in all parts of the city. It has moreover been estimated, that from fifty to one hundred students were last fall deterred from attending our University, in consequence of the great alarm created by the extravagant reports which circulated in the neighboring States. But what is all this in comparison with the loss of life, and the terrible domestic affliction we are doomed to witness. It is not only the recently arrived immigrants who are swept away by the pesti-

lence—many of our most valuable citizens, ornaments of society, are also among the victims.

As regards the probable prevalence of yellow fever any one season, I think there is little, if anything, to be inferred from the ranges of the thermometer and barometer, or the quantity of rain that may fall. In 1845, there were 103 rainy days, the total quantity of rain amounting, according to Mr. Lillie's rain-gauge, to 2.419 feet. In 1846, —114 rainy days—quantity of rain during the year—6.39 feet. In 1847, the rainy days amounted to 104, and the quantity of rain that fell was 8.407 feet. This year, up to June 24th, there have been 58 rainy days, and 6.88 feet of rain have already fallen.

The city is at present (July 20th) healthy for the season of the year. No yellow fever has been imported from Vera Cruz, although there was free intercourse between this city and that place during the prevalence of yellow fever there in April and May. Several persons who brought the disease with them from Mexico, died in the Military Hospitals, and with them it ended. A few deaths from the same cause, at the rate of one per week for three weeks, have occurred in the Charity Hospital, which were probably sporadic cases of domestic origin. Last week, four deaths occurred in that Hospital, the disease supposed to be brought from Mexico.

Nearly twenty thousand troops returning from Mexico have been temporarily encamped in the neighborhood of the city, awaiting transportation, and part of the time exposed to drenching rains. They neither brought nor contracted any disease here.

The sick soldiers in the Military Hospitals, who have come direct from the Hospitals in Mexico, form a numerous body. Whatever their diseases may be or have been, they are confined to the several localities, and have had no influence whatever on the health of our citizens.

We may escape an epidemic this year, although the same causes of disease which we are capable of observing, and which have already been described, are as abundant now as they were last season.

The famine of 1846 in Ireland, accompanied as is usual in that country by pestilence, caused a very large emigration to this continent in 1847. The unfortunate passengers improperly crowded in the ships, with but a scanty supply of bad provisions, and short of water, filled the hospitals of Canada and the United States with cases of ship fever.

A fair proportion of it was brought to this city. Every passenger vessel was visited by one or more members of the Board of Health, immediately on arrival in our port. The nature of the disease was carefully studied, and no evil arose from the course that was adopted by the Board. The whole subject should have been left to their care and discretion, as the whole responsibility rested upon them.

At a meeting of the general council on Friday, the 18th June, 1847, a message was sent in by the Mayor, part of which attracted the attention of the Board of Health, and a committee was appointed to address the council on the subject, which duty was performed as follows:

To the Honorable members of the General Council.

"GENTLEMEN.—In the Delta of the 19th instant, we find the following as constituting a part of the proceedings at the sitting of the council the 18th June."

"The Mayor then called on the council to devise some measure for the purpose of effectually preserving the public health and the salubrity of the city."

He says that vessels come to this port almost every day from foreign countries with a large number of emigrants, many of whom are laboring under infectious or contagious diseases. He could not too strongly urge the importance of enacting an ordinance having for its object to prevent the entrance into the port of New Orleans, of any vessel having on board passengers affected with epidemic or contagious diseases.

Conscious of having faithfully discharged their duty, as far as it was in their power to do so, the members of the Board of Health presume that the Mayor only alluded to such measures as might be deemed necessary to prevent the introduction of contagious or infectious diseases into the city, although the remarks are susceptible of a very different construction.

Now as to all general measures for preserving the public health, urged on various occasions by the Board officially, or by individual members, such as watering the streets, cleansing the gutters, removing all filth and offal to some point where it cannot prove a nuisance to the public—filling up low lots to prevent the existence of stagnant water—covering the surface of the earth within the bounds of the city with something that will effectually prevent exhalation from the alluvial soil on which it is built—providing an abundance of pure water for the poor and laboring classes, by means of public hydrants, or pumps—also public baths, &c. The members of the Board regret that they have met with so little co-operation on the part of the city authorities.

Much credit is, however, due to the council of the First Municipality for having established efficient water works in the front, and a draining apparatus that works admirably, in the rear of that Municipality.

The members of the Board of Health cannot but feel surprised that they were not officially consulted as to the nature of the diseases with which the foreign emigrants are affected, and that the Mayor has assumed the responsibility of pronouncing them to be so contagious or infectious as in his opinion to make it necessary to call for such stringent measures. It is well known to his Honor that every vessel reported to the Secretary of the Board of Health has been visited by members of the Board, and if permission has been given to the captain to carry his vessel to the wharf and send his sick passengers to the hospital, the members of the Board have yet to learn that the public health has suffered in consequence of such proceeding.

The diseases with which the emigrants have recently suffered are diarrhœa and the ship fever—a sort of typhoid disease engendered in the hold of a vessel crowded with people who were half starved when they embarked, and lived during the passage on damaged food. No one will pretend to say that diarrhœa is contagious or infectious, and not one particle of evidence has come before the Board to prove that the ship fever is contagious in this climate, but evidence enough to

prove that it is infectious to a very limited extent, and under peculiar circumstances.

In private practice where there was but one or two sick emigrants in a house, all that we have seen or heard of, have recovered speedily, and we have heard of none that have died; nor has the disease been communicated in a single instance, although circumstances were favorable to its spread by contagion.

From Dr. Wedderstrandt, of the Charity Hospital, we learn that when a large number of emigrants affected with ship fever are accumulated in a ward, other persons who have been confined for some time in the same ward, breathing the same infected atmosphere, have contracted the disease, and in a few instances death has occurred. We see no reason why other cases should not be kept separate, instead of being exposed to infection in a crowded ward with ship fever patients. The number of such cases, however, has been but small, and not the slightest danger is to be apprehended for persons in any other part of the hospital, or even for those who pass in and out of the room, remaining there but a short time, as nurses, physicians, visitors, &c., danger exists only for such persons as are confined in the ward, and can breathe no other atmosphere.

In this case it is evident that the ward is a focus of disease, beyond the precincts of which there is no danger of the fever passing.

As wards therefore may become foci of disease when crowded with ship fever cases, and as in several instances persons who were confined in such wards for a certain time have contracted the disease—and that not a single case of the fever has been communicated directly by one to another person, the best plan to obviate the evil would be to scatter the cases as far apart as possible, in order to dilute and render innocuous the vitiated atmosphere. * * * * *

The impression entertained by some persons that this ship fever must necessarily be contagious in Louisiana, has no doubt arisen from certain remarks in the Northern papers. We have heard of the death of one or two physicians in the vicinity of New York, who contracted the fever from the sick emigrants whom they visited. * * * * *

The typhus fever, however, of which the ship fever is one variety is a disease peculiar to northern climates precisely as yellow and other malignant fevers are peculiar to hot climates in the vicinity of the tropics. A difference of opinion has ever prevailed amongst medical men as to the contagiousness of the common typhus fever, but all agree that under certain circumstances, as in a crowded hospital, or jail or ship, or amongst a people emaciated by famine, whose moral and physical energies are alike prostrated, it often becomes highly infectious, so that very few who are exposed to the vitiated atmosphere escape its influence.

Most of the emigrant ships were bound to Quebec, Boston, or New York; and sailing in the winter and spring when the weather is very cold in those latitudes, circumstances were favorable to the generation of ship or even genuine typhus fever.

But the case is very different with ships bound to New Orleans; their course is steady southward from the moment of departure until they reach the warm climate of the West Indies, where the fever be-

comes greatly modified and disarmed of half its terrors ; and could the passengers remain on deck from that time until they arrive here enjoying the pure air, it is probable that all would speedily recover. * * *

The Charity Hospital is too full at present to admit any more cases, and we would therefore recommend that some other building or buildings, favorably situated for free circulation of air, be provided, while we assure you there will be no difficulty about medical attendance.

"The great object is not to crowd the sick, but to keep them well apart. There should not be more than three or four in one room, hence several small buildings would be preferable to one large one. It is improbable as the summer is so far advanced, that many more emigrant ships will come here until the next season, consequently any arrangement for the accommodation of the sick may be temporary and will incur comparatively small expense."

Signed,

WM. P. HORT,
Y. R. LEMMONIER,
J. U. LANDREAUX,

} Committee
of the
} Board of Health.

The opinions expressed in the foregoing letter on the subject of ship fever have been fully sustained here and elsewhere up to the present moment ; and it is difficult to account for the panic which seized some of our legislators last winter, and the zeal with which the city council were suddenly inspired, which, however, resulted in nothing more than the appointment and frequent meetings of committees from all the councils and the Board of Health, and the passage of a string of resolutions, of which I shall here insert four.

"1. The committee consider that there is no danger of the ship fever becoming epidemic—nor that the fever can extend beyond the hospital.

2. That measures should be adopted by the proper authorities to relieve the Charity Hospital.

3. That suitable buildings be rented or purchased near the river between the Convent and Barracks for the reception, and taking care of such persons as may arrive in this city affected with ship fever.

4. That measures should be adopted to prohibit any vessel having on board persons affected with ship fever from coming up to the levee. Every such vessel should be required to anchor opposite to where the above building may be located, and all persons afflicted with fever should be removed therefrom and the vessel thoroughly ventilated and cleansed."

The plan adopted and persevered in by the visiting member or members of the Board of Health, was, after due examination of all the passengers, to permit those who were well, to go where they pleased ; to commit to the care of friends such of the sick as had that advantage, where they were attended in private practice ; and to send all the rest of the sick to the Charity Hospital. We never met with a captain who was not glad enough to cleanse and purify his ship as soon as he was rid of the passengers.

The following extraordinary resolution in which the facts stated or assumed are utterly at variance with the actual facts, was introduced in the Senate by Mr. Hereford.

"Be it resolved, &c. : Whereas it is apparent from good information that the city of New Orleans is now visited by a fatal epidemic in the form of ship fever, the increasing rapidity of which surpasses conception, therefore be it resolved that the Legislature do now adjourn to meet in the city of Baton Rouge on the 15th March next."

The New York Academy of medicine appointed a committee last year to investigate the character of the disease known under the denomination of ship fever. The following resolutions were submitted to the Society :

1. That though there has been a decided increase of typhus or ship fever during the present season, such increase is only in proportion to the increased emigration and the bad condition of the emigrants.

2. The disease is confined almost exclusively to emigrants, and to those who are in constant and direct attendance upon those who are sick.

3. No danger need be apprehended of the disease becoming epidemic ; and that with a due regard to cleanliness and ventilation, the citizens have no cause whatever to be alarmed on the subject.

About the same opinion is expressed in the Boston Medical Journal.

"The disease is not confined to emigrants while at sea, but frequently breaks out after they have landed.

Clean beds, well ventilated rooms, and an avoidance of crowded hospitals are almost certain cures in a majority of cases.

It is not contagious in the ordinary sense of that term, though persons whose blood happens to be in a bad condition, if thrown among it are liable to injection."

In the Montreal Herald of the 14th June, 1847, it is stated that the disease (the ship fever) is confined to emigrants.

"We know of no instance of the fever having been introduced among our own people by the emigrants."

Such facts and authorities should be considered sufficient to allay all alarm of ship fever amongst our citizens and public authorities.

We have had from time to time information of the progress of the Asiatic Cholera in the eastern part of Europe. Late intelligence represents it to be raging in Moscow ; it may again cross the Ocean and traverse our continent. No quarantine measures or military posts guarding all the approaches to a country, have ever been able to retard or avert its progress. Cleanliness, however, is its greatest palliative ; and all writers and observers agree that whatever part of a town is most filthy, with stagnant water &c., feels especially the weight of that severe scourge.

The ravages would be dreadful in this city in the present condition of the streets, wharves and low lots filled with stagnant or putrid water. The bare possibility of such a visitation should stimulate our city authorities to carry into effect those sanitary measures which have now, and so often before, been recommended to them as the results of common sense and long and universal experience.

MONTGOMERY, ALA., August, 15th, 1848.

2.—*Ethereal Solution of Prepared Cotton.*

Boston is the lucky place of the accidental discovery of the surgical use of the *Ethereal Solution of Prepared Cotton*—a discovery second only in importance to that of *Etherization*. It must be universally regretted, that the discoverers of so useful an article should have thought proper to keep secret the particular formula for its preparation. Such a course belongs only to charlatanry, and is unbecoming those who have the dignity, the honor, the interest and usefulness of our profession at heart.

A very ridiculous contention (as to who was the first to use this preparation surgically) has arisen between two medical students, in which the two parties have played, in miniature, a farce very similar to that so recently enacted by the several claimants for the discovery of *Etherization*. We might have supposed that the mutual demolition of the claimants for this honor, would have been a salutary lesson to others. The world now enjoys all the benefits of the great blessing which these Boston discoverers tried to hide from us; and who now, out of Boston, cares a fig what agency Dr. Jackson or Dr. Morton may have had in the matter, since both have forfeited their claim to any credit whatever, by their disgraceful attempt at concealment—1st, by pretending that the agent was something else than what it really is; and 2d, by taking out a *patent*, so as to monopolize the *exclusive privilege of selling rights* to use their new compound. Their weakness is pitiable. Their prostitution of our noble calling to the vile purposes of gain is most melancholy.

And thus might we speak of the two young combatants for the honor! of bringing into notice the new adhesive solution. They have forfeited all claim to any honor, for the simple reason that they, too, have shrouded the subject in mystery and kept secret the formula for its preparation, and this with the sordid view of making a few dollars out of its manufacture. They are less to blame, however, than their preceptors and friends, who ought to have advised them as to what will be expected of honorable gentlemen in our enlightened profession. There must be something rotten in the professional atmosphere of Boston, else these things could not be so.

The preparation of this elegant adhesive solution could not always remain secret; and I am happy to inform you that Mr. S. Hutchings, of this city, has discovered a formula by which it may be made. With commendable magnanimity he freely gives it to the Profession, of which he is *not* a member. He has kindly furnished me the following note for publication:—

“The undersigned, Chemist and Apothecary, at the establishment of ‘Coster & Coxe,’ Montgomery, Ala., having instituted a series of experiments for the purpose of making the ‘*Ethereal solution of Prepared Cotton*,’ succeeded at last in producing a ‘solution’ which appears to be superior to that manufactured in Boston.

The following is my formula :

℞ Sulphuric Acid, (Commercial,) ℥ iv.
Nitric Acid, (Farr's Chemically Pure,) ℥ ii.
Cotton, ℥ iii.

Mix the acids. Saturate the cotton in the mixture for six minutes ; then gently press off the acids. Allow the cotton to remain for an hour in the fumes, in a covered vessel ; then wash repeatedly in water so as to remove the slightest taste of acid, and dry thoroughly in the sun.

℞ Prepared Cotton, grs. x to xii.
Ether Sulphuric, (Commercial,) ℥ i.—M.

The cotton should be dissolved in the Ether, as soon as it is perfectly dry ; if kept for any length of time, (for instance, a week,) it loses to some extent its solubility, which may be owing to its absorption of moisture from the atmosphere.

A common linen tape saturated with this solution, to the extent of one square inch, held up firmly *forty pounds* ; and half of one square inch (cotton tape) held up *twenty-five pounds*, for the space of six hours, which fell only on the breaking of the tape. In both instances the tapes were attached by the solution to the edge of a board. It will hold equally as well on the surface of the body, and was applied to the board only because more convenient."

S. HUTCHINGS.

The prepared cotton is very soluble in Commercial Sulphuric Ether, but wholly insoluble in *pure Ether*. If about a drachm of anhydrous alcohol be added to ℥ i of *pure Ether*, holding suspended about 10 grs. of the cotton, it will dissolve it readily. Made in this way, it has very much the appearance of the Boston preparation, which deposits a beautiful *cotton precipitate*, on standing, easily redissolved by agitation.

If instead of *absolute alcohol*, the common *rectified spirit* be added, the solution becomes gummy, looking like a thick solution of starch, (caused no doubt by the presence of water,) and is not fit for use, though it possesses adhesive qualities in a less degree. Made according to Mr. Hutchings' formula given above, the solution has very much the appearance of a nice preparation of simple syrup.

In the application of the solution to common incised wounds, nothing more is necessary than to bring their edges into apposition and smear the wound over with it, by means of a camel's hair pencil. But in wounds of greater magnitude, requiring sutures or straps, it will be necessary to use strips of linen or cotton fabric, of the requisite length and breadth, well moistened with the liquid. For this purpose, let one end of the strip be stuffed into the vial containing the preparation. When thoroughly saturated, let it be applied ; and wait patiently till it becomes perfectly dry, which will be in from $\frac{1}{2}$ minute to 3, 4 or 5, according to circumstances. The other end of the strip may now be wet and applied in like manner. In some wounds, as for instance, after amputation, the whole strip may be saturated at once and applied over the parts. Be sure of one thing, not to feel afraid of wasting the article ; and of another, not to be in a hurry—for a failure sometimes arises

from using the solution too sparingly, and again, from not waiting till it gets thoroughly dry.

I have used this new adhesive solution pretty extensively, and can freely testify to its great value, having dressed wounds on the scalp, the hands, the feet, and other parts of the body, which have healed principally by the first intention, but which without it would have required (some of them) many stitches, having to heal at last, in a good degree, by the granulating process.

Within the last fortnight, I have used it to dress the stumps of an amputated thigh and a leg, doing away entirely with sutures, and holding the edges of the wounds together more perfectly and more beautifully than stitches can possibly do. But its most triumphant use in my hands, has been in remedying a case of *Hernia Cerebri*, which I feel pretty sure would not have been done by the means in ordinary use. This case I consider of sufficient importance to give the details entire in some future number of this Journal.

Yours truly,

J. MARION SIMS.

NEW ORLEANS, SEPTEMBER 1, 1848.

LOUISIANA STATE MEDICAL ASSOCIATION.

In our July number we, in our remarks upon the proceedings of the "Attakapas Medical Society," promised to aid in bringing about a State Medical Convention. Since that time we have received a copy, in handsome pamphlet form, of the minutes of the proceedings of the Medical Convention of South Carolina, which convened in Charleston, in February, 1848. The zeal, industry, harmony and ability with which every step of the proceedings of this new Convention were characterized, has inspired us with increased interest in the subject, and led us to believe that the Physicians of Louisiana, as a body, active and intelligent, would hail with no ordinary pleasure an opportunity to emulate the example set before us by the Profession of a neighboring State. In vain have we, in almost every State of this great Union, sent in prayer after prayer and petition upon petition, to our respective State Legislatures, asking that protection which was not denied the artizan, the mechanic and, in some instances, the gambler, yet our appeal, although pressed in the most respectful language, and sustained by argument, as weighty as truthful, has been disregarded and our motives condemned as arbitrary and selfish. Wearied with asking for bread, and receiving in stead stones, the Profession is at last, we are truly proud to perceive, about to rise and vindicate its own claims to that respect and position in society, which the prejudices of a few and the ignorance of the many have so long denied to it.

Let the Profession, then, of the State, meet in the city of New Orleans, sometime during the winter or spring, and organize an association, the object of which shall be to lay down a platform upon which all the respectable physicians of the State may unite and discuss in a calm and temperate tone the question of "medical reform," and such others as affect the interest and good report of the Profession.

If union and harmony give additional strength to physical force, how much more do they augment intellectual power. The inducement for such an association is greatly enhanced from the fact, that in Louisiana we have medical men from almost every part of the world, differing widely among themselves, as might reasonably be supposed, in regard to the ethics of the Profession and other questions, which, remaining unsettled, have and must continue to affect seriously its dignity and character in public estimation.

In Pennsylvania, New York, South Carolina and some other States, we believe the physicians have, with commendable unanimity and a cordial co-operation, held conventions, in which questions involving the future interest of our science and the good of humanity have been discussed and, as far as practicable, settled. These associations have already inspired the medical public with confidence in its high mission, and infused into the breasts of those who labor to promote its usefulness an enthusiasm which we trust may spread from State to State, until it shall enlist the feelings and secure the co-operation of all who are anxious to advance the Profession and mitigate the infirmities of human nature. Movements of this kind, carried on in the proper spirit, will do more to destroy the influence of quackery and charlatanry than a folio of legislative enactments, which could never reach the evils complained of, or enlighten the public mind on the subject of medicine.

The Attakapas Medical Society indicates the first Tuesday in March, 1849, as the most convenient time for the convention to assemble in *New Orleans*. We have nothing to urge against this suggestion; on the contrary, we think the time well chosen, and most likely to meet with the approbation of the Profession generally throughout the State. Let, then, each Parish send one or more delegates to represent the interests and views of their brother practitioners in the Convention; in this way every section of Louisiana will be fairly represented, and the organization of the association will be complete.

We shall await the decision of the Profession on this important question.

A. H.

HEALTH OF THE CITY.

In our editorial remarks for the July number of this Journal, we stated that rain fell almost daily throughout the month of June; and now we may make the same statement in regard to the months of July and August. We shall put down in figures, obtained through the politeness of Mr. T. D. Lillie, of this city, a most careful and accurate observer, the quantity of rain that fell from the 1st day of June to the 18th day of August, both for 1847 and '48.

	Inches.
For the month of June, 1847, ...	7.233.
“ “ “ July, “	17.885.
Up to 18th of August, “	5.450.
Total,	30.568.

	Inches.
For the month of June, 1848, . . .	30.818.
“ “ “ July, “ . . .	9.242.
Up to 18th of August, “ . . .	6.843.
Total,	46.903.

Or nearly four feet of water.

It must be recollected that at this period in 1847, the epidemic yellow fever was raging with great violence, but it remains to be determined what influence was exerted in the development of the fever, by the great quantity of rain that fell during the month of July, 1847.

If heavy and almost constant showers for three months, can generate yellow fever in New Orleans, we may well look forward with painful apprehensions. But it must be admitted, that the most careful observations and sapient predictions are constantly baffled in reference to the appearance of our epidemics; for we maintain, that as yet we know nothing of the causes of yellow fever. It prevails alike amid daily showers or eternal sun-shine. We believe that external influences, such as heat, moisture, and the like, may, under certain circumstances, modify, aggravate or check the fever; but to undertake to determine the precise nature of such influences, or in what manner they act, would be a fruitless effort.

Having stated the quantity of rain fallen for the months of June, July and a part of August of this year, we shall now briefly sum up the number of deaths from yellow fever, as reported to the Board of Health, beginning with the first case. For the week ending the 17th June, 1848, 1 case was reported (not authentic). For the week ending 24th June, 2 cases (from Mexico); ending July 1st, 1 case; ending 8th, 5 cases; ending 15th, 6 cases, (4 of them from Vera Cruz); ending 22d, 6 cases; ending 29th, 16 cases, (some imported); ending August 5th, 12 cases; ending 12th, 26 cases; ending 19th, 38 cases, and ending 25th, 96 cases of yellow fever.

The above statement gives unequivocal evidence of the gradual increase of fever in our midst and fears are expressed that we shall have an epidemic. As far as our observation and inquiries have extended, we think the type of the disease unusually mild; in many instances so slightly marked as to require but a few hours' confinement and little medicine. Some have designated it the *acclimating fever*; but we apprehend that such acclimation will scarcely stand the test of a *bona-fide* epidemic.

Again, we find some cases characterized by the most violent symptoms, such as hemorrhage, black vomit, &c. The disease may be said to be *endemic* in the Charity Hospital, as it is the *prevailing* fever in that institution; in private practice, *sporadic* cases exist in almost every part of the city—in Algiers and Gretna, on the right bank of the river, and in Lafayette. It is confined chiefly to the poorer and exposed part of our population, hence the number of cases in the Charity Hospital.

On the 28th of August, the Board of Health of this city, issued the subjoined card, to allay and satisfy the public mind:—

BOARD OF HEALTH.

"At a meeting of the Board of Health, held August 28, 1848, the following preamble and resolution were unanimously adopted:

"It being incumbent on this Board to allay any unnecessary alarm in the city from exaggerated accounts now current respecting the extent and character of yellow fever—

"*Resolved*, That the President of this Board of Health be instructed to inform the public, over his own signature, agreeably to this resolution, of the gradual increase of the disease—its mild type, and that at present it has not assumed an epidemic form.

A. D. CROSSMAN,

President of the Board of Health.

A. HESTER, Secretary."

Although cases of yellow fever have been imported into this city from Mexico, from day to day, during the summer, yet in no instance has the disease been communicated to any of our citizens. This fact will, we trust, settle forever the question of importation. The cases that originated in this city had no connection or communication with such as were brought into the city. We therefore conclude, that the yellow fever originates in this place—is an endemic disease, and we may look in vain for quarantine to exclude it from our midst.

A. H.

List of interments in the City of New Orleans from the 28th of May, to 19th of August, 1848 being a period of 12 weeks.

Abscess, 1 ; do., hepatic, 2 ; Accidental 2 ; Accouchement, 2 ; Affection, chronic, 6 ; do. verminose, 1 ; Anasarca, 2 ; Anemia, 2 ; Aneurism, 1 ; Angina malig., 2 ; Apoplexy, 30 ; Arachnitis, 1 ; Ascites, 2 ; do. chronic, 1 ; do. hepatic, 1 ; Asphyxia, 1 ; Births, premature, 7 ; Bowels, obstruction of, 1 ; Brain, concussion of, 1 ; do. compression of, 1 ; do. congestion of, 33 ; do. dropsy on, 1 ; do. effusion upon, 1 ; do. softening of, 3 ; Bronchitis, 1 ; do. chronic, 1 ; Burn, 2 ; Cancer, 3 ; Catarrh, 7 ; do. chronic, 1 ; do. pulmonary, 1 ; Cerebritis, 14 ; Cholera infantum, 7 ; Cholera Morbus, 2 ; Colica, 2 ; do. bilious, 1 ; Colitis, 1 ; Congestion, pulmonary, 1 ; Consumption, 103 ; Convulsions, 56 ; Coxalgia, 1 ; Cramp, 1 ; Croup, 3 ; Debility, 38 ; Del'm. Tremens, 10 ; Dentition, 15 ; Diarrhœa, 39 ; do. chronic, 42 ; Disease, complicated, 1 ; do. inflammatory, 3 ; Dropsy, 19 ; Drowned, 46 ; Dysentery, 65 ; do. chronic, 62 ; Endocarditis, 1 ; Encephalitis, chronic, 1 ; Enteritis, 32 ; do. chronic, 4 ; Entero-Colitis, 1 ; Epilepsy, 1 ; Erysipelas, 1 ; Fever, 11 ; do. adynamic, 2 ; do. ataxic, 1 ; do. bilious, 4 ; do. do. remittent, 2 ; do. congestive, 35 ; do. hectic, 2 ; do. intermittent, 4 ; do. malignant, 2 ; do. do. intermittent 1 ; do. do. nervous, 3 ; do. mercurial, 1 ; do. pernicious, 6 ; do. puerperal, 1 ; do. putrid, 3 ; do. remittent, 4 ; do. scarlet, 23 ; do. do. maligna, 1 ; do. typhoid, 25 ; do. typhus, 45 ; do. do. congestive, 3 ; do. yellow 113 ; Gangrene, 4 ; Gastritis, 3 ; do. acute, 3 ; do. chronic, 2 ; Gastro-duodenitis, 1 ; Gastro-enteritis, 2 ; do. do. chronic, 6 ; Gastro-entero-encephalitis 1 ; Gastro-hepatitis, 1 ;

Heart, disease of, 9; do. hypertrophy of, 3; Hemorrhage, internal, 3; Hæmoptisis, 2; Hepatitis, 7; do. chronic, 7; Hydrocephalus, 1; do. acute, 1; Hydrothorax, 2; Hydropericarditis, 1; Indigestion, 1; do. from ice, 1; Intemperance, 5; Intestine, inflam. of, 1; do. perforation of, 1; do. ulceration of, 2; do. tube, disease of, 1; Kidney, disease of, 2; Labor, premature, 3; Laryngitis, 2; Leg, gangrene of, 1; Liver, disease of, 1; Lungs, abscess in, 1; do. congestion of, 1; do. apoplexy, of, 1; do. gangrene of, 3; Marasmus, 16; Measles, 23; Meningitis, 9; do. cerebro-spinal, 1; Myelitis, chronic, 1; Nephritis, chronic, 1; Old age, 3;* Paralysis, 1; Peritonitis, 4; Pertussis, 11; Phrenitis, 3; Pleurisy, 3; Pleuro-pneumonia, 2; Pneumonia, 14; do. typhoides, 1; Poisoned by Laudanum, 4; Purpura Hemorrhagica, 1; Rheumatism, 1; Scald, 4; Scurvy, 1; Scrofula, 1; Scrotum, gangrene of, 1; Skull, fracture of, 2; Small-Pox, 28; Sore Throat, 2; Spasms, 2; Spinal marrow, disease of, 1; Spine, injury of, 1; Still-born, 61; Stomach, cancer of, 1; Sudden death, 1; Suicide, 1; Sun stroke, 17; Syphilis, 3; do. secondary, 1; Tabes Mesenterica, 2; Tetanus, 8; Tibia, necrosis of, 1; Trismus Nascentium, 25; Typhus, Abdominalis, 1; Ulceration, venereal, 1; Uncertain, 203; Uterus, hemorrhage from, 1; Uterus, cancer of, 2; do. carcinoma of, 1; Wound, penetrating, 2. Total, 1478; of which 432 were under 10 years; 1208 were white, and 278 colored.

(Extracted from the Reports of the Board of Health.)

A. HESTER,

Secretary.

PROFESSOR W. M. BOLING, OF ALABAMA.

We are pleased to learn that our esteemed correspondent, Dr. W. M. Boling, of Montgomery, Ala., has been appointed to the Chair of *Materia Medica and Therapeutics* in the Memphis Medical College, and that he will deliver his maiden course of lectures at the ensuing session of that institution. We congratulate the Memphis School on this accession to their Faculty. Although not personally acquainted with Dr. B., we can testify to his high Professional reputation, and we feel quite satisfied that he will do honor to any position in which he may be placed. We hope still to number him amongst the contributors to our Journal.

NECROLOGY.

DIED at Cincinnati, Ohio, whither he had gone for the benefit of his health, on the 16th July, 1848, Dr. CHARLES A. LUZENBERG, for many years a prominent Physician and Surgeon of this city. For some months, Dr. L. struggled with a *valvular disease* of the heart, which ultimately terminated his career at the early age of 40 years. Dr. Thos. M. Logan, of New Orleans, was appointed by the *Medico-Chirurgical Society*, of which Dr. Luzenberg was President, to prepare a biographical sketch of the deceased, of which we may say more hereafter.

A. H.

* One negro of 100 years.

MEDICAL HISTORY OF ALABAMA.

Our readers are, no doubt, aware that for some time past *critical remarks* from the pen of Dr. Boling of Montgomery, on the *Medical History of Alabama*, written by Dr. Lewis of Mobile, have appeared in the pages of this Journal. We think these gentlemen should now be satisfied, as the subject has assumed rather a controversial than a critical turn—and we fear no good either to the Profession or the parties engaged, will result from its continuance. Both have written with much power and ability, and we beg them to turn their minds to other subjects.

There has evidently been manifested throughout this controversy, considerable irritation, and we hope the subject, with any ill-feeling that may have been engendered, will be forever buried.

Ed'rs.

ABSTRACT OF A METEOROLOGICAL JOURNAL FOR 1848.

By D. T. LILLIE, AT THE CITY OF NEW ORLEANS.

Latitude, 29 deg. 57 min.; Longitude, 90 deg. 07 min. west of Greenwich.

WEEKLY. — 1848.	THERMOMETER.			BAROMETER.			COURSE OF WIND.	FORCE OF WIND, Ratio 1 to 10.	Rainy Days.	Quantity of Rain. — Inches.
	Max.	Min.	Range.	Max.	Min.	Range.				
July - 1	89.0	75.0	14.0	30.13	29.95	0.18	S.	3	6	4.867
" - 8	90.7	77.0	13.7	30.17	29.88	0.29	W.	3 $\frac{1}{4}$	2	0.638
" - 15	89.0	73.5	15.5	30.20	29.60	0.60	S.W.	3 $\frac{1}{4}$	2	1.275
" - 22	87.0	74.5	12.5	30.10	29.87	0.23	S.	3	6	3.160
" - 29	84.0	74.5	9.5	30.24	30.08	0.16	S.	2 $\frac{1}{2}$	6	4.170
Aug. - 5	90.0	76.0	13.0	30.26	30.00	0.26	S.W.	2 $\frac{1}{2}$	3	0.658
" - 12	88.5	74.5	14.0	30.26	30.04	0.22	E.	3	6	4.365
" - 19	87.0	75.5	11.5	30.12	29.95	0.17	S.E.	4	3	2.190
" - 26	86.0	76.5	9.5	30.30	29.95	0.35	N.	3	3	2.125

REMARKS.—The Thermometer used for these observations is not attached to the Barometer, but is a self-registering one, and is placed in a fair exposure. Regular hours of observation, 8 A.M., 2 P.M. and 8 P.M.

The Barometer is located at an elevation of 19 feet above the level of the ocean, and is suspended clear of the wall of the building.

The Rain Gauge is graduated to the thousandth part of an inch, and the receiver is elevated 40 feet from the ground.

CORRECTION.

In the July number of this Journal, we unintentionally omitted to credit some of our able contemporaries for articles transferred to our pages.

The address delivered before the Medical Society of the County of Columbia, on the "*Influence of Mind in Disease*," by Dr. Joseph Bates, should have been credited to the "*Transactions of the Medical Society of the State of New York*," Vol. VII., Part 2.

We also failed to credit the *British American Journal of Medical and Physical Science*, for an interesting paper on the "*Modern Chemical Philosophy*," by T. S. Hunt, Chemist, to the Geological Survey of Canada.

Fiat justitia.

Ed'rs.

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OF

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MEDICAL AND SURGICAL JOURNAL.

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TO READERS AND CORRESPONDENTS.

We have received the following books for review ; also the subjoined list of exchanges :

1. RANKING'S *Abstract*, from Jan. to June 1848.
2. WILLIAMS'S *Principles of Medicine*, edited by CLYMER.
3. BELL and STOKES'S *Practice*. 4th Edit.
4. GARDNER'S *Medical Chemistry*.
5. *British and Foreign Medico-Chirurgical Review*, July, 1848.
6. *Western Journal*, for August.
7. *Western Lancet*, for September.
8. *Ohio Medical and Surgical Journal*, September. (New Journal.)
9. *Southern Medical and Surgical Journal*, September and October.
10. *Medical Examiner*.—Philad., September and October.
11. *Summary of the Transactions of the College of Physicians of Philadelphia*.
12. *St. Louis Medical and Surgical Journal*, July and August.
13. *Medical News*, September and October.
14. *Annalist*, September and October.
15. *Boston Medical Journal*, September and October.
16. *Valedictory Address to the Medical Class and Graduates of the Indiana Medical College, (Laporte University.)* By A. B. SHIPMAN, M. D. Prof. of Surgery.
17. *Prospectus for publishing by subscription* SPRATT'S *Obstetric tables*.
18. MEIGS on children.
19. *American Journal of Science and Arts*—September.
20. *Christison's Dispensatory* by GRIFFITH.
21. WILSON'S *Human Anatomy*. 4th Edit.—By GODDARD.
22. *New York Journal of Medicine* for July and October.
23. *On the Pathology of Congenital Dislocation of the Head of the Femur upon the Dorsum of the Ilium*.—By JOHN MURRAY CARNOCHON, M. D. etc., with plates.
24. *Annual Circular of the Medical Department of University of La.*
25. *American Journal of Insanity*, October, 1848.
26. *Buffalo Medical Journal*, October, 1848.
27. *North-Western Medical and Surgical Journal*, August and September, 1848.
28. ROBT. GOOCH on *Diseases of Females*. Second Edition.
29. *System of Clinical Medicine*. By GRAVES & GERHARD. Third Edition. 1848.
30. CARPENTER'S *Human Physiology*.
31. BARTLETT on the *Certainty of Medicine*.
32. DUFTON on the *Ear*.

THE NEW ORLEANS MEDICAL AND SURGICAL JOURNAL.

NOVEMBER, 1848.

Part First.

ORIGINAL COMMUNICATIONS.

An Examination of the "Reply," etc., of P. H. Lewis, M. D., of Mobile, to the Review of his Medical History of Alabama. By Wm. M. BOLING, M. D., of Montgomery, Ala.

[CONCLUDED.]

Dr. Lewis seems somewhat inclined to object to our statement in the Review, that he considers congestive fever and remittent fever entirely distinct diseases. It would be altogether unnecessary here to quote from the Medical History proofs of this, but we will refer to the paper itself, in which, *passim*, they may be found; especially as passages containing some of them will, in all probability, have to be presented as we proceed. He says—

"If Dr. Boling means that we believe congestive fever to be as distinct from, and dissimilar to *remittent fever*, as small pox, for instance, is from *remittent fever*, then we object to such a construction." Now, we say nothing in the Review in regard to what Dr. Lewis may suppose the *extent of the dissimilarity* of congestive fever to other diseases, but merely that he considers it a disease entirely and radically distinct from remittent fever. In the further progress of his remarks on this subject, in the Reply, Dr. Lewis, we think, (but will not assert positively, for it is somewhat difficult to discover his meaning,) at length arrives at the conclusion, that though he did not consider congestive fever, as different and distinct from remittent fever as small-pox is from remittent fever, he did pronounce it as distinct from remittent fever as small-pox is from measles. Now small-pox, we believe, is considered neither

shade, grade, form, nor modification of measles, but entirely and radically a distinct disease.

We quote from the *Reply* a few lines copied into it from the History, answering partly for the intended ground-work of the attempted distinction to which we have just referred.—

“That the chemical character of the soil varies with the geological formations, and that the morbid agents are as varied as these, is so evident as to require no argument. In fact it is upon this hypothesis, that the old and well-established doctrine of malaria can rest with security; for if disease were uniformly the same in regions distinguished by separate and peculiar physical characters, even in the same latitude, then that theory which is based upon the supposition that these different formations would give out emanations that are identical, would not be entitled to serious consideration.”

A *well-established* doctrine, resting with *security* on a *hypothesis*! And then *such* a hypothesis! one that implies that *all* the chemical compounds of a soil give out poisonous emanations; or, that with each variation of the chemical components, a different poisonous emanation results. We cannot perceive that the Dr. has advanced any facts, giving to his assumption even a shadow of probability; and, if we are not mistaken, the *hypothesis* must fall, and the *well-established doctrine* rest upon some other basis.

We are now about to present before the reader the very essence, seemingly, of Dr. Lewis' *peculiar* views in regard to congestive fever—a condensed definition of the disease indeed,—“*what we mean*” (in the language of Dr. Lewis himself) “*by congestive fever, as constituting in this family of diseases, one that should be classed as a separate and distinct malady.*”

We venture to request especially, the attention of the reader to this, for sure we are, that we have seldom seen so much condensed in so small a compass. There is not a line, scarcely a word of it, that is not deserving of a separate and particular notice. In speaking, however of what we are about to present as a *definition* of the disease, it may be proper for us to state, that further on, the Doctor presents us formally and separately, with what he *calls* a *definition* of congestive fever, amounting to an imperfect description—however, occupying something more than two entire pages.—

“In looking over the books, we meet with the terms typhoid pneumonia, typhoid yellow fever, &c., *which means*, that this pathological condition called *typhoid*, has been superadded, or has supervened upon a distinct and separate disorder, as the case may be. Louis and Bartlett both inform the reader, that this element is not only one very prominent in disorders wholly different from *typhoid fever*, but also in morbid conditions arising from injuries. Notwithstanding this, patient and rigid investigation has established the fact, that there is a distinct disease, a disease *sui generis*, called *typhoid fever*; and this, too, *in a large family or circle of diseases resembling it very closely*, and contended by a majority of writers to be *identical*. In like manner do we meet with the terms, adynamic pneumonia, adynamic typhoid fever, adynamic yellow

fever,&c., by which is meant that this morbid element has been superadded to the phenomena essential to these respective maladies. We will now be able to get at the question. Does this striking pathological element or condition called *adynamic* ever exist alone, from the access of disease, throughout its course, and to its termination in death or a healthful reaction? We are driven by personal observation and experience to say that it does. If this, then, brings us to the conclusion that congestive fever is a disease *sui generis*, differing *radically from all others*, we most cheerfully embrace it."

There now—in brief—if this pathological element ever exists alone, and from experience we know that it does so exist, then congestive fever is a disease *sui generis*.

It will be understood, we presume, that Dr. Lewis here refers to the term *adynamia* as a synonym of congestion. We can spare but little time to comment on this substitution of terms. *Adynamia*, according to the received definition, means, we believe, simply, want of power, or great debility of the vital powers. Perhaps no purer example of *adynamia* (of temporary duration) of all the functions, animal and organic, could be presented than the state of syncope.

We will present part of the paragraph last quoted again before the reader.—

"Does this striking pathological element or condition called *adynamic* ever exist *alone*, from the access of disease, throughout its course, and to its termination in death or a healthful reaction?"

Being doubtful in regard to the true meaning intended in the above sentence, it will be necessary to examine it, under the two constructions of which it is susceptible, for the purpose of developing its bearing precisely upon what Dr. Lewis *means by congestive fever*; for, upon its *proper* interpretation, it will, by a careful examination, be perceived, depends the proper understanding of his *peculiar* views on the subject; and he has here brought the question to a focus, as it were.

Now the literal interpretation, or more obvious meaning of the sentence, we are inclined to think is thus:—Does it ever occur, that "from the access of disease, throughout its course, and to its termination in death or a healthful reaction," an *adynamic* condition, a want of power, or in other words, a low grade of action, exists *alone*; that is, without, at *any period* of the attack, an opposite condition, a *sthenic*, or high grade of action being present? All, we presume, will admit that this may be the case; and under this construction, as there are several diseases usually considered radically and essentially different, (as for instance, those named by Dr. Lewis himself,—typhoid fever, yellow fever and typhoid pneumonia, in which the "*striking pathological element*," *adynamia*, *alone*, in contradistinction to an opposite condition or high grade of action, may exist, "from the access of disease, throughout its course, and to its termination in death or a healthy reaction;" it follows, that under such circumstances, *all, or any one of them*, becomes the *congestive fever* of Dr. Lewis. At least without a still more explicit explanation of his views, this is the only deducible inference; for if there is any *one* peculiar disease only, in conjunction with which the

adynamic condition must exist to constitute congestive fever, we are left altogether to conjecture in regard to it. On the supposition, for a moment, that the latter is the case, is this *one* disease plague—is it typhus fever—is it bilious or remittent fever—or is it typhoid fever?—or what is it? It is of course neither of the latter; for it is an object held constantly in view, in speaking of the disease in the Medical History, to establish a radical difference between bilious fever and congestive fever; and the belief in the distinct nature of congestive fever—that “it is a disease *sui generis*, differing radically from all others,” it will be remembered, is newly and *curiously* announced and embraced. in the last extended quotation we made from the *Reply*; and it will also be recollected that Dr. Lewis has indignantly disavowed the opinion, that congestive fever is identical with typhoid fever, though, in the *Reply*, even, in which the disavowal is given, in one part he quotes authorities to sustain him in using the terms *typhoid* and *adynamic* as synonymous, and in another part uses the term *adynamic* as a synonym of *congestive*. We cannot pause over the comments which this suggests.

“Does this striking pathological element or condition, called *adynamic*, ever exist *alone*, from the access of disease, throughout its course, and to its termination in death or a healthful reaction?”

We must next proceed to examine this sentence with reference to the other construction, of which it may by some be considered susceptible; that is, that, notwithstanding the occurrence also of the word “*disease*,” the meaning of Dr. Lewis is, that congestive fever is this striking pathological element or condition called *adynamia*, existing “*alone*, from the access of disease, throughout its course, and to its termination in death or a healthful reaction,” *not in conjunction*, but constituting at the same time *itself the disease*. Now we are not without reasons for supposing that this is really our author’s meaning. In the first place he has designated *no one* particular disease, existing in conjunction with which, a continued *adynamic* condition or grade of action constitutes the congestive fever; and hence it would seem to follow that any one of the long list of diseases, in which the *adynamic* element may exist, throughout its entire progress, without at any period the presence of the opposite condition, may, nay does, under such circumstances, become the *congestive fever*. But that this is not the true meaning intended, is rendered probable by the observation of Dr. Lewis, that when the term *adynamic* is used in conjunction with pneumonia, typhoid fever, yellow fever, &c., the meaning is, that “this morbid element has been super-added to the phenomena essential to these respective maladies; and they therefore of course become *adynamic pneumonia*, *adynamic typhoid fever*, *adynamic yellow fever*, &c., as the case may be, and *not congestive fever*. These, we say, are reasons for supposing it not at all improbable, that the latter construction is the correct one, that is, we mean, the one intended by the author. Yet this one, too, is equally irreconcilable with opinions expressed in the Medical History, and in other parts of the *Reply*. Thus, the purest example of *adynamia*, temporary to be sure, that could well be presented, existing *alone*, according to the general understanding in regard to the meaning of the term, as we have

before observed, we are inclined to think, is the state of syncope; yet Dr. Lewis, decidedly objects to the supposition that there is even a resemblance between this condition and congestive fever, and as a consequence would more strongly object to its being *the congestive fever itself*. Thus, in regard to the views of Dr. Parrish, (in the American Journal of Medical Sciences for April, 1845,) who, it may be remembered, seems to consider the condition called *congestion*, as a consequence merely of diminished nervous power, and nothing more or less than that state which occurs in all cases of sudden prostration, as *syncope*, for example; Dr. Lewis remarks, that "it is unnecessary to say more than that he could not very readily get more wide of the mark than he has in this;"—and although it has been our fortune to be "usually of the opposition," in the discussion of the opinions expressed by Doctor Lewis in regard to congestive fever, we fully concur with him in the correctness of this latter observation. This then forces us to the conclusion, that adynamia, or the adynamic condition *alone*, is not according to Dr. Lewis the *congestive fever*, notwithstanding the objections which have been presented in regard to the only alternative interpretation of the definition we have been discussing, arising out of opinions seemingly incompatible with it. After all this, then, still we remain in the dark as to what Dr. Lewis does "*mean by congestive fever*, as constituting in this *family of diseases*, one that should be classed as a *separate and distinct malady*." May we not hope, however, that in the future progress of our examination, something more explicit, something at least involved in less perplexity, and presented with no incompatibilities, may be developed?

But we cannot yet dismiss our text.

To the same extent, it will be seen, (though this perhaps was more than he intended,) that Dr. Lewis defines what congestive fever is, does he define what typhoid fever is; indeed, it is by distinctly settling the nature of the latter first, it would seem, that the parallel is presented, through which he arrives at the nature of the former. It is the pathological element or condition called *typhoid*, *existing alone*, from the access of disease, throughout its course, and to its termination, &c.

Dr. Lewis observes, it will be seen, after stating the fact that the typhoid element may exist in other diseases than typhoid fever, that, "notwithstanding this, patient and rigid investigation has established the fact, that there is a distinct disease, a disease *sui generis*, called typhoid fever." Now patient and rigid investigation has indeed rendered it probable that there is a disease distinct from *typhus*, which, after its discovery, or supposed discovery, was *called* by some typhoid fever, from the fact that there was in it a striking resemblance to typhus; or, that in a large proportion of cases the *striking pathological element or condition called typhoid existed*. "*Notwithstanding this, patient and rigid investigation*," however, has *also* established the fact, that the disease called *typhoid fever* may and does sometimes exist, *without the element or condition called typhoid* being necessarily present; and the name has on this account been objected to by some writers, and others adopted in its stead. In view, then, of the parallel which Dr. Lewis has drawn, and the reasoning by which he arrives at what constitutes his congestive fever in this particular instance, it as legitimately follows,

that his *congestive fever* may also exist without the condition called *congestion*, or as he just now seems to prefer calling it—*adynamia*—being necessarily present. But we have neither space nor leisure to dwell longer on, or present other phases of, the text which has for some time engaged us, and must therefore dismiss it, deviating, however, from our usual course of following our author from page to page, for a moment, for the purpose of presenting a few paragraphs in connection with it.

It is proper that the reader should here be reminded of one of the distinctions of Dr. Lewis, between a form of periodical fever described in the History, and *his congestive fever*. Speaking of the former in comparison with the latter, he says :—

“The cold stage is not attended with the same difficulty of respiration, cold clammy skin, sense of oppression, heat of the internal surface, extreme thirst, together with that marked depression of all the vital forces, that characterizes the true congestive fever; the continuance of the cold stage, and moderate febrile reaction, seeming to depend upon the *general tendency to a state of adynamia*, that so peculiarly marks every form of fever at the present day, rather than *any vital or important derangement of the functions*.”

Here then are vital and important derangements of the functions, specified as *belonging to*, and distinguishing *congestive fever* from the periodical fever described, while some of the phenomena of the periodical fever are explained and distinguished from those occurring in the *adynamic fever*, by reference to a “*general tendency to a state of adynamia*,” &c.

We quote again :—

“In view of any misrepresentations” (Dr. Lewis says) “that may hereafter be made, we wish to qualify somewhat our conclusion. Like ‘*pure typhus and pure synochus*,’ there may be comparatively but few cases of *pure periodical and pure adynamic fever*, but that they do exist cannot be doubted.”

Now this seems a distinct avowal of something. But what is this? Dr. Lewis next proceeds to remark on *intermittent congestive fever*, and on the page immediately following the one from which the above extracts are made, we find this :—

“We will not say whether this *periodical element of disease* belongs properly to *congestive fever*, or is merely mixed up with it, as in other disorders.”

In the *formal definition*, given by Dr. Lewis, of *congestive fever*,—the *pure adynamic*, it is to be presumed of course,—speaking of the commencement of the disease, instances are alluded to, in which, “at the first moment of alarm, the patient will be found in a state of great depression, attended with an *alarming perversion and derangement of all the functions*,” &c.; and on the next page of the definition it is stated, that “death takes place, not as a consequence of a series of *disordered and diseased actions* as in other affections, but by the *direct and positive agency* of that poison which makes its impression, we

know not how," &c. ; but according to all this, though producing "an alarming perversion and *derangement of all the functions*," causing death without a "series of disordered and diseased actions."

We will continue to quote from the "*definition*" of congestive fever.

"Death usually takes place between 24 and 60 hours, in some cases not before the fourth or fifth day. In many of these cases, where the disease continues from day to day, a slight reaction takes place in the afternoon ; the skin becomes partially warm ; the pulse rises so as to be perceived at the wrist ; the lungs give token of a freer action ; the tortured features relax, and the patient congratulates himself that the fearful ordeal has been passed. The improvement, though great, is but comparatively so ; the pulse is still quick, small and compressible ; the hand placed for a minute on the surface, discovers not only a want of substantial warmth, but a coldness, also great relaxation, or a want of ordinary firmness and consistence in the capillary net work ; though smiling and cheerful, there is restlessness, with an occasional sighing. A few hours more elapse, and the disease, *scattered* only for a moment, gathers again with increased terrors ; Nature's efforts, unequal to the struggle, soon yield the victory to this strong malady."

It would not, it would seem, accord with the views of Dr. Lewis, to call the above described *great* and decided quotidian amelioration of symptoms a *remission*, occurring as it does in the pure adynamic fever ; for, let it be remembered, we have quoted from the "*definition*." What then can we with propriety name it ? Let us see. "The disease *scattered* only for a moment, gathers again with increased terrors." Not a *remission* then, but a "*scatterment*."

The following observation we do not find in the *definition*, but as it occurs in the general description of the disease, immediately succeeding the *only case* given in detail, to which Dr. Lewis refers, as representing "the general condition and symptoms" of the cases which occurred in *Dallas county* in 1835, it is also presumable that he has reference in it to the congestive or *pure adynamic fever*.

"With a majority of those attacked there was in the course of 24 hours a return of warmth to the surface, and in fact a *general improvement*, showing a strong effort or disposition on the part of nature to react, but if this resolution amounted only to a *gentle glow of warmth* on the surface, the pulse continuing quick, and perspiration profuse, the ensuing morn witnessed in a more aggravated form a *return* of all the perilous symptoms, so peculiar to the disease."

"The formation of the *true* and healthful reaction, could always be distinguished from the *false* and ephemeral. In the *first* case the perspiration sensibly diminishes ; as the skin becomes dry, the capillaries fill, and a general warmth ensues ; the pulse, which a short time since may have been extinct at the wrist, is now perceived, and as it becomes raised more to the surface, it lessens in frequency and increases in force."

Now at the risk, perchance of being *laughed at by Dr. Lewis*, and charged with inability "to give to the observations and descriptions of

others, that force and effect to which they are entitled," we must honestly confess, that we are incapable of appreciating to the full extent intended, the distinctions which he has here pointed out, between the "*true and healthful reaction*," and the "*false and ephemeral*." It will be seen that in the *false and ephemeral*, in some instances, "there was in the course of 24 hours," at least, "*a gentle glow of warmth*," if not "a return of warmth to the surface and in fact a general improvement," and if it ever happens in *any kind of reaction*, where the pulse had previously been "*extinct at the wrist*," that it does *not lessen in frequency and become raised more to the surface*, it must at least be admitted, that always, of necessity, it "*increases in force*," omitting such instances of the false and ephemeral *reaction*, as Dr. Lewis may have possibly referred to, in which the pulse still continued *extinct at the wrist*. We cannot avoid the conclusion either, that even in instances of *remittent fever* or indeed any other disease, in which the *pulse has been extinct in the wrist*, on the occurrence of reaction, it will probably become increased in force,—be "*perceived*," indeed, *if not raised more to the surface and diminished in frequency*.

Again :—

"Our attention has been thus far directed to the graver and more malignant cases, about one-third of which proved fatal between the second and third days. There were those of a much lighter and milder grade, distinguished by the same phenomena, such as cool skin, continued perspiration, quick thready pulse, interrupted respiration, uneasiness and occasional sighing. These symptoms would *deepen and become more urgent in the morning, a partial reaction* taking place in the afternoon. In these, as well as those previously described, there was no marked remission or intermission; the patient continuing in a pathological condition, until a dryness or permanent warmth of skin announced a restoration of the functions of the body."

Now, call this "*partial reaction*" a "*scatterment*," or call it by any other name; still we cannot discover in what consists its weight of testimony in establishing the distinction sought, unless it is first shown, that in the *remissions* of an attack of remittent fever, the patient *is not in a pathological condition*.

We are confirmed in the opinion expressed in the Review, that the "*remarkable colluvies*," could not be considered among the "*pathognomonic symptoms*" of the congestive fever; for, though in the Reply it is re-produced in the Doctor's general description, it is omitted in the definition, no evacuations of such a character being spoken of in the latter.

Among the additional matter on the subject of congestive fever, presented in the Reply, is "some authentic information touching the early history of the disease in the Southern country," selected from several authorities. The manner in which Dr. Lewis comments upon, and explains away any sentences in the quotations given by himself, which do not exactly suit his views, is sufficiently *peculiar* and amusing, as well as the ingenious manner in which words are omitted or language modified, when necessary for his purpose. It will be remembered, that coldness of the surface, without a corresponding sensation of coldness

on the part of the patient, is a great point with Dr. Lewis in his congestive fever. In a quotation from a paper on Congestive Fever, by W. B. Johnson, these words—"he has alternate flushes of heat and chilliness,"—occur; and also these—"There is in some instances *such a degree of coldness as to produce shaking*, but most commonly he is not conscious of the coldness of his extremities, and says he is burning up." Here are the comments on, and the light in which, this is presented by Dr. Lewis:—

"Dr. Johnson says, there is in some instances such a degree of coldness as to produce shaking, *but that the patient is not conscious of coldness*. We have noticed *this shaking* or tremulousness in a few instances; *in these* it followed, and seemed to be caused by, great physical exertion on the part of the patient. We have noticed the same phenomena, under like circumstances, in *ataxic* fever*."

Dr. Johnson, however, says, "there is such a degree of *coldness as to produce shaking*," and, of course, if in his cases it had been produced by "*great physical exertion*," he would have attributed it to the proper cause and not to the coldness.

Some quotations from a paper on Congestive Fever, by Dr. G. W. Wright are introduced.

"It is very difficult" (says Dr. Wright, as quoted by Dr. Lewis,) "to describe the pathognomonic symptoms of this morbid state of the body. It is *not* the pneumonia biliosa" (an exceedingly important and interesting announcement indeed) "noticed by Dr. Cartwright, of Natchez, in which he gave from six to ten grains of Tartar Emetic every three or four hours. *It is not the ordinary bilious fever of this country, unless we admit it is the same, though of a higher and peculiar grade.*"

Says Dr. Lewis to this—

"We beg the reader to reflect one moment on the language of this observer. It is *difficult to describe the symptoms pathognomonic of this morbid condition*. *It is not the pneumonia of Dr. Cartwright; and if bilious fever, must be of a peculiar grade.*"

Now this really does seem something like an admission of the correctness of the *supposition* of Dr. Wright, that the congestive fever which he witnessed and described may possibly have been the ordinary bilious fever, "though of a higher and peculiar grade." Small-pox, however, we still believe is not considered "a peculiar grade" (the word "*higher*," it seems, did not suit the Doctor) of measles; and Dr. Lewis every where else makes the diseases radically and totally different, and has just told us that, when he was a student in the Charity

* What new fever is this the Doctor has discovered? and what are its "pathognomonic symptoms?" We have for some time been under the impression that the term *ataxic*, was now merely used to express a *condition* in fever, and not a *distinct disease*; and that it was considered a settled question, that the cases termed *ataxic* by former authors, were merely cases of the present recognized fevers, but mainly of typhus and typhoid fevers, with a more than usually marked disturbance of the nervous system.

Hospital in New Orleans, "the man who *then* should have called these cases" (meaning congestive fever) "remittent fever of any *form* or *variety*, would have been *laughed at*." These were "good old times," no doubt. Why Dr. Lewis requests attention to the words of Dr. Wright,—“it is difficult to describe the symptoms pathognomonic of this morbid state of the body,”—we cannot well conceive, nor discover the relevancy of the matter; for, in his own *allusions* to the *pathognomonic symptoms of the disease*, he hints at no such difficulty, indeed, speaks of the diagnosis as an easy matter, asserting that it is a disease "*which of all others is most peculiar and easily diagnosed*."

We think it is sufficiently evident, that, though several times we have appeared to be on the very point of discovering what Dr. Lewis does mean by congestive fever, we have not yet succeeded. It is not remittent fever "of any *form* or *variety*;" its identity with typhoid fever—though seemingly made out from some of his statements—he indignantly *repudiates*; and, when we come to search for it under the "striking pathological element or condition called *adynamic*," like an *ignis fatuus*, it is still in the distance, still eludes the grasp. Perhaps we are now about to arrive at the end of the chase.

In a quotation brought forward by Dr. Lewis, from an account of the disease as it prevailed about 1824, near Apalachicola, by Dr. Crews, we find these words:—"The disease though new to me is of frequent occurrence on the lower Mississippi, and is there called *cold plague*; but *congestive typhus* would be a more appropriate name for it." Dr. Lewis says:—"Now *mark* the language of Dr. Crews. It is called *cold plague*; but '*congestive typhus* would be a more appropriate name;' "and, (continues Dr. Lewis,) when we take into consideration the great loss and perversion of nervous power, with the recession of blood from the surface, a name more *completely comprehending the peculiar condition could not be found*."

Dr. Lewis does not claim the origination of this term, and the view in regard to the nature of the disease, which it implies; but as he fully approves and warmly sanctions it, he of course makes it his own by adoption. The past, however, should warn us not to be surprised, should we, as we proceed, find him actually, or virtually by the adoption of another, renouncing it. However this may be, should it actually be established in the progress of investigation, that the disease, called in the South congestive fever, is *really typhus*, and not a mere grade or form of *periodical fever*, we fully and entirely concur with Dr. Lewis in the appropriateness of the prefix, *congestive*, in preference to *adynamic*, and in that event, that it should be called *congestive typhus*. It does not appear, however, to bear a very striking resemblance to the congestive grade of typhus described by Dr. Armstrong, and much less an identity with it.

From a letter from Dr. R. Johnson, Dr. Lewis quotes some passages regarding the question of the identity of remittent fever and congestive fever, a portion of which we will make room for.

"It" (congestive fever, says Dr. Johnson,) "came on with cold skin, cold sweat, sickness at stomach, oppression of the lungs and great anxiety. *There might have been, in the course of the afternoon, a slight*

return of warmth to the surface, with less difficulty of breathing, and some drying up of the sweat, upon which the patient would grow animated and cheerful; but the observing medical man who had been taught a few painful lessons, would, by a *quick*, compressible pulse, feeble action of the heart, and a want of substantial natural warmth of the skin, be apprised that the strong hand of disease was 'still upon the sufferer. To be as short as possible, I regard congestive fever *then*, and congestive fever *now*, one *continued* state of peculiar diseased action from the onset to recovery or death."

Now, there are none who argue, we believe, that during the *remissions* of remittent fever, the *strong hand of disease*, may not in proportion to the violence and severity of the case, be still upon the patient; and the *opinion*, merely, of Dr. Johnson, in regard to the "*continued state of peculiar diseased action*,"—unless it could be shown, that in the remissions of remittent fever, *diseased action* was not present,—proves nothing; for his own language proves the *remittent* character of the cases of which he speaks.

Also, observes Dr. Johnson, speaking of the congestive fever on the Alabama river in 1834 and 1835.—"In those days, I never heard it suggested, that the disease had *any connection with remittent fever*; so far from it, that it was *called cold plague*."

Now, what does Dr. Lewis expect to prove by this; or what new and eccentric views in regard to the nature of congestive fever is he next going to present? Surely he does not mean to startle us anew, by the announcement that it has *any connection with plague*; and yet we think it will be admitted, that the testimony he has here adduced, goes no further to prove that it has *no connection with remittent fever*, than it does to prove that it *has a connection with the plague*.

Dr. Lewis mingles to no inconsiderable extent, in his Reply, amusement with instruction—the *dulce cum utile*; and to this end, somehow in connection with the subject of congestive fever, introduces a funny, alligator story. Perhaps, however, it will be considered, that in no part of the Reply is so large a proportion of the former element infused, as in that in which, after presenting his highly colored full-length portrait of *congestive fever*—his "pure adynamic fever," he asks, "can Dr. Boling find in the foregoing portraiture of this disease 'no symptoms pathognomonic of congestive fever?' Will he acknowledge to the world, that he can find nothing here distinguishing this disease from remittent fever?"—and then proceeds to contrast with it an abbreviated account of a paroxysm of the more simple form of the disease, as given in our paper on remittent fever; which, it will be recollected, he states, contains no account of congestive fever; carefully avoiding our account of that form of *pernicious* remittent, to which we have referred as the *congestive fever* of the South, contained in the same paper, and *stopping* his quotation when he comes upon the following lines, tending to show that even this more simple form of the disease, when from any cause permitted to become protracted, may present phenomena strikingly analogous to the more malignant cases, or to the "pure adynamic fever," differing only in degree and rapidity of progress. "In the succeeding paroxysms, the heat of the body is still about the same; but if the case be at all protracted, there is after a

certain period a tendency to coolness of the extremities, *even during the height of the exacerbation*; but, *unlike the coolness attending the early rigors, the patient is unaware of it, and is much distressed with a sensation of burning heat.* This coolness of the extremities gradually increases with each succeeding exacerbation, after it has once appeared, &c."

In answer, we may state, that a difference does, we believe, exist in the violence of the symptoms, between the simpler and more malignant cases of any disease; but, that we do not think that Dr. Lewis has yet established the distinct and separate character of congestive fever from remittent fever, much less, pointed out a difference (a proper disposition, to which we have already referred, being made of certain symptoms which he has enumerated,) between congestive fever and that which he has named, and distinctly asserted, was not congestive fever. As well might Dr. Lewis, after presenting a highly colored portrait of scarlatina maligna of his own, place beside it, from another paper, a simple case of the disease (avoiding carefully the more malignant cases presented in the latter, though asserted by himself not to be scarlatina maligna, and every point of gradation marking the transitions from the milder to the more malignant form,) and ask, *can no difference be discovered?* We dislike to deal in remarks of a vague and general character, and rather wish, that any comments we may indulge in, should proceed in a manner, as it were, as legitimate deductions from the text under consideration at the time; nor do we fear being charged, with a very wide deviation from the line thus marked out in giving utterance to the impression, that this *manœuver* scarcely deserves to be called *puerile*, and that *girlish* would be a more appropriate appellation for it. Indeed, we do believe that Dr. Lewis himself, on reflection, will come to the conclusion that it bears a striking resemblance to the occasional efforts of a boarding-school Miss, if not that it has about it a strong odor of "bread and butter."

There is a discrepancy among writers in regard to the condition of the heart's action in congestive fever, which it is somewhat difficult to explain. According to our own experience, its sounds are loud and the impulse strong; decidedly so, as compared with most other diseases, especially on the near approach of dissolution. Others, however, have reported differently, and as a matter of course, we could not say that such is never the case. In the late work of Professor Wood on the Practice of Medicine, it is stated to be the result of his experience, that the action of the heart is feeble; but Dr. Wood *resides in Philadelphia*, and his *cherished theory* of the possible formation of congestion, implies a diminished power of the heart. The discrepancy may possibly in part result from the different stages of the paroxysm, at which the notes of different observers may sometimes have been made; a *comparatively* feeble action being present during the remissions, to that existing during the exacerbations of the disease. The condition of the *heart itself* may not, perhaps, by some have been examined, and a weakened action inferred, from the state of the pulse at the wrist, the observer being governed by the supposed invariable correctness of the dogma of Professor Cook, "that the action of the heart is measured by its effect, the distension, which we call the pulsation of the arteries." It is not unreasonable, we say, to suppose, that in some instances at

least the action of the heart has been stated by observers to be feeble, merely because the pulse at the wrist was small, seeing that the above is among the doctrines taught by one of the most popular teachers of the West, who, as has been stated by Dr. Lewis in his Medical History, exercised at one time such an unbounded influence in Alabama. It would be unnecessary, we presume, at the present day to enter into arguments to prove that the *volume and force* of the pulse at the wrist, is not entirely, in all cases, strictly proportionate to the force with which the heart beats, irrespective of the state of innervation in the distal parts, though *its frequency* is, except in cases in which the impulse may occasionally fail to extend to the artery at the wrist; and we believe, only under circumstances of high local irritation, as in whitlow, for example, has it *heretofore* been observed, that in a distal part, the pulse may be disproportionately strong, as compared to the other branches of the arterial system or the heart. Dr. Lewis himself, however, speaks of the action of the heart as a mere "tremulous flutter," though in a case given in detail, in which we find the "action of the heart changed to a *tremulous flutter*," the pulse itself was probably at the same time, though "frequent, small and thready," something *more* than a mere "tremulous flutter."

In regard to our opinion, that in congestive fever the action of the heart is disproportionately strong; that "notwithstanding the small and thready state of the pulse * * * * * the action of the heart will be found strong, as indicated by the loudness of its sounds* and the force of its impulse," Dr. Lewis remarks:—"We have searched in vain for authority to sustain Dr. Boling; his assertion in this particular stands contradicted by all the medical men of the South." Now, as we have neither time nor inclination to search out new authorities to sustain our view in this particular, it happens most fortunately that it is unnecessary, as we believe the *Medical History itself* will supply us with several. We quote from a description of congestive fever, by Dr. Bates, of Dallas county, as presented in the Medical History by Dr. Lewis.

"The pulse is *small*, and more frequent than natural. In the most violent cases it is *imperceptible* except just above the elbow. The heart is felt beating tumultuously, as if struggling to free itself of a load."

But why should we call up other testimony, when Dr. Lewis himself, an unwilling witness, speaking for himself, sustains us in the most decided manner, as we expect to render now evident. Listen to what Dr. Lewis, "with all the *simplicity of nature itself*, says, and says truly."—"It is said by *some*, that the action of the heart is 'loud, strong and tumultuous,' hence they conclude there cannot be diminished nervous power. True it is 'loud and tumultuous,' and often beats violently against the thoracic wall." What other evidence could we desire of a forcible action of the heart, or in what more emphatic language could it be expressed than this? Dr. Lewis, however, being,

* Of course it will be understood, we presume, that in these remarks, the *natural* sounds of the organ only are alluded to, without reference to the morbid sounds, resulting from organic disease, &c.

as we have observed, a reluctant witness, attempts to qualify somewhat this admission, and proceeds to state :—"but it is that tremulous, irregular action (often seen from depressing poisons) where the heart painfully labors to force on the stream of blood which flows in too fast for its exhausted powers." Now, if Dr. Lewis will show us the entire compatibility of a *loud* sound and a *violent* beating of the heart against the thoracic walls, with a comparatively exhausted state of its powers ; and then the entire compatibility of a *loud* sound, &c., with a mere "*tremulous flutter* ;" and then explain in what consists the difference between a *loud* sound of the heart, when it "beats violently against the thoracic walls," "from depressing poisons," and a *loud* sound of the organ, under different circumstances ; and then how he discovers a "*tremulous irregular action*," or a mere "*tremulous flutter*" of the heart, by a "*loud and tumultuous*" action of the organ, or by its beating *violently* against the thoracic wall ; and then, how a *loud* sound and *violent* beating, implying as they are generally, we believe, supposed to do, a *forcible* action of the heart, should also be evidences of a *feeble* action ; and finally, how he discriminates between the *loud* sounds and *violent* beating, which indicate *increased* action, and the *loud* sounds and *violent* beating which indicate *enfeebled* action of the organ, it is possible that the extent and bearing of his qualification may be perceived by others.

But time presses, and we are compelled to dismiss for the present the subject of congestive fever, with the belief, however, that, though we have been unable to discover that we have done *injustice* to the views of Dr. Lewis in regard to it, it has, as we proceeded, become sufficiently manifest that it would be a task of no small magnitude, to render them *complete and entire justice*.

It yet remains to notice a few of the Doctor's offensive operations, having carried "the war into Africa," in the "*enemy's camp*."

In regard to an opinion expressed in our article on Remittent Fever, that the condition called congestion may be a consequence of "*perverted innervation or irregular distribution* of nervous influence, but certainly not diminished nervous power," &c. Dr. Lewis says,—"*this is a nice distinction truly*." Of course, comment, illustration or remark on this is unnecessary.

In the paper just referred to, we have spoken of and described briefly, several of the shades of pernicious or malignant remittent fever, and among them, that one, "known in many parts of the South as *congestive fever* ;" not by any means, however considering all the *pernicious* forms *congestive*, or using the terms synonymously. After copying our account of some pernicious cases, to which we have also applied in addition, the term *insidious*, on account of a sudden and unlooked for development of violent symptoms, in the progress of what at first seemed cases of the *mildest character*, Dr. Lewis appends a comment, in which he applies to them the term "*congestive*," as coming from us : supplying also, in addition to our description, a most striking feature to the cases himself, to wit—the vomiting of half ripe melons, including the seeds, nuts and raisin skins," and further says, "we believe we have at last discovered the Doctor's *reasons for saying that the aid of chemistry is not required in ætiological investigations*."

We really had no idea that the Doctor would find it necessary to resort to measures of this character; for, notwithstanding the partiality which paternity is so apt to engender, in glancing over the production from which the extract is made, we find even as it stands, without *alteration* or *addition*, room for critical comments, in not a few particulars; and, "bye-and-bye," should he be disposed to make a second inroad, if he will condescend to call on us for a few hints in regard to the matter, they shall be cheerfully supplied.

Another:—

"It is not only *impracticable*, but altogether *unnecessary*, to follow Dr. Boling in his comments on that portion of the Medical History devoted to the Yellow Fever of Mobile. The Doctor says that he has not seen yellow fever, and if we make his remarks on this occasion a criterion, we would say that he had never studied its literary history; at all events, he has not touched the merits of the question he affects to examine."

Dr. Lewis gives his *opinion* here merely, without the reasons upon which it is predicated. Now, without pretending to an extensive and general acquaintance with the literary history of yellow fever, we think it is apparent from the above frank admission, that Dr. Lewis does still believe that so much at least of the literary history of yellow fever as is contained in the *Medical History*, we have studied pretty thoroughly.

Dr. Lewis next proceeds to offer some strictures on our views relative to the action of quinine, calling a case to which *incidental* reference merely is made in our article on the "inflammatory affections of malarious districts,"—the "leading case." We will here present it with the comments &c., from the Reply.

"At another time, I was disposed to think that this controlling influence was only exerted in those laboring under the influence of malaria; but in the advanced stage of a case of endo-pericarditis, in which nothing in the circumstances of the patient or in the character of the accompanying fever indicated a malarious taint, I was able, by *administering night and morning* an enema containing grs. xx of quinine, to moderate the rapid progress of the disease, and to reduce the pulse from 110 to 80, and to keep it at that standard, so long as the use of the quinine was persevered in. The influence of the quinine was satisfactorily proven by the fact, that the omission of an enema was invariably followed, in the next twelve hours, by a rise of the pulse to the original standard. One of the remedies used in this case before the quinine was digitalis, which had no effect whatever in controlling the pulse. The case eventually proved fatal, in consequence of its becoming complicated with gastro-enteritis, induced by a moderate, but for the safety of the patient, too free use of calomel and tartar emetic. But to return. It would not be considered scientific to call it a specific, and yet in malarious diseases, its effects seem almost *antidotal*. In almost every case, whatever the nature of the disease, supposing the system to be at the time laboring under the influence of malaria, either

as the principal curative agent, or as an important adjuvant, the best effects may be anticipated from its administration.”*

Then follow Dr. Lewis’ comments :—

“As to Dr. Boling’s opinion in regard to the *modus operandi* of quinine, he certainly leaves us no room for doubts; for in the preceding pages, after mentioning previous doubts, he says, that “its general effect is that of a sedative, reducing and controlling the action of the heart and arteries with more certainty than any *other* remedy.” And here Dr. Lewis “*stops*,” having however interpolated the word “*other* ;” though these five little words,—“with which I am acquainted,”—would have completed the sentence. He continues :—

“In casting the mind’s eye over the *Materia Medica*, this language of the Doctor sounds *strong and strange*.”

Yet, with the slight qualification which the *completion* of the sentence, and the omission of the *interpolated* word, produce, we will now state, that in the course of several years of additional experience and observation even, we have not become *acquainted* with the properties of any remedy that has caused a change or modification of this opinion. Candor, however, requires that we should admit that there are several articles with which we have had no experience, and with the *remedial properties* of which we acknowledge ourselves *unacquainted*; among which we may mention more especially “*the unknown agent of yellow fever*,” to which Dr. Lewis calls attention in the New Orleans Medical Journal for March, 1845.

“It will also be observed” (continues Dr. Lewis) “from the quoted passage, that Dr. Boling distinctly avers, that this action of quinine is not confined to diseases of a malarious taint, but those of a strictly inflammatory character, as the case in point imports.”

We presume from this, that Dr. Lewis would say, that a malarious complication implies the impossibility of the existence of a strictly inflammatory affection; or, in other words, that inflammation, in connection with a malarious taint, is *not* inflammation. But to proceed with our quotation.—

“We would now inquire of the reader as to the sufficiency of the doubtful case reported by the Doctor, to the strong and important inference he deduces from it.”

Those who have read the paper here referred to, may remember, that in it we reported a considerable number of cases of febrile and inflammatory affections, complicated seemingly with a malarious taint of the system, giving the symptoms and treatment in detail; and inferred, from the results of the latter, that quinine exercised a controlling influence over the action of the heart and arteries. This “*leading case*,” as Dr. Lewis calls it, as is evident, is merely incidentally introduced; and

* Dr. Lewis, in quoting this, makes it read,—“the best *effects* may be anticipated from its *effects*.”

the facts of the case, as stated, we are still inclined to believe, sustain our general inference ; and so far as a single case can, does it lead also to the inference, that in inflammations not complicated with a malarious taint, quinine may exercise a control over the action of the heart. Thus, by giving, morning and evening, twenty grains of quinine, in an enema, the pulse was reduced from 110 to 80, and kept at 80, so long as the quinine was regularly continued ; but “the omission of an enema, was *invariably* followed during the next twelve hours, by a rise of the pulse to the original standard.” The “inference,” then, mentioned by Dr. Lewis, will not be considered an unreasonable one from the premises.

Dr. Lewis still continues :—

“Notwithstanding quinine *moderated* the rapid progress of the disease, still the patient died. But, says the Doctor, it was in consequence of its becoming complicated with gastro-enteritis, induced by a moderate, but, for the safety of the patient, too free use of calomel and tartar emetic.”

Dr. Lewis would lead us to suppose from this, that he had never been able to moderate the rapidity of the progress of a case towards a fatal termination, which he could not cure ; nor, with any remedy, to moderate the rapid progress of a case, which, on a change of treatment, has subsequently proved fatal. If Dr. Lewis has never seen gastro-enteritis induced by the use of calomel, but more especially by tartar emetic, or the two combined, used in the treatment of other diseases, he has certainly used them but little, or has been exceedingly fortunate. Among the cases of pneumonia which we have treated with tartar emetic principally, we do not hesitate to say that half as many deaths have occurred in consequence of gastro-enteritis—induced seemingly by the remedy,—supervening during the progress of the disease, or at the moment of apparent convalescence, as from the primary disease itself.

“So it appears after all” (proceeds Dr. Lewis) “that this reduction of the pulse may have been induced by calomel and tartar emetic, which the Doctor administered, but keeps in the *back ground* until it becomes necessary to account for the death of the patient. It is believed by some that tartar emetic will reduce the action of the heart and arteries.”

The *two facts* appear equally, distinctly and openly stated : first, that the omission of a quinine enema was *invariably* followed within the twelve hours succeeding, by a rise of the pulse to the original standard ; and second, that the case eventually proved fatal by becoming complicated with gastro-enteritis, induced by the use of calomel and tartar emetic.

Dr. Lewis does not enter into a detail of facts and circumstances to show the reasonableness of *his inference*, nor does he explain to us the process of reasoning by which he reaches it. He does not report or refer to any case in which this alternate rise and fall in the pulse to such an extent, in so short a period, was noted as a consequence of the alternate omission and administration of these remedies. He does not show that in the particular case in question, just so long as the qui-

nine was regularly continued, the pulse then being kept from 110 down to 80, tartar emetic and calomel were also given; nor does he show, that, "on the omission of an enema, which was *invariably* followed during the next twelve hours by a rise in the pulse to the original standard," the calomel and tartar emetic were also *invariably* omitted; he does not attempt to show even the probability or reasonableness of such a supposition; nay, he does not even show that the latter remedies were used at all, during the period that the patient was kept under the influence of the quinine. This indeed is one of the very unique processes of ratiocination, peculiar, entirely peculiar, so far as our observation extends, to Dr. Lewis.

"There is another case," (Dr. Lewis continues to say,) "reported by the Doctor, more in point than the great mass of those which he instances; we will admit that it is strictly inflammatory, and see how far it sustains the Doctor's conclusions."

We cannot make room for the entire case, but will present some of the chief features connected with it, bearing upon the present question.

The case was one of acute bronchitis. We visited the patient first on the 25th of December, 1843, and prescribed *tartar emetic*, to be taken in doses of one-fourth of a grain, in mucilage, every second hour; also of calomel and Dovers powder, each five grains, to be taken at bed-time. On the 27th, the tartar emetic having been continued without improvement or diminution in the frequency of the pulse, the dose was increased to half a grain every third hour. *Still, up to the evening of the 28th, the symptoms continued unabated, the pulse continuing as at first at 100.*

On the evening of the 28th, at 7 o'clock, ten grains of quinine were given, and in three hours, at 10 o'clock, the pulse had fallen to 94. At this time, six grains of quinine and two of blue mass, to be formed into pills, were ordered to be taken every second hour, and on the morning of the 29th, his pulse was found to have fallen to 86. The pills were continued in the same manner, and by evening his pulse was still further reduced, being only 78. It was ordered that the pills should be continued through the night, but through neglect of the nurse they *were not procured*, and by next morning the *pulse had risen again to 86*. The quinine* and blue mass were again resumed in the same dose, every third hour, with the addition *now* of one-fourth of a grain of tartar emetic to each dose. The pulse was soon again reduced in frequency, and under a continuation of the treatment, the patient recovered; his gums having become a "little sore" by the 4th of January.

Dr. Lewis remarks in regard to this case:—

"Dr. Boling indirectly attributes the favorable influence exerted by the treatment in the foregoing case to the quinine which was administered, and of course infers from it that this salt reduces the action of the heart and arteries, and exerts a favorable influence in the treatment of

* By a typographical error in the report of this case, on this date, 4 grains instead of 24 grains of quinine, appear in the prescription.

the true phlegmasiæ. Now as calomel and ipecac., or blue pill, nitre and tartar emetic were given, from the onset to the termination of the case, until *the gums at last became sore*, we imagine that *they* had some agency in the favorable effect produced."

We very willingly agree with Dr. Lewis that such other remedies *as we gave*, in conjunction with the quinine, probably did exercise a favorable influence in regard to the ultimate result of the case. We hoped they would at least, at the time of prescribing them. The Doctor, however, has made some *additions* to our prescriptions; and we must think, has been guilty of a gross breach of professional propriety and etiquette, in thus prescribing to our patient, without consultation and without our knowledge or consent. We prescribed *no nitre* for him, nor ipecac., except the half grain contained in the five grains of Dover's powder, which he took the first night of our attendance; and so far from tartar emetic having been given *by us*, "from the onset to the termination of the case," the report shows that it was omitted from the evening of the 28th, when the quinine was commenced, till the 30th, though prior to the administration of the quinine, it had been given freely without producing any reduction in the pulse. Thus, from the 25th to the 28th, tartar emetic is given freely, and under its action the pulse continues the same. It is now omitted, and quinine, with blue mass enough to form it into pills, is substituted, with the effect of a gradual reduction of the pulse in 24 hours, from 100 to 78. At the end of this time the quinine is accidentally omitted, and in twelve hours, we find that the pulse has increased from 78 to 86. Quinine is again resumed, this time with the addition of tartar emetic, and the patient's pulse is again reduced in frequency and kept down, his gums not becoming sore till the 4th of January, when convalescence is established.

Omitting the part of the treatment *supplied* in this case by Dr. Lewis, it would be difficult for any one, not possessed of the Doctor's own peculiar secret for arriving at conclusions, to imagine how he deduces the inference that it was the tartar emetic, &c., and not the quinine that reduced and controlled the patient's pulse. Somehow in this manner he must have reached it:—

Tartar emetic was given freely and continuously for three days, without any diminution of the frequency of the pulse, and on suspending its use and substituting quinine in its stead, an immediate reduction of the pulse took place; but the latter being omitted by accident, the pulse again rose, therefore it was the tartar emetic, &c., and not the quinine, by which the frequency of the patient's pulse was reduced.

Or in this manner:—

No reduction of the frequency of the pulse was produced during the administration of the tartar emetic without the quinine, therefore the tartar emetic did reduce the frequency of the pulse; and the pulse did diminish in frequency during the administration of the quinine without the tartar emetic, and increased again during its accidental suspension, therefore, it is unreasonable to suppose that the quinine had any agency in reducing the frequency of the pulse.

Might we not well, in view of some of the conclusions of Dr. Lewis, and the peculiar process by which only it is possible he could have ar-

rived at them, with propriety exclaim—Oh, short-sighted Francis Bacon! and, while deploring the darkness and ignorance of times past, shout in regard to our own—“*Oh, thrice fortunate and happy age,*” for which was reserved the coming of a new philosopher—not a “second Daniel,” but another Bacon,—in whose *novum organon* it is shown that *any desired conclusions* may be deduced from *any kind of premises*.

Some good things, we are inclined to think, might be said on the manner in which these two cases have been used and handled by Dr. Lewis; but were we even qualified for the task ourselves, time is not left for the effort.

We proceed with the comments of Dr. Lewis on the last case.

“The treatment of this case reminds us of an incident that occurred not long since in one of our Southern cities. A sick man was left by his medical attendant, with directions to give brandy and water freely, which was done. During this state of the case, some friends brought in a Homœopathist, who put *two drops* of liquid in a tumbler of water, and directed that a table-spoonful should be given every two hours; at the same time telling the nurse to give the patient some brandy and water every *half hour*, to *quench his thirst*. The patient rallied from his collapse, and of course the general inference was, that recovery was owing to the *little drops*. Is not this the way that Dr. Boling cures his patients? He gives the *little drops*, (quinine,) but takes good care to throw in the brandy (calomel, tartar emetic and opium) and water.”

In the Review of the Medical History, it became necessary to point out the peculiar *perversity* of this remedy, in the hands of Dr. Lewis. That, though “it never failed to augment all the inflammatory febrile symptoms,” in cases of a phlogistic character, where of course he would have wished it to act as a sedative; where the action of a stimulant might be desirable—“given in adynamic diseases, its effect is decidedly sedative.” Here it would seem, however, that something new, most extraordinary and entirely inconsistent, seemingly with either of the above implied properties, has been discovered by Dr. Lewis in regard to it, that is, that, like the “little drops” of the shrewd Homœopathist, it is *entirely inert*; for, unless this is his meaning and view of the matter at present, we feel certain he could have found no analogy whatever between the circumstances. No other conclusion can be deduced, we say, from the analogy supposed by Dr. Lewis to exist between the cases, but that his last discovery in regard to quinine is, that it is inert; *unless, indeed*, the Doctor has himself become a *convert*, and believes in the all-powerful influence of the infinitesimal nonentities of Homœopathy, and intends to give utterance in the *Reply* to the same warm partiality for the “little drops” of Samuel Hahnemann, that he had previously given in the *Medical History*, for the “hot drops” of Samuel Thompson.* One or the other horn of the dilemma

* It is a not less curious and interesting than amusing *mental phenomenon*, that, as a general rule, the same class of persons, not immediately connect-

the Doctor must ride upon ; for, unless he does, it is utterly impossible to trace out the implied analogy, and there would then be nothing in the case to “*remind*” him of the “little drops,” more than of the man in the moon, the Khan of Tartary, or the Great Mogul.

Which most to admire, we are at a loss ; the *magnanimity* evinced by Dr. Lewis in the following, or the easy rate at which he is satisfied.

“We could pursue this examination of the articles on quinine and remittent fever to the still greater disadvantage of Doctor Boling ; but as we are *satisfied* with our defence, we have no disposition to “*carry the war into Africa*.”

“He is *well paid* that is *well satisfied*.”

But we think that we have now done enough for the *Medical History*, and do hope that Dr. Lewis will let us off with this, as we are anxious in future to dispose more profitably of our leisure moments. We are really wearied with the subject, and if Dr. Lewis is as much so, —and we hope he is,—it will not be recurred to again. We will make *an effort* at least to refrain, come what will, and leave *him to himself*.

“E così quel che fece agli altri spesso,”

in one sense—if, indeed the entire Profession is not very much wronged by a censorious world ; *in another sense*—we mean as regards “*medical literature*,” it is more than probable—

Quel buon medico al fin farà à se stesso.”

II.—*Some Account of an Epidemic, Cerebro-spinal Meningitis, which prevailed in Montgomery, Ala., in the Winter and Spring of 1843.* By S. AMES, M. D., of Montgomery, Ala.

The name, *Cerebro-spinal Meningitis* is not strictly applicable to the epidemic I am about to describe. Whatever may have been the extent of the organic lesions in the milder forms, which, it will be seen, the disease frequently assumed, both the symptoms and the morbid anatomy of the malignant forms showed that the substance of the brain, if not of both the great nervous centres, was almost always involved. In some parts of France, which were visited, some years ago, by a similar epidemic, it is said, on the contrary, that the integrity of the brain was

ed with the Profession—the amateurs in physic—who, a few years past, were the avowed advocates of *steam and lobelia*, in those sections in which this *system* has been superseded by Homœopathy, are now the enthusiastic worshippers at the shrine of the “little drops.” The same meddlesome, babbling, bustling, silly old women—the same *wiselings* in breeches, who then, reeking with the foul odors of “*number six*,” were found prying about the chambers of the sick, chaunting Te-Deums to the shade of Thompson, may now, under similar circumstances, be found, shouting peans to the memory of Hahnemann.

the rule, and its alteration an exception, which, when it occurred, ought rather to be viewed as a complication than as a part of the original or primary affection.* This assertion, however, seems to be the expression of an opinion instead of a statement of fact,—the opinion depending, as I conceive, more on the peculiar views entertained by the French pathologists in regard to the post mortem evidences of pre-existing inflammation, than on just conclusions drawn from the morbid anatomy which they observed. Requiring always, for example, the presence of some product of inflammation, they reject all other evidence, giving no weight to the presence of that vascular condition which, nevertheless, is admitted to be essential to such products. The French physicians in describing the epidemic just spoken of, affirm the existence of hyperemia of the brain in, I believe, every instance in which the condition of that organ is noticed, the occasional observation of one of the products, viz: softening, of inflammation, and in all the fatal cases the occurrence of symptoms, such as abnormal sensibility and muscular motions, delirium, coma, &c., which, being observed in a disease having a constant excess of fibrine in the blood, may be considered unequivocal evidence of inflammation of the nervous substance—at least so long as there is no proof that simple meningitis can produce them—proof which would almost necessarily be to the effect that the meninges are the seat of the intellectual faculties. If, then, this excessive refinement or incredulity in the French pathological school be set aside, the pathology of the two epidemics is in strict accordance with their resemblance in symptoms, and the anatomical characters have only this difference, that one of the products of inflammation was found in the brain more frequently in the one than in the other,—a state of facts which can leave no doubt as to the identity of the two diseases. I have retained the name, therefore, which has already been applied to the French epidemic, as well as to others, in order to avoid the confusion which might grow out of a description of the same disease under another name. If it were necessary or important, however, to give it a name which would approach somewhat nearer to an expression of its true pathology, that of meningo-cerebritis and myelitis, would better answer the purpose.

MORBID ANATOMY.

The following account of the anatomical characters of the disease is drawn up from notes taken in eleven different cases, examined at periods varying from four to twenty hours after death.

Vascularity of the Membranes of the Brain.—The dura mater was the seat of abnormal vascularity in two cases, in one of which there was merely a greater number of vessels than usual displayed on its upper and lateral surface on the left side; on the other, there were red patches occupying parts on each side and along the entire course of

* *Compendium de Medicine Practique.* Tom. vi.

† I refer here more particularly to M. Tourdes (*Histoire de l'épidémie Méningite Cérébro-Spinale qui a régné à Strasbourg en 1840 et 1841*); and M. Forget (*Gazette Medicale* 1841.)

longitudinal sinus, and also that portion which covers the frontal bone just above the orbital plates of this bone; at these places the number of red patches was greatest on the right side. The arachnoid covering of the dura mater showed this kind of alteration in one case only; in this there was a very delicate pink or carmine tint diffused over it, while its usual polish was retained. The pia mater showed the usual vascularity of meningeal inflammation in every case. The vessels lying across the convolutions were uniformly red, numerous and large, being seen in several instances, many of them as much as a line, or even more, in diameter. They were also found in great numbers on the walls of all the ventricles, accompanied occasionally with spots of capilliform injection resembling ecchymosis to the naked eye. Spots of red ecchymosis were observed in different subjects on the lateral walls of the third ventricle, the anterior wall of the fourth, and in the posterior horn of the left lateral ventricle; they were also seen in several cases on the upper surface of the hemispheres and on the cerebellum.

Vascularity of the Brain.—On cutting into the nervous substance, there was found, invariably, besides the red points, as they are commonly seen in congestion, as well as inflammation of the brain, an infinite number of red vessels, sometimes containing fluid, sometimes coagulated blood. The enlarged vessels occupied different parts in different subjects. They were absent in the hemispheres in but one case; in this the grey matter was of a uniform pink color, and spots similarly colored occupied various parts of the white substance. They might be seen also after the membranes were peeled off, ramifying on the several parts at the base of the brain, and on the parts constituting the floors and walls of all the ventricles, as well as on a section of any of these parts. The grey and white matter had also a pink color, which was not dependent on the presence of vessels that were separately visible, and this, too, occupied different parts in different subjects, and was never altogether absent. The medulla oblongata was dotted interiorly with dark ecchymose spots in two cases. The cerebellum partook of these alterations to a greater or less extent in every case.

Softening of the Membranes.—This lesion occurred in circumscribed portions of the membranes in seven cases, viz: on the exterior of the brain, including the inferior surface of the cerebellum, in four cases, and in the membrane covering the floor of the lateral ventricle in three.

Softening of the Brain.—This lesion was observed in nine cases, viz: in some part of the hemispheres, chiefly in the cortical, in seven cases; in the medulla oblongata and pons varolii, in three; in the fornix and septum lucidum, in five; in the walls of the third ventricle and canal leading to the fourth, and in the infundibulum, in one case; in the corpus callosum and outer semi-circular vein of the left corpus striatum, in one case; of the optic nerves and commissure and tractus opticus, in one case; in the crus cerebri, in two cases, and in the cerebellum in one case. In the latter, the softening, which was also seen, in the grey matter of the hemispheres in the same case, was confined to two spots on the posterior and superior surface of the cerebellum on either side of the median line; at these spots, which looked under the membranes like ecchymosis, a part of the nervous matter was con-

verted into a reddish pulp, which adhered to the membranes so as to tear out with them, leaving a regular and smooth cavity with white walls, the largest about the size of the first joint of the thumb. A portion of the disorganized mass showed, under the microscope, blood, lymph, and pus globules, the latter largely predominating, with scarcely a trace of neurine.*

Thickening of the Membranes.—The arachnoid was thickened and opaque where it covers the anterior sub-arachnoidean space in three cases, and where it covers the posterior sub-arachnoidean space, in one case. This alteration could be easily demonstrated at these places, so as to leave no doubt as to the fact. There was an appearance of thickening in other places, but this was not certainly ascertained by separating the membrane from its connections.

Effusion.—There was an effusion of yellowish colored matter, showing under the microscope an abundance of pus and lymph globules, on some part of the membranes covering the exterior surfaces of the brain in ten cases. In the greater number, enough of the effused lymph was coagulated to give some cohesiveness to the morbid product, but not enough to give it the appearance of an organized membrane. Its appearance and consistence was generally that of a very loose coagulum of fibrin. It was always found under the arachnoid covering of the pia mater. In some cases it was so diffused under the membrane on the hemispheres as to look like a coating of cream, or, rather as if the membrane had a cream color; in others it was confined on the convex surface of the hemispheres, to the course of the large vessels between the convolutions. At the base of the brain this lympho-purulent deposit was largest and found most frequently about the optic commissure—indeed, at this place, either on the commissure and mammillary bodies, or between them and the tuber annulare, it was never absent. In one case, it was found only at this place, the interstices of the convolutions being occupied by a serous fluid containing a few pus globules. In several instances it projected from the arachnoid membrane into the sub-arachnoidean spaces, a line or more in thickness; and once, the anterior cavity was occupied and even distended by it. It was found also on the corpora quadragemina, the medulla oblongata, and around the third pair of nerves, where they penetrate the arachnoid membrane. The exterior surface of the cerebellum was frequently the seat of this deposit.†

* If the reader require any further evidence that this is an instance of inflammatory softening, in which Dr. Bennett (*Med. Chir. Rev.* Vol. LX., p. 460) says, the purulent infiltration has no existence, he may consult the history of the case at page —, case 11.—This kind of softening, in exact appearance at least to the naked eye, occurred in several other instances; in one the seat of it was the external posterior surface of the medulla oblongata, and in another, the anterior surface of the tuber annulare. In other cases, the softened parts were whiter than the normal brain, and in others the natural color remained unchanged.

† The facts lead to the inference that this effusion was exclusively a product of the under or inner surface of the arachnoid. The reader will have noticed that it was not found in any of the ventricles, where the membrane is said not

An effusion of fluid into the arachnoid cavity was found in nine cases. With this effusion, pus globules were mixed in uncertain proportions in every case in which they were sought. In two cases the effusion chiefly consisted of pus, mixed with blood globules. In one case, that in which the arachnoid membrane had a pink color, a small quantity of the effused fluid, having been collected and allowed to stand a short time, coagulated; this was not submitted to the microscope. In one case, the cranial portion of the arachnoid was the seat of an organized false membrane. In some places, this organized effusion was soft and but slightly adherent to the true membrane; but, in other places, and in large patches, it was quite as strong as the arachnoid at the base of the brain, and adhered with a good deal of firmness. Its color was rather a dingy pink, and from its surface everywhere pus could be scraped.

A morbid effusion was found in the lateral ventricles in four cases; in one it consisted of a cream-colored layer, semi-fluid, on a part of the left corpus striatum; in another, there was about a drachm of greenish colored pus; in another, there was serum, made turbid with flocculi; and in another, there was serum, mixed with pus and blood.

(*Fibrous Concretions.*—Concrete fibrin was found in the vessels of the brain in several cases, viz: in the internal carotids, in two; in the basilar artery, in one, [a dark coagulum of blood was found in this artery in one case, and black fluid blood in another], and in the longitudinal sinus and veins leading to it, in another; the concretion in this case occupied the entire length of the large vessel, and extended from it into the smaller vessels, from which threads of fibrin were drawn out to the length of several inches.)

Spine.—Several partial examinations of the spinal cord were made, in which the lesions, it may be said generally, were the same as those found in the brain in the same cases. The intense vascularity of the pia mater was always present, but the lympho-purulent deposit and the injection of the cord proper, were less common than in the brain. The spine was opened in its whole length in one case only. In this the dura mater had the color of muscle, which was uniformly diffused over every part of it, the internal surface being divested of its polish. It was thickened also in its whole length, but not equally; its arachnoid covering was also thickened in the parts which were separately examined after maceration. The cervical portion of the cord was softened in every part, presenting in color various shades of white, yellow and red; the filamentous portion exteriorly being reduced to a disorganised pulp, which came off with the pia mater, and adhered to the finger when touched; the color of this part, without being brilliant, was a pure and beautiful white. This alteration was found principally in the upper part of the cervical expansion of the cord; nearer the dorsal portion, where the softening was rather less, the color degenerated into a dingy yellow of several shades. The vesicular matter of this part of the cord was less altered than the fibrous—though much softened it would

to extend, nor between the convolutions, and that it was found on the inner surface of the membranous walls of the anterior and posterior sub-arachnoidean spaces, having, in more than one instance, no intimate connection with the pia mater.

bear division with the scalpel; its color, in some parts, was pinkish, and in others, a more or less dull red. Besides this general coloring, a transverse section of the firmest parts, showed a number of red and dark dots and striæ. In the dorsal division, the grey matter was more softened than the white, the softening becoming more decided in proceeding downwards. The pure white color was not observed in this division; the yellow tinge predominated, intermingled here and there with shades of dull red. The deposit of lympho-purulent matter was found chiefly about the roots of the anterior cervical nerves, being decidedly greater on the right than on the left column.*

Abdomen.—In other parts of the body no lesions were observed, which seemed to be other than accidental, or particularly worthy of note, except those found in the digestive tube and mesenteric glands. Here the alterations were so constant as, even in the few cases examined, to denote something more than a casual complication. The abdomen was opened in five cases. In all of them the mucous membrane of the stomach and the ileum, to a greater or less extent, was reddened, thickened and softened. In the stomach, this alteration was found mostly in the smaller curvature, about the cardiac orifice; the parts immediately about the pylorus were not observed to be softened, in either case. In the ileum, the examination was confined to the lower part, in extent from one and a half to four or five feet. In all but one, the membrane was most altered in that part nearest the cæcum; in the case excepted, the membrane was equally affected throughout the portion examined—about five feet, in four cases, including the latter, there were lesions of the agminated, solitary and mesenteric glands, which on account of the circumstances under which they were found, became the subject of particular attention and interest, and seem to require here a somewhat detailed notice.

Case 1. T. H., white male, aged 14 years. Attack sudden. Death on the second day. Ex. 6 hours after death. A part of the ileum next to the cæcum was taken out and washed. The mesenteric glands, near this part of the gut, as also for some distance above it, were enlarged and red, but not perceptibly softened. Its internal surface had a deep red color imparted to it by the injection of the sub-mucous cellular tissue, the vessels forming a beautiful arborescence under, and immediately around the elliptical plates, where they were largest and most numerous. On raising portions of the mucous tissue, its color was found to be rather a pale red. There were three elliptical plates in the part examined, all of which were elevated above the adjacent surface, half a line or more; their surfaces covered with prominent granules, which slightly roughened them to the touch; and the cellular tissue beneath was thickened, apparently to the extent of the elevation of the plates, and softened. The lower plate was more deeply colored, and more elevated than the one immediately above it, which was itself more altered in these respects, than the third. A great number of the solitary glands were also enlarged and elevated. For the further history of this case, see case 12.

*The reader will find the history of the symptoms of this case at page —, case 5th. In connection with the post mortem appearances, it presents some points of interest to the physiologist as well as to the pathologist.

Case 2. J. H. White, male, aged 12 years. Attack sudden: death on the fourth day. Ex. twenty hours after death. The mesenteric glands were enlarged and red. About four feet of the lower part of the ileum was taken out and washed. The mucous membrane was more or less reddened, softened and thickened, throughout this part. There were found in it eight of the agminated, and a great number of the solitary glands, in a diseased state. In all of the former, the elevated and granular aspect was very obvious, but not equally so in all. The difference, however, did not offer a regular gradation of changes from below upwards; those most altered were, as in the preceding case, nearest the colon, but next to these there were others which were less altered than those further up. Under the glands, the cellular tissue was thickened and softened. There were no symptoms of abdominal disease in either of these cases. The further history of this may be found in case 11.

Case 3. Black male, aged 3 years. Attack sudden: death on the second day. Ex. eighteen hours after death. In this case, in addition to the alteration in the intestinal glands, which corresponded very nearly with that of case 1, a great many lumbrici were found in the part taken out. The mesenteric glands were not altered. There was acute pain in the abdomen, on the first day; there was no vomiting, and the bowels were constipated.

Case 4. May, black Male, aged 17 years. Attack sudden: death on the fifth day. Ex. 6 hours after death. The mesenteric glands, to a great extent, along the ileum, were enlarged, dark red, and much softened. About five feet of the lower part of the ileum was taken out and washed. Opposed to the elliptical plates, on the external surface were patches of mottled purple, corresponding with the plates in size. The internal surface had a deep and rather dark red color, except immediately around and in the ulcerated glands, presently to be described. The deep redness was owing to the injection of the sub-mucous cellular tissue; the mucous membrane when separated being less deeply colored than the general appearance of the mucous surface indicated.

The agminated and solitary glands presented the following alterations; The inner surface of the gut, in the whole extent examined, was thickly embossed with nodules, elevated from a line and half to two lines above the adjacent membrane, and differing from each other in size and color. These nodules represented the solitary and agminated glands. The glandular structure of all the elliptical plates, and of a great number of the solitary glands, with the mucous membrane of which they form a part, was entirely destroyed by ulceration. The edges of the ulcers, formed out of deep rose-colored mucous membrane, were abrupt and irregular. The place of the glands and of the cellular tissue under them, was occupied by a dingy yellow substance, whose surface was a little depressed below the edges of the ulcers, so as to give a well-marked prominence to the rose-colored zone, from which it was separated by a line, which, I supposed, indicated the beginning of a sloughing process. The consistence of this deposit was nearly that of crude tubercle, and, like that, it was friable. Its surface was smooth, or, at least, presented no very marked irregularities any where but in the largest gland, nearest the cæcum, where it was irregularly grooved or

pitted. It rested on the muscular coat, which remained unaltered—no trace of the normal cellular tissue could be found in it. The same substance occupied the place of the sub-mucous cellular tissue under the solitary glands. There was no apparent diminution of disease in proceeding upwards to the extremity of the section examined—a fact which renders it probable that the whole, or, at least, the greater part of the ileum was involved in it.* This patient had neither vomiting, nausea, nor diarrhœa. He took a dose of castor oil on the first day of the attack, which acted very gently; on the second and third days his bowels were constipated; on the third day he had pain in the abdomen, with tenderness on pressure, and some tympanitic distension, to relieve which, he took some cathartic medicine, which, operating in the evening of the same day, gave entire and permanent relief to the pain, distension and tenderness. With the exception of these symptoms, lasting but a few hours on the third day, the very formidable disease of the abdomen was effectually masked by the brain disease.

Blood.—The blood taken from the arm and, by cups, from the back of the neck, presented some physical characters which, considering the nature of the disease, were peculiar, and sufficiently uniform to assist in the diagnosis of some equivocal cases. It coagulated with great rapidity, so much so, that I was foiled in several attempts to ascertain its density by the hydrometer, forming a large and loose coagulum in which all the corpuscles were rarely included. The serum separated from it very slowly, and in small quantity. Its color was generally bright—in a few cases, nearly approaching to that of arterial blood; it was seldom buffed: in thirty seven cases in which its appearance was noted, it was buffed in only four. The blood, an analysis of which is given below, was taken early in the disease from the arm, and was the first bleeding in each case. The first was from a laboring man, thirty-five years old; the second from a boy of twelve years old, while comatose, and the two others from stout women, between thirty and thirty-five.

1.		3.	
Sp. gr. of defibrinated blood	1053,50	Sp. gr. defibrinated blood	1046,50
" Serum	1026	" Serum	1024
Solid contents	767,87	Water	796,07
Fibrin	6,40	Solid contents	203,93
Corpuscles	140,29	Fibrin	3,64
Solid residue of serum	85,31	Corpuscles	123,45
2.		4.	
Sp. gr. of defibrinated blood	1046	Water	787,37
" Serum	1022	Solid contents	212,63
Water	813,83	Fibrin	4,56
Solid contents	186,17	Corpuscles	129,50
Fibrin	5,20	Residue of serum	78,57
Corpuscles	112,79		
Solid residue of serum	68,10		

* All the post mortem examinations, that I made or witnessed, were more or less incomplete, for want of time. The usual course was to take away the part or parts which were objects of particular interest, and inspect them af-

SYMPTOMS.

The analysis of symptoms which follows is made from notes taken in sixty-four cases; this number, consequently, is to be taken as the standard of comparison where the proportion is not otherwise stated.

Mode of Access.—Premonitory symptoms occurred in forty-three cases. In all of these, pain in some part of the head was a prominent and constant symptom. The cephalalgia was not so acute in this stage, as after the attack was formed, except in those cases of the latter where the premonitory symptoms were followed by a comatose state, in which the sensibility was blunted by oppression of the brain. It was generally continuous, but occasionally it was observed to be remittent or intermittent, with or without regular periods. Its seat was commonly in the forehead, temples, superciliary ridges of one or both sides, or the occiput; sometimes it occupied the whole head, being in such cases most acute under the frontal or occipital bones. In a few instances among persons who had been subject to attacks of sick headache, the premonition assumed this form, and to such cases, nausea and vomiting, as a premonitory symptom, was strictly confined. In the greater number, the headache was the only warning given, but sometimes there were added some pain along the course of the spinal column, particularly in the neck; soreness in the muscles or joints; facial neuralgia; and, very rarely, giddiness, with or without dimness of vision. There was but little impairment of the strength, and but little complaint of a general feeling of malaise; persons seldom left their employment, whatever that might be, before the attack was fairly begun. As a general rule, the appetite was but little affected, though it was occasionally lost, or a great deal diminished. In a few cases, the disease was developed on a relapse from intermittent fever, showing itself in the cold stage of the second paroxysm. The length of the premonitory stage was very various, nor did the time of its continuance afford any indication of the severity of the approaching attack; the most malignant sometimes giving several days' warning, and at others, making the attack without even a momentary precursory symptom. The premonition lasted less than twelve hours in eleven cases; twenty-four hours, in eight; two days, in five; three days, in six; four days, in four; five days, in three; six days, in one; seven days, in three; and over seven, in two. The hour at which the full development of the attack began, I find noted in twenty-seven. Of this number, nineteen began between 12 M. and 12 P. M., including the former and excluding the latter hour.

Mode of Attack.—The disease was ushered in by some degree of chilliness, never amounting to rigors, in twenty-eight, of which eighteen had had premonitory symptoms. The chill was generally short, being followed in a few minutes, but sometimes not for half an hour, either by a decided febrile reaction; a state of stupor more or less profound, with a cool skin and feeble pulse; or, this condition of the skin and pulse,

terwards, as time permitted. In this way the examinations of the interior of the intestines were made, so that any questions of new interest arising during the examination which would have led, if practicable, to further inquiry, had to be overlooked or left to conjecture.

without stupor. When there was no chill, the development was sometimes manifested by the sudden advent of a comatose or apoplectic state—a degree of stupor not amounting to coma, accompanied by a feeling of excessive debility, giddiness, dimness of sight, or double vision; or, by a sudden aggravation of the premonitory cephalalgia, accompanied by various changes in the condition of the pulse, skin, &c.; or, as in a few instances, the premonitory headache increased gradually day by day, with but little change in the skin or pulse, until at last the patient was compelled to take his bed.

State of the Skin.—This was noted in fifty-seven cases: of these, the skin was *dry* on the first day in forty-two: of these, it had the natural temperature, in fourteen, was below the natural temperature in seventeen, and above it in eleven; it was *moist*, on the first day, in fifteen; of which the temperature was natural in four, below natural in three, and above natural in eight. The dryness, which it is seen was so common, even with a low temperature, sometimes predominated throughout the attack, when it ran its course quickly. In these cases, it proved to be very unyielding to remedies, particularly so, to losses of blood. I have noted, in several instances, that patients were bled to the verge of syncope, and this repeated within an hour or two, without affecting it. Death took place in more than one instance with a dry skin in the moribund state. Generally, however, in the progress of the attack, the state of the skin varied a good deal in regard to dryness and moisture, being dry or moist at different periods, on the same day, or on different days, without any regularity, or any apparent dependence on the action of the heart. The temperature was not usually so changeable; very frequently there was no change in this respect during several days, and but very rarely was any observed on the same day; this kind of change, too, when it did occur, seemed to follow certain changes in the pulse, from a weaker to a more active state. There was never any pungent heat in the skin; in the greater number in which it was above the natural temperature, it was still below ordinary febrile heat; nor was the head ever warmer than the trunk, though the extremities might be cool when the head and trunk were warm, but this was not very common. When, however, the skin was elsewhere cool, it was always cooler or positively cold on the feet and hands. In some instances the skin was abnormally sensitive to the touch, particularly on the extremities; the scalp was sore in one case only. An eruption around the mouth (*herpes labialis*) was observed in three cases. Petechial purple spots, which did not disappear on pressure, in one case. In some the face was flushed and the skin clear; in others, the face was pale and the skin sallow; the lips were always deeply colored. In one case, the skin had a peculiar shining or glistening appearance, at the beginning of the attack. These observations concerning the color and petechiæ, of course refer only to white persons, which, it will be seen, formed a small proportion of the whole number.

Pulse.—It may be said generally, in regard to the pulse, that it was slower, smaller and softer on the first day, than in health; and very irregular in the force of its beats, in the time between them and in their number to the minute, in the same, as well as in different subjects. To this general character, however, there were many exceptions, appertaining to

certain varieties or forms of the disease, which will be pointed out more particularly in another place. The softness of the pulse was its most constant feature. I find it noted *corded*, or hard, in but one case; and those instances in which it rose above the natural fulness were very rare. A pulse slower than that of health, was less common than either of the preceding. In general, it was slower on the first day than afterwards; in the greater number it was below 76—in many below 60, in others below 50, and in several below 40. The same remark is applicable to the irregularity of the pulse. Its irregularity in the number of beats to the minute, was remarkable and characteristic. It is thus noted in one patient on the first day. 10 o'clock A. M., 76; 12 M., 96; 5½ P. M., 68; 9½ P. M., 96; 9¾ P. M., 76; and so varying on the second day between 120 and 150. On another occasion, the pulse on the first day, during several hours, was continually varying its number to some figure between 30 and 76. There was one remarkable exception to this kind of irregularity, in which the pulse stood steadily at 100 throughout the attack, which lasted three days, being counted frequently, and the last time but half an hour before death. These characters of the pulse—slowness, softness and irregularity, were not confined to the more violent or malignant varieties of the disease; but were observed in some of the very mildest attacks, in which there was no other evidence of serious organic lesion—and, on the other hand, the smallness and feebleness were but little marked in the beginning and through the progress of some of the most malignant cases.

Very few notes, I find, were made concerning the action of the heart; so far as its sounds were observed, they were never less feeble than in health, nor in other respects offered any peculiarity.

Tongue and Mouth.—The tongue underwent two kinds of changes from its natural state, one or the other of which, with a single exception, was always seen at the beginning; and, (if there were no considerable complication,) through the whole course of the attack, even if greatly protracted. They were exhibited separately, in one of the two varieties of the disease, and will be referred to hereafter, in describing these varieties.

The first kind that I shall refer to, occurred most frequently in one of the more grave or malignant forms of the disease. In this kind, the tongue was broad and flabby, filling up the mouth sometimes, so as to impede articulation, and press against the teeth, which formed by the pressure indentations on its edges laterally as well as anteriorly. In one case, a portion the tongue, by its enlargement, was protruded constantly beyond the front teeth, and in a few others the pressure produced on the thick edges anteriorly, a line or raphé, resembling that on the gums of an infant. A short time before death, the thickening and elongation were apt to increase. The coating which was usually of a pale ash color, or white, but sometimes yellowish, very often covered the whole of the free surface of the tongue; always the whole of the dorsum, and part at least of the edges, being thinner at the tip and sides, and thickening backwards and towards the centre. With this state of the tongue, there was always an abundance of saliva, which being generally more viscid than in health, had sometimes the consistence of thick mucilage; when the coat began to give way, its edges became abrupt, and the clean edges

of the tongue assumed the bright pink color belonging to the alteration next to be described.

In the other kind of alteration which I shall designate the *second*, the tongue was less obviously enlarged; being generally broad, however, though not often thickened nor indented by the teeth. The coating, which was always present, had the color of the first, but it never occupied more than three-fourths, sometimes not more than one-half of the dorsum; beginning thin before, as if it were coloring matter incorporated with the epithelium, and generally thickening backwards. The parts uncovered, particularly the end and sides of the anterior two-thirds, had a bright pink color, which was very peculiar and striking. As the disease advanced towards a fatal termination, it was not uncommon for this condition to assume that of the other, in all its characteristics. In neither of these conditions was the tongue ever dry on the first day. It became dry after the first day, in five cases, viz: on the second, third, fifth, sixth and seventh day.

Cephalalgia.—This proved to be a very important diagnostic symptom. It was absent throughout the attack in but one case in which there was consciousness enough to answer a question. Those persons who had been subject to spasmodic or neuralgic headache, uniformly spoke of the pain from this disease as different from any they had ever experienced before. Some described it as consisting of a continued painful roaring; others as a fulness and tightness, as if a tight band was drawn round the head; or, as if the forehead, or crown, or sides were being crushed inwards. A greater number described it as darting and throbbing; frequent shooting or darting through the head from behind forwards, and from side to side, and of such violence as to cause the patient to cry out. It occupied in some the temples, superciliary ridges, and frontal region; in others, the back of the head only; but in the larger number, it was felt in the whole head; being more violent in some one of the places just mentioned, or under the parietal bones.

The intensity of the headache was by no means proportioned to the severity of the other symptoms; in some of the worst cases the pain was but little complained of, being obtunded, as it were, by the cerebral oppression; but it was sometimes slight also, in cases of equal malignity, when there was no want of intelligence and sensibility; and, on the other hand, it was at times intense and almost insupportable when the disease assumed its mildest form. It was continuous with the attack in but few instances, disappearing towards the close of the first, or on the second, third, or fourth day, to return generally after an uncertain interval. It was generally aggravated by an erect or semi-erect posture, though in two instances, such a position relieved it very much whenever it was assumed. In a number of cases the cephalalgia was aggravated by pressure on some part of the spinal column, chiefly the cervical—a pain was said by the patient to dart from the point pressed on through the head to the eyes and temples. The pain so produced was spoken of as very acute; on one occasion, the patient, a young lady, begged me with the most earnest and moving entreaty, to desist from a very slight pressure on the fourth cervical vertebra, and to refrain from repeating it; she complained of the intense agony produced by it during some minutes—the case proved fatal.

Giddiness.—This was rarely complained of after the first few hours of the attack. It occurred in but a few cases, and was never a prominent nor an important symptom.

State of the Senses.—The conjunctiva was generally injected, and the eyes glittering and watery; in a few cases, principally among children, the conjunctiva had a pinkish tint, when there were no distinct vessels to be seen. The pupils were dilated in seven out of forty-one cases in which their condition was noted; in the rest, they were either contracted or natural; they were insensible to light from coma, in three cases, and insensible without loss of consciousness (amaurotic), in two cases, in both of which perfect vision was restored in a few hours, and both of whom died apoplectic. Double vision occurred in six cases; painful sensibility to light occurred also in six cases; in a number of others, there was enough of abnormal sensibility to make the ordinary light disagreeable, but in far the greater number, the strongest light was not complained of.

In several cases, there was a profuse discharge from the nostrils of thick mucus, alone or mixed with blood. In one patient, the sense of smell was lost in one nostril. Spontaneous deafness was noted in a few patients, in one of whom there were three exacerbations of it on the second day, each lasting about three hours, with intermissions lasting about the same time.

State of the Mind.—Some degree of stupor, amounting in most instances to profound coma, was present on the first day, in eighteen cases; and there were a few others, in which it appeared on the second or third day. In the greater number this symptom passed off within twenty-four hours, but in others not for two or even three days, generally to return at some subsequent period, varying from twelve hours to several days; sometimes it did not return at all, though the disease was protracted to many days—there were cases again in which it did not remit, but these were such only as proved speedily fatal. Delirium, with or without stupor, occurred at some period of the attack in twenty-four cases; unlike the stupor, it was never continuous, sometimes lasting but three or four hours, and rarely ever more than twenty-four. It was wild and furious in two cases only, and playful in one; in a few it was melancholy and pathetic, the voice being subdued and the tone and language touching. The mind was generally desponding and apprehensive, so much so, that, even in the delirium, fears were not unfrequently expressed in regard to the result of the attack. There was, in all the more violent cases, forgetfulness of the events which transpired during the attack, not only after it was ended, but frequently from day to day while it continued; and this, whether delirium or stupor was present or not.

State of the Muscles.—The posture commonly assumed was on the back, with the lower extremities extended. There was never any disposition to assume the fatal position, or a near approach to it. In two patients, both comatose, a position was taken on the right side, and maintained with great pertinacity; in another, in which there was continued opisthotonos, the position taken and kept during the greater part of several days, was on the chest and abdomen, the head being

kept elevated, or if resting on the arm or pillow, was still thrown considerably backwards; in other cases, when the head was thus drawn back, the decubitus was on the side. Tonic contractions of the extensors of the back occurred in three other cases, in each of which a similar action in the extensors of the head was also observed; but the latter was observed also in a number of patients, in whom the extensors of the back were not affected. Besides these cases of tetanic affections of the muscles of the neck, the latter were affected with stiffness and soreness in thirty-five cases. In a few of them, the affection was confined to the sterno-mastoid muscles of one or both sides; but in the others the extensors were principally concerned: the head in these cases, instead of being drawn back, as in the more active contractions, was steadily kept in the natural position (of the head) when the body is erect, either involuntarily or voluntarily as the easiest position.

Quivering of the muscles of the face, tremors of the hands, and embarrassment in the movements of the arms; tonic contraction of the flexors of the fore-arm, and of the recti muscles of the abdomen were, one or more of them, observed in several cases; subsultus tendinum, picking at the bed clothes, and reaching after imaginary objects, with other typhoid symptoms, occurred in three protracted cases. Strabismus occurred in nine cases; convulsions coming on towards the close of the attack, in three cases; and at the beginning of the attack, in one case.*

An incomplete paralysis of the right eye-lid occurred in one case, and of one entire side of the body, in another—one of the mildest kind, in which the symptoms previously indicated there was no serious affection of the brain or spinal cord. In a few cases, there was a general soreness of the muscles, which gave rise to a great deal of pain in moving.

Spine.—Pain in the cervical division of the spinal cord occurred in twenty-three cases; in the dorsal, in eleven; and in the lumbar, in six. In all but two, the pain was slight, many making no mention of it, until they were questioned, and others complaining of it less than patients generally do in remittent fever. A more remarkable feature in the symptoms of spinal disease I have already had occasion to refer to, viz: the effect of pressure on the cervical and dorsal vertebræ. This, when applied to the cervical part of the column, produced pain in the head, frequently darting from the spot pressed on to the forehead, eyes and temples, in twenty cases. In two, during the existence of deep coma, pressure on the upper dorsal vertebræ occasioned great

* Dr. Abercrombie (Path. and Practical researches, &c.) seems to consider that meningitis most commonly attacks by general convulsions. Dr. Watson reiterates this opinion. Both, however, were treating of sporadic meningitis. The observations of others, of the French physicians particularly, are more in accordance with the fact stated in the text. For example, in Andral's cases, if I remember aright, convulsions are not mentioned among the symptoms at all. Dr. Abercrombie's cases are themselves far from satisfactory in proof of the general conclusion, for I find that the ages of his patients, in whom this symptom occurred, were, with one exception—a girl of fourteen—under twelve years, and a great majority of them under eight; proving, if anything, only that the fact may be true in regard to children, although my own experience, neither in the sporadic disease, any more than in the epidemic, tends at all to confirm even the latter conclusion.

restlessness and apparent distress. Pressure on the cervical vertebræ produced, also, pain at the top of the sternum—on the dorsal, pain in the middle of the sternum, at the epigastrium, or about the umbilicus, as it was made higher or lower. Sometimes pain was not experienced in the part pressed on at all, but only at some one of the places mentioned above. The pain so produced was often violent, and not always transient continuing sometimes for several minutes. In one case, the patient, a black woman, cried out from a slight pressure on one of the upper dorsal vertebræ, saying “you hurt me all over,” and she recurred to the circumstance more than once afterwards, with much bitterness of complaint.

Stomach and Bowels.—Vomiting occurred in fifteen cases—at the beginning of the attack in two—at a later period on the first day, in eight; not till the second day, in three, and not till the third day, in two. There was nausea, without vomiting, in three cases; one on the first, and the two others on the third and fourth days.

The bowels were either natural or constipated in every case but one, in which diarrhœa occurred on the sixth day, probably from the use of tartar emetic for a pneumonic inflammation. There was acute pain in the abdomen in three cases, and tympanitic distension, with tenderness on pressure, in one, occurring on the third day.

Appetite—Thirst.—A desire for food, more or less urgent, was expressed in eight cases, at such times as obviously pointed it out as a symptom of disease, and not as a sign of convalescence, or of its near approach. The desire was evinced in some on the first or second day; in others, protracted cases, on the eighth or ninth. It was rather urgent in two or three patients, and in one continued to be so for several days.

Respiration.—This I find noticed in twenty-five cases. The most remarkable feature concerning it, was the absence of stertor, which occurred in only two; another feature worthy of notice, was the number of respirations to the minute—in five only it was below twenty-one, in two of these it was twenty, and in one fell as low as eight; in five, it was between twenty-six and thirty; in eight, between thirty and forty; in five, between forty and fifty; and in four, from fifty-two to fifty-six, all of them being counted on the first or second days, and before the breathing had become affected by the approach of death—Generally the breathing was regular. I noticed it as suspicious in one instance.

Physiognomy.—The expression of the countenance was exceedingly various, and variable, affording little or nothing which was available in diagnosis, or prognosis. It was frequently natural even under the influence of the most fatal symptoms; in not more than two instances, and in these after the disease had been long protracted, was there any thing of idiocy in the expression? When it was neither natural nor idiotic, the various shades presented may be expressed by the words, wild, anxious or distressed, melancholy, heavy, and lastly, as was rarely the case, however, brilliant and pleasing; and several of these varieties of expression might be observed in the same patient at different periods during the attack, and occasionally at different times on the same day.

Emaciation.—Extreme emaciation occurred in but a small proportion of even the protracted cases. In far the greater part of the whole number there was none perceptible. In one, however, it took place

suddenly, in a manner which could not be accounted for by the extent of the evacuations, for they were inconsiderable, on the accession of an acute and very formidable gastro-enteritis,—induced probably, by tartar emetic,—which appeared on the sixth day.

Duration.—This disease seemed but little disposed to observe the usual critical days of the endemic diseases of this locality, which are the fifth, seventh, and ninth,—particularly the ninth. The extremes of duration were fifteen hours, and fifty days. Between these periods inclusively, there were three which terminated on the fifth day, none on the seventh, and three on the ninth, while there were sixteen which terminated on the second and third, and nineteen on the fourth. Besides these there were two which proved fatal within the first twenty-four hours, and two which lasted over forty days; one terminated on the fourteenth, and nine between the fourteenth and thirtieth—making fifty-five cases to which the notes have reference.

FORMS.

The disease assumed a great variety in its aspect, and a distinctness in the varieties, which was very remarkable. I do not refer merely to shades of violence, nor to modifications occasioned by serious complications, in which the disease might still preserve its identity by the symptoms alone; but to more palpable differences seemingly dependent on other causes, connected perhaps essentially, with the epidemic constitution of the atmosphere; and of such kinds, as to very much embarrass the diagnosis for some time after the epidemic first made its appearance. Various as the forms were, however, their distinctness of outline renders them capable of being easily arranged under two principal heads, each of which will admit of being advantageously sub-divided—an arrangement which appears to me to be of practical importance, and necessary indeed, in order to present this part of the subject in a tolerably clear light.

Taking the character of the symptoms alone for the foundation of the principal division, one is naturally led to apply the term *congestive*, to one class of cases, and that of *inflammatory* to another; under which the whole number may be included,—the former comprising a little more than one half. The distinguishing features of these classes, if I may so call them, which are very accurately represented by their names, were always well marked at the beginning, and generally also through the course of the attack, even when most protracted. The exceptions to this latter remark were found in but a small number, in some of which, for example, the disease, inflammatory in its aspect at the beginning, assumed after a first remission, a stage of congestion more or less decided in its symptoms; or, as in others, the first stage of congestion, after an uncertain time, gave way before reaction, which might continue to characterize the attack in its further progress; or, might be succeeded after another uncertain interval, by a more or less obvious return to the congestive state. The latter, however, can hardly be called exceptions with strict propriety, for the stage of reaction in them, no matter how violent it might prove to be afterwards, was at first a state of improvement—a remission in fact, differing from other remissions presently to be noticed, only when the secondary state proved to be permanent. No instance occurred of a contrary change.

With these few exceptions then, the stamp put upon the character of the attack, as concerns these classes, was neither effaced, nor indeed hardly observed in its progress or march.

It is next to be observed that among the individuals of each class there were many well defined shades of difference which require to be fully set forth in a true picture of the epidemic, and which will afford the foundation for the further division or sub-division spoken of. These differences were of two kinds: The first depending chiefly on different degrees of violence, may be represented in three groups, the general features of which may be properly expressed by the terms *malignant*, *grave* and *mild*; the middle term being applicable to the inflammatory class only! The second kind occurring among cases of equal violence, cannot be expressed in general terms; but may be represented to some extent in a selection of cases from the several groups. It will be found that this sub-division is in point of fact much less arbitrary than the terms would generally imply. The several varieties were not disposed to run into each other by imperceptible shades, so as ever to leave a doubt as to the place to which certain cases might belong,—on the contrary, they preserved a distinctness and uniformity of outline, which could not have been anticipated from different shades of violence merely, in the same epidemic disease, nor were the individuals of one group disposed to change abruptly to that of another. I have not been able to find, neither among my own notes, nor among those furnished me by other physicians, more than a single instance in which an individual of one of the sub-divisions, or, the *grave*, assumed in its progress, the symptoms of another, viz, the *malignant*. Thus, the first impress of the power of the disease giving rise to this sub-division was not less a lasting one, than was that, on which the higher division was founded; and whatever deviations from it might have been observed, were comprised within limits which fairly justify, as an expression of uniform and stable differences, the application of the comparative terms I have employed to designate them.

I now proceed to consider the two classes and their sub-varieties separately.

1st. *Of the congestive class.*—As I have already intimated, there were but two divisions of this class; viz,—the *malignant*, and the *mild*. Their affinity to each other was manifested by such symptoms as distinguished this from the inflammatory class, viz,—the constant presence of a cool skin, a small and feeble pulse, and a characteristic tongue, which corresponded in appearance with that first described at page —. The traits which distinguished them from each other were not less obvious and uniform.

2d. *The malignant*, was distinguished by the frequent occurrence of chilliness in the forming stage; a great diminution or prostration of strength; the invariable occurrence, at some period of the attack, for the most part on the first day, of coma, or at least a great degree of stupor, which was more common than delirium;—of delirium, or of both; embarrassment in the movements of other muscles than those of the neck, and the stronger contraction of some of the former, drawing the occiput downwards; a wider range in the frequency of the pulse, and its more changeable character otherwise; the greater quantity and

viscosity of the saliva, and thicker coating of the tongue; and by an occasional tendency to assume the symptoms of the inflammatory.

Case 1. Moody, white, aged 20, Printer, was taken with a slight chill on the 13th of March 1848, at 5½ P. M., and had some fever during the night, and rested badly. On the next day, at 11 A. M. he was seen by Dr. Sims, then had the following symptoms: comatose, skin cool and dry all over; face pale; hands and feet cold; pulse 76 to 80, small, very feeble and irregular; resp 32; pupils natural, and move readily to the light; conjunctiva injected; lies with his mouth open when quiet, but is very restless; flexure of the hand on the fore arm, and of the latter on the arm; great sensitiveness of the skin of the arms to the touch or pressure; pressure on the cervical vertebra caused great distress, and increased restlessness, during which he made strenuous efforts to get out of bed, turning his eyes wildly; it was now ascertained that he was amaurotic; does not speak. He remained pretty much in this condition during the day, his pulse continuing feeble, but variable, ranging from 62 to 96. Towards evening his skin had nearly the normal temperature, and had become moist, but again became cool, and his feet very cold before 9 P. M., about which time he had a large evacuation of urine. On the 17th, his bowels had been opened freely by Croton Oil, and a blister which had been applied to spine had filled well; his pulse had become very frequent, 138 to the minute, with rather more feebleness and diminution of volume; the amaurosis had disappeared with the tonic contraction of the muscles of the arm; during the day he was restless and disposed to get out of bed, which he did frequently; then he would go to the fire and sit awhile, then to the pail and drink water, return to the bed, respread the covering with some care, and lie down; while this was going on he carried his head bent backwards with the chin elevated; did not speak, but groaned a great deal in a way that indicated a general uneasiness and distress, rather than pain. At night it was evident that he was failing fast, he continued restless, but now kept his bed; his eyes had become more injected; his pulse still quicker and more feeble; and the expression of his countenance, which had before been rather stupid, was wild; when importuned to speak he merely ejaculated, "Oh Lord!" He died without convulsions about 9 A. M.

Case 2. Black, age 22, called at the office of Dr. Jones early on the morning of the 8th of March. He had been treated during the past summer for a quotidian intermittent cephalalgia, which proved to be very obstinate, but yielded at last to a salivation. He now complained of a similar intermittent pain which came on early in the morning, increased in severity until 12 M., and then gradually declined until night, when it went off. The paroxysm for which he consulted Dr. J. was the third,—along with it he had tenderness of the cervical vertebræ. He was directed to have a blister put upon the spine over the cervical and upper dorsal vertebræ, and to take half a grain of valerinate of zinc, with extract of hyoscyamus, every four hours. On the 9th, I saw him in the absence of Dr. Jones, at 8 A. M., he was then very stupid, but could be roused to answer questions in monosyllables without much difficulty; he was lying on his right side, and would not keep any other position; his skin was moist and cool on the head and body, and cold

and dry on his feet and hands; countenance dull, as if from sleepiness rather than coma; pulse 84, small, feeble, and irregular; conjunctiva not injected; pupils natural; when questioned said he had pain in his head. At 9 A. M. Dr. Jones took charge of him, and from him I have obtained the following additional history. At this time his pulse had increased in frequency, without any other change in it, and he had become more stupid. He took a cathartic, and had mustard cataplasms to his extremities. At 12 M. his bowels had been acted on twice; his skin was becoming dry, without getting warmer, and the pulse was even more feeble. At 4 P. M. his pulse had risen to 112, and having a little increased fullness, he was bled from the arm to thirty-two ounces, after which the pulse became again exceedingly weak and rose in number to 125. He was then directed to have six grains of ferrocyanide of quinine with Sp. lavend. comp. every four hours. At 9 A. M. his pulse was 160, his skin moist, and the coma profound.

On the 10th, at 8 A. M., he had taken twenty-four grains of Quinine; pulse 132, other symptoms not at all improved. At 11 A. M. he was taken with convulsions in paroxysms which lasted about a minute, and recurred at intervals of about ten minutes—these continuing, he died about 3 P. M.

Case 3. Dennis, black, aged about 30, Ferryman, was taken with a bad cold and cough on the 21st of February; along with the cough he had also a sore throat, and some stiffness and soreness on motion in his neck; he continued at his work until the 24th at 4 o'clock, when he took a chill and came home to bed; rested badly during the night; had great thirst, and complained of slight pain in the head, and severe pain in all his joints. On the 26th, at 10 A. M., he was in the following condition—the history of the case from this time being taken from the notes of Dr. McLester:—Free from delirium and coma, eyes injected; pupils, skin, and respiration natural; tongue covered with a thick whitish coat; nausea, and occasional vomiting of a greenish water; bowels open; pulse 62, small and irregular; urine highly color'd; has severe pain in the region of the frontal sinuses, which is increased by light pressure over them; pain also in the occiput and back of the neck, but not violent; some increase of pain on pressing the cervical spine; soreness in the muscles of the neck; movements of the head and arms embarrassed; discharges from his nose with difficulty, a thick bloody muco-purulent matter. He was ordered to take sulphate of Quinine, morphine and ext. of hyoscyamus every four hours, and to have a sinapism to the neck, which at 8 P. M. had relieved the pain in the head and neck to a great degree. On the 27th at 8 A. M., the pain in the head, which was relieved during the night, had returned; the movements of the head were more embarrassed; and those of the arms were tremulous; in other respects the symptoms were the same. In the evening he had some difficulty in deglutition, and in getting rid of the matter in his nose; in the course of the night he became delirious for the first time, and continued restless until the morning of the 28th, when he had become perfectly rational, and was almost free from pain in the head, the little remaining in the region of the frontal sinuses. From this time his condition changed but little until late in the afternoon of the 29th—he was kept during this time quiet, with an occa-

sional dose of morphine, and had calomel in doses of five grains given to produce ptyalism; in the afternoon of the 29th, he was evidently sinking, and at 7 P. M. he died quietly, having had no delirium, and conversing sensibly of his approaching death almost to the last moment.

Case 4. Rose, black female, aged 20, having complained for a short time of pain in the head, neck, and shoulders, was observed to totter and show an inclination to fall backwards while at work in the field on the 18th of April. Soon afterwards she got separated from the other hands, and was found lying on the ground insensible,—this was about 12 M. At 5 P. M. when she was seen by Dr. Holt she was in a profound coma, pulse 72, small, and very compressible; skin cool generally, and cold on the extremities; eyes natural; *muscles of the extremities cataleptic*. She was directed to take a drachm of calomel, and to have a blister on the spine to cover the whole of the cervical and dorsal vertebræ. On the 19th, at 9 A. M. the symptoms had not changed in any respect; two scruples of calomel were given at this time. At 5 P. M. the stupor had subsided, and she then complained of pain in the head, and soreness and stiffness of the neck. She was directed to have an enema, and take ten grains of calomel every three hours. On the 20th, she had become salivated, her skin warm, and pulse fuller, 110. From this time she continued to improve, and was discharged on the 23d, the salivation having become very profuse.

Case 5, (from Dr. Berney's notes.) Cornelius, Blacksmith, thirty years old, of sound constitution and regular habits, complained for the first time on the morning of the 17th of March, of head-ache over the entire frontal region, which caused him to quit work. The pain not having diminished at seven o'clock P. M., I, (Dr. B.) was requested to visit him. He now complains, as during the day, of pain in the forehead, but is free from pain or uneasiness in every part of the spine; his skin cool; tongue clean; pulse natural in frequency, but feeble; pupils natural; no thirst; bowels constipated.

12th 11 A. M. He was moved this morning early, at his own urgent request to his master's, four miles from town. Temperature of the surface rather below natural, hands and feet cooler than the rest of the body; pulse 96, small, feeble, and very compressible; delirium; excessive pain in the head and lumbar region; strong contraction of the flexor muscles of the left fore arm; extreme restlessness, rolling and tossing in bed, exclaiming "I shall die;" pupils natural; tongue clean; (this is a single exception to a rule which was otherwise uniform) no thirst; sensibility unembarrassed, as also muscular motion, except in the left arm: R. V. S. to thirty-eight ounces, blisters to neck, arms, thighs and legs, hot turpentine rubbed on extremities, calomel gr. XL. At 4 P. M. he was bled again to ten ounces, and again to the same extent during the night.

13th. The blood drawn on yesterday exhibits a soft coagulum, not buffed, and but little serum; pulse 94, still small and feeble; some tendency to stupor; the contraction of the arm having ceased entirely, there is now no embarrassment in the functions of any of the motor or sensitive nerves—R. Cal. and ext. colocynth comp., each five grains to be given every two hours, blisters over Ossa Parietalia.

14th. Tranquil, head free from pain; slight pain in the back; pulse 112, feeble and small; skin natural in temperature; herpetic eruption on the lips; asks for food, and says he is hungry; ptyalism suspected.

15th. Ptyalism evident; has been restless during the night from strangury, which is still very troublesome; is very loquacious. In the afternoon he was observed to pick at the bed clothes, pulse 112, a little fuller and stronger.

21st. But little change since the 15th until to-day. He has been troubled a great deal with strangury, and has had along with it, rigid contraction of the recti muscles of the abdomen. He has been free from delirium, and nearly so from pain, the pulse varying from 100 to 120; to-day he is stupid, and greatly prostrated—these increasing, he died on the 22d, without convulsions.

The two following may be called mixed cases, as pertaining to both of the malignant groups, having at the beginning those pertaining to the congestive.

Case 6. Miss B., a young lady, just returned home from boarding-school in excellent health, aged about 16, of a lymphatic temperament, had some degree of chilliness about midnight on the 16th March, which was followed by fever and pain in the head. On the morning of the 17th, the fever and pain continued; at four P. M. she had some feverish heat about the skin, her eyes were somewhat injected; her tongue was coated all over the dorsum; and her pulse was 118 beats to the minute, and nearly natural in fullness and force; she had no pain in the head when still, but complained of pain in the forehead when it was raised from the pillow; in about half an hour she vomited once, but made no complaint of sick stomach afterwards; at 9 P. M. she had a return of headache, and was somewhat restless until 11½ P. M. when she expressed a wish to get up, in doing which she was assisted by her father; but she had no sooner gained a sitting posture than she began to talk rapidly and incoherently; her articulation becoming indistinct, she very soon fell into a comatose state, and had a large involuntary evacuation from her bowels in bed. I saw her in half an hour afterwards, at which time she was comatose, but restless and moaning; her skin pale, cool, and dry; pulse about 70 beats to the minute, was small, soft, and irregular in the time and force of its beats. In a very short time the coma became profound and the breathing slow, and a little stertorous. She was now still, and insensible to any kind of stimulant; the right pupil was steadfastly dilated, so as to leave but a very narrow rim of the iris visible; the left pupil was dilated, but moved spontaneously, both being insensible to light. She was bled from the arm to twenty ounces,—the blood was not buffed; before it had ceased running the left pupil moved to the light of a candle to a limited extent, at the same time a free sweat broke out which was particularly cold on her forehead; she got restless, and her pulse quickened. After these symptoms had subsided, which required half an hour or more, six ounces more of blood were taken from the back of the neck by cups, and sometime afterwards eight ounces more from the arm, each of the last bleedings producing a recurrence of the symptoms which followed the first, after the last one her pulse rose to 104; in the meantime mustard had been applied to her extremities, hot applications had been made to various

parts of her body, and her head had been shaved, and a blister applied so as to cover a part of the frontal and nearly the whole of the parietal bones. For several hours afterwards the left pupil became more and more moveable, and the right contracted gradually, being still insensible to light. At 10 A. M. on the 18th, both pupils, though dilated, responded to the light, but not perfectly, and their motion was not consentaneous, when asleep both pupils were contracted, but not equally, and when awake they were unequally dilated, and the dilatation was invariably increased when she made any muscular effort. At this time the skin on the head and body had become warm, while her feet and hands were still cool, and her face very pale; her pulse was 108, small and soft; she was more sensible, spoke when importuned, and articulated well; drank water when it was put to her lips, and pressed the glass towards her mouth; it was now ascertained that the right eye lid was partially paralytic. At 7 P. M. she had recovered entirely from the coma; her skin had become warm all over, and she was free from pain of any kind. On the 19th, at 6 A. M., I was told that she had passed a very restless night, had vomited several times, and had had a frequent desire to evacuate her bowels; her pulse was now 76, small, soft and labouring; she complained of her head, and had a cool and dry skin. One hour afterwards, without any medicine having been given, all these symptoms had changed; her pulse was full, strong, firm, and 130 to the minute; her face flushed, and skin warm; had no pain; spoke cheerfully, and jested with a younger sister when she came to the bedside. This state of improvement continued throughout the day, her pulse falling in the evening to 104. At 12½ A. M. of the 20th, I was called to see her, and found that half an hour before she had been taken suddenly with a very acute throbbing, and lancinating pain, which occupied at times the whole of her head; her pulse was 120, full, steady, and firm; face flushed; lips deeply colored; frequent desire to go to stool; nausea, and at times vomiting; no delirium, nor coma; about five ounces of blood having been taken from her neck by cups she complained of feeling faint, and asked for cologne, at the same time her face and lips turned pale, and her pulse became very feeble. Her head not being at all relieved, two cups more were applied, drawing not more than an ounce and a half of blood, when the symptoms of approaching syncope appeared again, without the slightest relief, but rather an aggravation of the cephalalgia. As the morning advanced the pain increased in violence, so that she frequently cried out with it. She became a little delirious towards five o'clock A. M., asking irrelevant questions, and replying to imaginary ones. She now took one-fourth of a grain of morphine, and at 8 A. M. was sleeping quietly; pulse 80, full, irregular and hard; face flushed; skin hot and moist; no pain in the head; and though she talks when asleep, is free from delirium when awake; she asked for ice which was allowed her. At 3 P. M. I received a note from her Father to the effect that she had become very talkative and restless, and that her face had become more flushed; at 3½ P. M. I found her comatose, breathing slowly and stertorously; pulse 70, full and irregular; tongue much enlarged, and protruding from between the teeth; the mouth overflowing with saliva; face red; lips purple; she could be roused with some difficulty, and gave short

answers correctly, though her articulation was greatly impeded; she complained of being numb all over. Blood was again drawn, but without effect; the coma deepening rapidly, she died asphyxiated at 5½ P. M., her heart having continued to beat nearly two minutes after she had ceased to breathe.

Case 7. B. P., Black, male, aged 46, went to bed as well as usual on the night of the 1st of March, and was found at day-light lying with his feet on the door-sill, and his head and body on the ground, snoring and insensible. Half an hour later he had the following symptoms; feet and hands cold; body and head cool; pulse small and soft, and varying from 30 to 60 beats in the minute; eye-lids closed; respiration slow, irregular, suspirous and from 10 to 15 in the minute; occasional tetanic rigidity of the muscles of the back and extremities, very slight and lasting but a few seconds at a time; skin absolutely insensible to any ordinary stimulant; eye lids motionless when touched with a straw, and also when the conjunctiva was irritated; pupils not affected by light; contracted but moving spontaneously; deglutition difficult; occasional attempts to vomit; succeeded twice in throwing up some yellow fluid. At 1½ o'clock P. M. he had a strong convulsion, his pulse having previously risen to 96, and became full and firm, while his skin had attained more than the normal heat all over, and the reflex sensibility had been restored to the eyelids and conjunctiva. The convulsions recurring at short intervals, he died at 6½ o'clock P. M.

Second. *The mild congestive* form was distinguished from the former by the preservation of a good degree of strength, no matter how protracted the attack; the continued slowness of the pulse, which never fell below forty-six, nor rose above ninety, and its greater regularity in other respects; by the greater violence of the cephalalgia, and more frequent tenderness of the cervical vertebræ; and negatively, by the uniform absence of any cold stage; of coma; of delirium; of any nausea or vomiting, and of any embarrassment in the movements of other muscles than those of the neck.

Case 8. C. G., Mulatto, male, aged 26, Carpenter, had had headache for several days with an occasional cessation of an hour or so, but kept at work until the 22d of March, when he was obliged to quit on account of general weakness, with giddiness and dimness of vision. At 3½ P. M. he presented the following symptoms: surface of the body and head moist, and hands and feet cold; pupils natural; countenance dull; tongue moist, large, and indented by the teeth, coated with a thick nasty looking ash colored coat on the posterior three-fourths of its surface, and having an epithelium-like termination anteriorly; some pain in the back of the head, and giddiness when sitting up, and sometimes also when lying down; pulse 56, soft, and small; resp. 26. With these symptoms, which it is perceived differ from the malignant cases of this variety in little else than the absence of coma and delirium, and abnormal muscular contractions, the patient retained a good deal of strength, so as to be able to sit while being bled. He was bled to 3 xxiv, after which his pulse rose to 60, but was not otherwise changed. He had then a blister applied to his neck and back, and took a cathartic. On the next day his pulse was nearly in the same state, but his skin had become warm and dry; his head free from pain and giddiness, and he was discharged on the 24th without further treatment.

Case 9. Henry, Mulatto, male, aged 33 years, Barber; having had headache the day before, which remitted in the night, complained on the 17th of March of general feeling of heaviness in his head, and of fixed pain in the occiput, and also in the back of the neck, where there was some stiffness and soreness on motion; his countenance at this time was anxious, and he expressed apprehensions in regard to his condition; he was sitting up in his shop when I visited him, and had but recently been at his work. At this time his pulse was 70, small, and very feeble; his skin dry and cool elsewhere, was positively cold on his feet and hands; his tongue was broad, white and flabby, moist, and indented by the teeth. There was tenderness of all the cervical and two-thirds of the dorsal vertebræ; pressure on the latter produced a sharp pain, darting forward from the part pressed on to the sternum. He had a blister to his neck and back, and took an active cathartic. On the next day he was greatly better, and was discharged on the 19th.

II. *Of the Inflammatory class.*—In this class there is an intermediate group, viz;—the *grave*, between the malignant and the *mild*. Their affinity to each other of the individuals of these three groups was manifested by the symptoms which distinguish this from the congestive class; viz, the uniform presence of a temperature of the skin above that of health; a full, and generally a firm pulse, and a characteristic tongue, which corresponded with the second kind described at page —. The traits by which they were distinguished from each other are placed under the several heads, as follows:

First. The *Malignant*, was marked by the early occurrence of delirium, or of coma, the former being the more common; a great irregularity of the pulse; the abnormal contraction of the other large muscles of the neck, either tonic or convulsive; strabismus; and the occasional occurrence of amaurosis; a tendency to nausea and vomiting, and a tendency to a speedy and fatal termination.

Case 10. (From Dr. Pollard's notes.) F. M., white male, aged 25, painter, had a paroxysm of acute pain in the lower part of the back, on the evening of the 18th march, and another of very great severity twenty-four hours afterwards; was pretty well on the 20th, until near night, when he had a distinct chill, accompanied with pain in the region of the lower dorsal and lumbar vertebræ, extending into the groin, lancinating and of great severity, but without pain in the head. After some reaction had taken place, he was bled nearly to syncope, with only slight relief. On the 21st, at 7 A. M., his skin was hot and dry; pulse 104, full and tense; tongue moist, thickly coated on the posterior two-thirds, and red on the tip and edges; pain in the back, only on motion; general soreness of the muscles; face flushed; eyes brilliant, pupils dilated. He remained in this condition, without delirium or pain in the head, until 11½ o'clock P. M., when he was found much worse; he had become wildly delirious, was complaining of great pain in the head, tossing about the bed, pulling at his hair, and at times retching violently, bringing up only a little white froth; pulse 84. After losing fourteen ounces of blood, he became quiet, and continued so during the night, being rather comatose. On the 22d, the coma had increased; when roused he was delirious; in the course of this day his tongue became dry, and the coating was coming off. At 3½ P. M. he was bled again,

to fourteen ounces, immediately after which his pulse, which just before was 104, rose to 120, and became smaller; the coma was not at all diminished by the bleeding; sometime after it, he passed urine involuntarily. On the 23d, it was found that he had spent a quiet night, sleeping well, was free from coma, and nearly so from delirium; said his head felt better, (he had not complained of it for twenty-four hours,) but gave him some pain; pulse 100, full and soft; his tongue, however, was red and dry, and black sordes were gathering on his teeth, and he complained of great soreness all over. On the 25th, having been freed from pain in the head and delirium since the 23d, he complained of pain in his head again, the skin of which was hot; and he was inclined to wander in his speech, and his tongue continued to be dry and red. A pitcher full of cold water having been poured on his head, the pain and delirium disappeared, and his pulse fell from 84 to 76, and became softer. After remaining in this improved condition, but without improving for several days, he began to get better, but so slowly that he was not discharged for several weeks.

Case 11. (From Dr. Holt's notes.) J. H., white male, aged 12 years, was attacked, without premonition, on the morning of the 21st of April, with chilliness and pain in the forehead. In the course of two hours after, the chilliness had subsided; the pain in his head was violent, of a throbbing kind; his skin was hot and dry; face flushed and frowning; tongue coated white on the posterior two-thirds, and the tip and edges of a bright pink color; pupils natural; pulse 108, full, soft and irregular. On the next day (22d), I learned that he had had delirium through the night, and was restless and moaned a good deal. At 8 o'clock A. M., the delirium had subsided, but had left him entirely blind, so that he could not discover the sun light which shone on his eyes through the window. At the same time he was free from stupor, and answered questions even cheerfully; his pulse 120, full and soft; the pupils were dilated generally, but contracted at times spontaneously. About 12 M., he fell into a semi-comatose state, his pulse fell to 80, and was very irregular in force, and in the time between the beats; he had also strabismus, and spasmodic flexure of the fore-arms and hands. He complained now bitterly, when pressure was made on any part of the cervical vertebræ, or on the three or four first dorsal, saying, that the former produced pain in his head and neck, and the latter in the belly. He was bled from the arm, which took the flush out of his face and quickened his pulse to 120; the blood was not buffed. At 4 P. M. all these bad symptoms had subsided, including the amaurosis, and he remained tranquil and rational through the night. On the 23d, the febrile symptoms again increased, threatening a return of coma; he was again bled; the blood not buffed or cupped, after which his pulse, which, previously, was 120, rose very soon to 150. Little or no change occurred during the day, but in the night he became wild with delirium, which subsided towards morning to a milder state, but accompanied still with great restlessness. At 8 A. M., on the 24th, this state continued, the pupils were dilated; the vision imperfect, and the strabismus had returned; pulse 138. After eight ounces of blood were drawn from the temporal artery, the strabismus went off, but his pulse rose to 160. From this time he gradually sunk, without coma or convulsions, and with but little delirium, until 7 P. M., when he died.

Case 12. J. H., white male, aged 14 years, was taken sick sometime towards morning, on the night of the 18th of April, complaining of pain in the head. On the 19th, at 9 A. M., he was seen by Dr. Holt. He had then violent pain in his head, neck, back and legs; skin hot and dry; face flushed; eyes red; pupils natural; countenance anxious; tongue coated white on the posterior two-thirds, and bright pink on the edges and tip; pulse 100, full and firm, but not hard; no delirium, nor coma; lies on his left side chiefly; bowels constipated. He was directed to have a blister along the spinal column, from the head nearly to the lumbar vertebræ; hot pediluvium and sinapisms to his legs, and to take twenty grains of colomel, which was immediately thrown up with the contents of the stomach, but was repeated and retained. At 5 P. M., he had become delirious and very restless, with jactitation; he now took another scruple of calomel, and was directed to have ten grains every three hours, until half a drachm was given. On the 20th, at 9 A. M., the delirium, which had continued through the night, was increased; his extremities had become cool, while his body and head continued hot and dry. During the night he had several large green evacuations from the operation of the calomel, which had ceased to operate for some hours. At 5 P. M. he was furiously delirious. After this he soon took convulsions, and died in the night.

Secondly. *The grave* was distinguished from the former by the regularity of the pulse, which was never below one hundred in the minute, except during a remission; by the morbid contraction of the muscles being confined to those of the neck, and sometimes to the sterno-mastoid, of one, or both sides, alone; by the occasional occurrence of a painful sensibility of the skin to the touch, and of the retina to light; the occurrence of regular remissions, and the absence, uniformly, of coma, and of delirium.

Case 13. J. T., black male, aged 12 years, having had headache, with red and suffused eyes, from early in the morning of the 25th of March, was taken with a chill at four o'clock in the afternoon, which was soon followed by fever. He was seen soon after the febrile exacerbation begun, by Dr. McLester, who bled him from the arm to ten ounces, when he got sick at the stomach and vomited, and his pulse, which had been 120, fell to 104. His pulse, at this time, was full and firm; his skin hot and dry; his tongue was coated with a brownish fur, red on the tip and edges, and rather sharp; his headache was not relieved by the bleeding. At 3 A. M., on the 26th, he had become lethargic, rather than stupid; when awake, he complained of his head aching all over, but particularly in the frontal region; the pupils were somewhat dilated, and the conjunctivæ injected; the skin hot and dry, and the pulse firm and regular. He was bled to 14 oz., but without any perceptible relief to the headache, though his pulse was reduced in force. At 8 A. M., the febrile symptoms had, in a great measure, subsided, and, along with them, the headache, which was only felt when he raised his head from the pillow; then it throbbed painfully; he was ordered to take quinine, in doses of five grains, make into pills with mass. hydrg., every two hours. At 1 P. M., he had a return of fever without any chill having preceded it, and with it the cephalalgia returned with increased violence, being throbbing and lancinating; pulse 124,

full and strong; skin dry and hot; resp. 28; slight strabismus when looking intently; sleeping when not complaining of his head. Bled to sixteen ounces, after which his pulse became softer, and fell to 118. 10 P. M., remission of fever; pulse smaller and softer, 110; no pain nor drowsiness; talks freely; has no recollection of having seen me to-day, nor last night, nor of being bled. At 7 A. M. next day, he was free from pain, pulse 104. At 5 P. M., he was found with fever again; pulse 128; darting pain in the right eye, cheeks and gums; free discharge of saliva; discharge of thick mucus from the nose; sense of smell confined to one nostril; no headache nor drowsiness. This paroxysm declined early in the night, and on the 28th he was nearly free from fever, and had some appetite. On the next day he had continued to improve, and was discharged convalescent.

Case 14. G. O., female, aged 9 years, clear white skin, large light blue eyes and light curling hair, delicately formed and nurtured, who had complained occasionally for several days of pain in the back and soreness in her legs, when playing, was taken with a slight chilliness, which was followed by fever, at 1 A. M. on the night of the 20th March. At 9 A. M. has fever; pulse 120, sharp and jerking, but not hard; skin hot and dry; eyes pinkish and watery; tongue coated brown on the posterior two-thirds, and pink at the tip and edges; intense pain in the forehead and eyes, alternately; pain frequently lancinating; excessive intolerance of light; soreness and stiffness in the sterno-mastoid muscle, on both sides, and soreness in the muscles of the arms and legs. This state continued until the next morning (21st), when there was a considerable remission of all the symptoms, except the pain in the head, and the intolerance of light, neither of which were much mitigated. On the 22d, she was still improving, and continued comfortable until 10 o'clock, P. M., when the febrile symptoms returned, as I was told, with an aggravation of pain, &c. On the 23d, at 8 A. M., her pulse was 124, full and sharp; intolerance of light, and intense headache; painful sensibility of the skin, so much so that, at times, she could not bear to be touched; when her head was raised she complained very much; on one occasion she said, "you are dragging my head to pieces." She was now directed to take carb. potass in solution, four grains every two hours, and to have a blister to the neck. At 12 M. all these bad symptoms had disappeared—the pulse had fallen to 84, the skin cool and moist; the face, which had all along been flushed, was now pale; she had neither pain in the head, nor soreness in the skin or muscles, and was cheerful. The carb. of potass was now suspended, and two grains of quinine ordered in its place, every two hours. On the 24th, at 9 A. M., I learned that the fever had come on early in the night, and had continued, without any remission; her cheeks were again flushed, and the pain in the head; intolerance of light, and sensibility of the surface had returned. The quinine was now suspended, and the carb. potass ordered to be given as before. At 1 P. M., the symptoms of head disease had again disappeared, and there was a very great remission of fever, and this state of improvement continued, with but little interruption, until the 27th, when she was discharged convalescent.

Thirdly. *Mild Inflammatory*. The individuals of this group were distinguished from either of the preceding, by the lower grade of the febrile excitement; the preservation of a good degree of strength, no

matter how long the attack might be continued; a tendency to take on a chronic form: and, negatively, by the absence of coma, drowsiness, or delirium, and of a cold stage.

Case 15. Nat, black male, aged 18 years, drayman, had had headache for a week when he came to my office on the 21st of March, having continued at work until the 20th. He has now fullness and pain all over his head, but particularly in the frontal and occipital regions; his skin has a feverish heat and dryness; his pulse is 80, and full; his tongue has a white coat on it and red edges not enlarged. He was bled sitting to xxxii oz., when he became faint, and his pulse got small and quick, while his skin remained dry. After lying down a few minutes he got up, and again became very faint, and his pulse slow and soft; the dryness of the skin persisting; blood not buffed; the coagulum very large and firmer than usual in this disease. He had a blister to his neck, and took a cathartic dose of colomel and rhubarb, was directed to take some quinine, and return the next day. On the 22d, he was without headache, but had a very troublesome sense of fullness in the head, and shrunk from a very slight pressure at the junction of the occiput with the spine; pulse full and firmer, 104. He was ordered to take a pill composed of five grains of mass. hydrg., and one fourth of a grain of tart ant. every four hours. On the 23d his gums became sore, and on the 24th he was discharged.

Case 16. Solomon, aged 32, black, stout and healthy, had pain in the head and loss of appetite on the 23d of March, which continued through the night. He went to work on the 24th, but the pain becoming aggravated he desisted, and went home where he was visited by Dr. McLester, at 12½ o'clock, p. m. He had then acute pain in the frontal and occipital regions, and in the neck and back, which was aggravated by pressure on all the cervical and on the dorsal vertebræ as far down as the sixth; eyes injected; tongue covered with a thin white coat; some nausea; bowels constipated; pulse 90, regular and tolerably full and strong; skin warm and dry; he was bled to xxxii oz., and had a blister along the upper half of the spinal column, and took a dose of calomel with tartar emetic; he became very faint from the bleeding, and vomited. At 2½ p. m., the head not having been at all relieved, he was bled from the arm to xvi oz. more, which brought on syncope, with vomiting, cramp in the fingers, and temporary delirium. At 6 p. m., he still had pain in his head when raising himself in the bed, and also in the back. On the 25th at 8 a. m., it was found that he had rested very well during the night, was free from pain and fever, pulse 65, and strong. On the 26th was convalescent, but had forgotten nearly all that had passed during his sickness, and had no conception of the length of time since he was taken.

In other cases of the mild varieties the disease was more protracted, lasting sometimes ten days, or two, or even three weeks, the patients all the time being able to sit up and walk about a little. In one instance, a man came to my office, having had pain in his head, stiffness in the muscles of his neck, double vision, and dimness of sight for several days—symptoms which required large bleedings and a salivation to remove.

Sometimes the effects of the mild cases, when apparent recovery took place within two or three days, were experienced for weeks in the form of confusion of the mind; a feeling of tightness across the forehead, or

about the base of the brain, from one temporal bone to the other ; or pain in the occiput, or neck, upon any unusual mental occupation. Upon one occasion, a very mild attack, in a man about thirty years old, who was dismissed as cured on the second or third day, was followed by great imbecility of mind and weakness of body, which continued for some weeks. It was remarkable, as I have before said, that no matter how long continued these mild cases might be, if they began with the cool skin, and feeble and slow pulse of the congestive form, so they continued to the end, or until convalescence was begun ; and so also of those which assumed the inflammatory form.

Remissions.—I have selected the preceding cases, for the purpose of illustrating the general features of the several groups into which I have gathered the innumerable varieties which the disease assumed, without reference to an important feature which, though frequently unobserved, occurred in a great number of cases belonging to each division, viz : a tendency to remission, which was manifested very irregularly at some period of the attack, varying from a few hours to several days from its onset ; but occurring most frequently on the second and third days—days on which, also, the greater proportion of deaths occurred. These remissions were sometimes very great, approaching very nearly to intermissions ; at others, they were obscure, and there were various degrees of amelioration of the symptoms between these extremes. They were irregular also in other respects—sometimes they were quotidian or tertian, and accompanied by remissions of fever, so as to make it appear as if the meningitis were engrafted, as it were, on an ordinary endemic fever. In other cases, a remission of the cerebral symptoms took place without any very obvious change in the febrile state ; and in others, again, the febrile symptoms remitted, or, it might be, intermitted, leaving those of the head disease unmitigated. In all these cases, though the hours of their return might be uncertain, the changes were so evidently remissions merely, that there was but little difficulty in identifying them as such, so that future changes could, to a great extent, be anticipated ; but there were others which, from their great obscurity in this respect, and from their more frequent connection with the malignant forms of the disease, were much more important in a practical point of view. In such cases as I now refer to, after an uncertain time, it might be not more than twelve hours, or it might be four or five days, or any intervening period, but commonly on the second or third day, there would take place a change, sometimes so decidedly favorable as to lead to the belief that convalescence was established, or at least about to become so ; the pulse and skin might approach very nearly to the condition of health, though some irregularity or abnormal sharpness or frequency might always be detected in the former (a fact of great importance) ; the mind sound and even cheerful ; the head free from pain, and the muscles from abnormal contractions, and this state of things might end in a rapid recovery. It was very seldom, however, that the promise was so kept, if the preceding stage had lasted more than twenty-four hours ; such remissions, or pauses, in the march of the disease, being generally followed after an interval, the length of which could never be foreseen, but which might be a few hours or days, by an exacerbation of equal severity as the first. At other times, these remissions were less decided, but still sufficiently so to feed the hopes of the patient, and frequently

to delude the judgment of the physician. In a few cases, the remissions were somewhat peculiar, consisting in a change of form from the congestive to the inflammatory type; as, for example, when a state of stupor or coma, attended with a cool skin and feeble pulse, was changed to a state of reaction, in which the stupor or coma went off, and the skin became warm and the pulse full and strong; the latter state always presenting features less alarming than the former.

It was in the inflammatory forms, particularly the grave varieties, that the remissions were most regular, and in which there was, consequently, the least difficulty in recognizing them as such.

Diagnosis.—The perusal of the several groups into which I have gathered the more constant phases in which the epidemic presented itself, will serve to show, but partially, the exceeding obscurity and difficulty of the diagnosis before it was suspected that an epidemic meningitis was prevailing. The individuals of each of these groups presented other varieties than those I have given examples of, and, it unfortunately so happened, that those most widely separated were the first that appeared, and this successively. The consequence of this want of uniformity was, that the opinion of the physician had to be made up by the study of each case, almost unaided by any analogy with the symptoms of a preceding one, and besides this, so great was the diversity, that even the post-mortem examinations, which were made in every one of the first few fatal cases, threw but little light beyond the particular case examined. The first cases were nearly all of them of the congestive form, which, although they might present unequivocal evidences of an affection of the brain or its meninges, and even this they did not always do, the symptoms indicated, if at all, but very obscurely, its true seat and nature. To all this it must be added, that there was only that general resemblance to sporadic meningo-cerebritis as might occur in diseases differing in their nature, and not unfrequently even this general resemblance was wanting. Simulating, then, as the epidemic did, in the malignant varieties, in one attack an endemic cerebro-congestive fever; in another, idiopathic apoplexy; in another, abscess of the frontal sinus, and in another, hysteria, it will be readily seen that but little aid in the diagnosis could be attained, either from the similarity of one of these cases to another, or, to the sporadic disease in its common aspect. The symptoms of spinal inflammation, too, as may have been observed, were always obscure, without excepting those cases in which there was tenderness on pressure of the vertebral processes, for this is not held to be, ordinarily at least, a sign of inflammation.

I need hardly add, nor can it be a matter of surprise, that in several instances, at the breaking out of the epidemic, the diagnosis was either erroneously made, or remained doubtful, until a post-mortem examination, or a more extended observation, satisfactorily determined it. It is to be remarked, however, that such errors or doubts could only occur at the beginning of the epidemic. After several post-mortem examinations, and a free interchange of opinion among the members of the medical society were had, and it was thus ascertained that these varieties of fatal disease represented an epidemic meningitis, the difficulties were at once either fully dissipated, or sufficiently so to secure a prompt and proper treatment when a doubt existed. But even with this qualification, such errors or doubts as did occur, were of a magnitude proportion-

nate to the fatality of the disease, for any treatment to be efficient, as was abundantly proved, had to be entered on with the utmost vigor, on the first day, and thus an error in the diagnosis was calculated, and no doubt did so, occasionally so far to modify the treatment as to suffer the precious moments to escape, when a more active medication would have been successful.

The preceding observations are chiefly intended to be applied to the malignant varieties. In the mild and grave varieties, the errors, if made, were of but little consequence, as none of the cases belonging to them proved fatal, and many were not attended with even the appearance of danger. Nevertheless, difficulties did occur in the diagnosis of these cases, which if they were of but little comparative consequence to the patient, were troublesome and embarrassing to the physician. There was necessarily a good deal of doubt at first, as to the nature of the cases, being comparatively so mild, whether they were in fact but the insidious precursors of a more malignant aspect, or were the disease in a mild form, fully developed—a difficulty, in fact, in prognosis as well as diagnosis. Whilst such doubts existed, they were frequently treated with unnecessary vigor, were watched with the utmost vigilance, and were the source of incessant anxiety. They were embarrassing in this, that on the one hand the indications in the mildest varieties, which were the most numerous, nor in the less violent of the grave, did not warrant, without better proof of present or prospective danger than any at hand, the active depletion and urgent efforts to bring about a speedy mercurialization which was thought indispensable in the malignant varieties; and, on the other, it was feared that to neglect these remedies might compromise the safety of the patient. It was not till after the epidemic had made considerable progress, that this source of trouble was obviated by the steadiness with which each variety of the several groups run its course, without intermingling with another.

The symptoms by which the meningitis was distinguished after some familiarity with the symptoms was obtained, were the following, viz: The cephalalgia, its seat, particularly when in the back of the head, and other peculiarities connected with the nature of the pain as differing from other headaches. The state of the tongue, particularly its enlargement, indentation, and gummy or pasty coating, and the bright pink of its edges. The physical characters of the blood—the pain in the neck, and stiffness and soreness in the muscles—muscular tremors, and other embarrassment of motion; and traction of the occiput downwards—the state of the pulse, which was valuable as a diagnostic sign in both varieties of the congestive type, but only in the malignant varieties of the inflammatory type—delirium; rigidity of the large extensors of the spine; coma—the state of the pupil, and the injection of the conjunctiva which last, though not so important as the other, was more constant.

Age, sex, color.—Among eighty-five cases, sixty-four of which were taken from the register of Dr. McLester and myself, and twenty-one from all the notes furnished me by other physicians,) there were of *whites*, five years old and under, one,—over five and not exceeding ten, two,—over ten and not exceeding twenty, eight,—over twenty and not exceeding thirty, three,—over thirty and not exceeding forty, three,—over forty and not exceeding fifty, two,—over fifty, three. Of these ten were

males and twelve females. Of *blacks* under five years, one,—over five and not exceeding ten, six,—over ten and not exceeding twenty, fifteen,—over twenty and not exceeding thirty, twenty-four,—over thirty and not exceeding forty, ten,—over forty and not exceeding fifty, five,—over fifty, two. Of these thirty-six were males and twenty-seven females.

Mortality.—The mortality was confined to the malignant varieties. Among these, the ratio from the best information I can get was about sixty per cent.

Locality.—The little city of Montgomery is situated on the left bank of the Alabama river, at the extreme southern convexity of a horse shoe shaped curvature, or bend of the river. The *bend* nearly encloses several thousand acres of alluvial land which is subjected to an annual overflow. The geological formation on which the town is built, following Mr. Lyell's division, is the eocene tertiary. At distances varying from fourteen to thirty miles north and east, the tertiary forms an abrupt junction with the primary, consisting at various points of mica slate, gneiss, and granite. A belt of the cretaceous formation (prairie) running east and west, and varying in width from eight to fifteen miles, approaching within a mile of the southern and south-eastern border of the town. Beyond the prairie towards the south, the eocene tertiary is again found. On the western limit of the town a range of hills takes its rise rather abruptly, and running first south, then east, and then north, forms an amphitheatre, which rises in some places one hundred and fifty feet above the river bank, looking from its northern aspect on the river and the alluvial ground beyond it, and is crowned with a narrow strip of table land, beyond which, except on the eastern side, the ground again descends for several miles to a large creek which empties itself into the river, several miles below, or west of the town. A basin is thus formed with the hills on three sides nearly, and the river on the other, having a sufficient inclination towards the latter to carry off the water which gathers during a rain in rapid currents, and thus to secure, whenever a hard rain falls, a thorough cleansing of every part of the town. It is within the limits just described, and on the hills above that the town is built; the greater part of the population residing below the range of hills. The residents on the sides of the hills, and on the level above, hardly comprise one-third of the whole population, which numbers in all about four thousand souls, a large part of which, the proportion not being known, are blacks. The dwellings are built with ample space around them; there is no crowding, nor are the residents crowded within them; nor are there any accumulations of filth any where to be found at any season; for besides the frequent washings in wet seasons from rain, the yards and streets are subjected to frequent inspection by a commission which always includes at least one physician; neither is there found within the city any of the destitution of extreme poverty, all the inhabitants being supplied, if not by their own exertions, by the charity of others, with a sufficient supply of good and comfortable clothing—but objects of such charity are very seldom seen. The residents on the sides and top of the hills have no advantage in any of these respects over those occupying the less elevated parts of the town.

It was within the limits just described bounded by the base of the hills on nearly three sides, and by the river on the other, that the epidemic was chiefly confined. whilst a large porportion of the residents

below were attacked with the disease there, but very few cases were found among the residents on the sides of the hills, and still fewer on the table land above. The statistics of Dr. McLester and myself give three cases on the hills for sixty-four below them, which is larger than the general average. From the best information I can get, there were not less than two hundred and fifty cases within the town limits, of which not more than ten occurred above the base of the hills. As, however, but few of the physicians register their cases, the former number is in some measure conjectural; I am certain, nevertheless, that the proportion given is not too large.

The surrounding country which is thickly populated, was remarkably exempt at this time, not only from the visitation of the epidemic, but from all other diseases, contrasting forcibly in both respects with the city. After the epidemic influence had exhausted its virulence here, and had in a great measure subsided, the disease made its appearance on a large prairie plantation, belonging to Mr. Wm. Knox, lying eight miles south-east of the city, where according to a report kindly furnished me by Dr. Holt, nineteen cases occurred, a majority of which were of the malignant congestive type. About the same time it fell upon another plantation (prairie) some three miles south of the former, stepping over an intervening one, and leaving the latter, as also the other adjoining ones, as free from sickness, as at the same season in other years. After it had disappeared from these localities, it again made its appearance on the plantation of N. Harris Esq., situated three miles west of that of Mr. Knox, where four cases occurred, all of which were of the grave inflammatory variety, and here, the epidemic influence terminated. In regard to the causes which terminated the attack on these plantations, to the exclusion of others, I can give no information, any more than why it fell on a part of the town of Montgomery, almost to the exclusion of another part. In regard to those of Mr. Knox and Mr. Harris, I can affirm from personal observation, that there was nothing more apparent than in the city, which might be supposed to invite the visitation of any disease of an epidemic kind. The negro houses on both, are unusually comfortable and cleanly, and the negroes are well fed, and well clothed, and are not overworked, nor otherwise harshly treated.

General influence of the constitution of the atmosphere.—The meningitis made its appearance early in February, was most prevalent in March, and disappeared as an epidemic in the latter part of April. Three cases occurred after the first of May, but were divided from each other by intervals of some weeks. An epidemic *roseola* preceded and accompanied the former as long as it lasted, and disappeared along with it, and this was the case on the plantation, referred to above, as well as in the city. Along with these epidemic diseases, others which are common to this climate and locality prevailed, some of them to a very unusual extent for the season. There were dysentery, diarrhœa, intermittent, and remittent fever; various forms of neuralgia, spasmodic cephalalgia, catarrhal fever and bronchitis.

The following table of Meteorological observations was made by Mr. Swan of this city, from his own records. It shows an unusually high temperature for January, and February, and a very low one for March. The almost total absence of rain in January is also remarkable:

TABLE OF METEOROLOGICAL OBSERVATIONS, REDUCED TO THE WEEKLY MEAN OF BAROMETER AND THERMOMETER, FROM DECEMBER 23^d, 1847, TWENTY-EIGHT OBSERVATIONS PER WEEK, TILL MARCH 16TH, 1848, TWENTY-ONE PER WEEK AFTERWARDS.

	Bar.	Ther.	N.	S.	E.	W.	N.E.	S.E.	N.W.	S.W.	Clear.	Cloudy.	Rain.	Quantity of Rain fallen.		Bar.	Ther.
														24th & 25th	in. 100ths.		
Dec. 23d to 30th	29.97	40°	4	3	1	0	7	4	3	6	17	10	1	—	.46	—	—
Dec. 30 to Jan. 6	29.99	51	1	5	0	2	2	2	10	6	15	13	0	—	—	—	—
Jan. 6 to 13	29.94	48	1	0	0	3	1	4	10	9	13	15	9	—	—	—	—
Jan. 13 to 20	30.03	59	5	4	3	0	1	1	3	11	11	17	0	—	—	—	—
Jan. 20 to 27	29.94	54	7	8	5	1	1	2	1	3	13	10	5	—	.56	—	—
Jan. 27 to Feb. 3	29.80	53	1	3	4	2	1	4	8	5	14	10	4	25th & 26th	3.24	—	—
Feb. 3 to 10	29.88	50	0	0	2	7	5	0	13	2	20	8	0	27th to 2nd	—	—	—
" 10 to 17	30.00	60	0	7	1	6	0	0	6	8	11	16	1	—	.13	—	—
" 17 to 24	29.81	63	0	5	1	3	3	4	5	7	6	15	7	—	4.32	—	—
Feb. 24 to Mar. 2	29.89	59	0	4	3	0	2	0	4	15	16	12	0	—	—	—	—
March 2 to 9	29.86	49	2	3	1	5	1	3	7	6	15	10	3	—	—	Mar. 6*	30.16
" 9 to 16	29.98	54	1	4	0	0	4	1	6	5	18	3	0	—	—	" 8	32°
" 16 to 23	29.83	68	2	0	0	3	0	0	0	16	4	15	2	—	—	" 9	70°
" 23 to 30	29.85	70	2	2	0	0	0	0	1	16	14	4	3	—	2.13	—	—
Mar. 30 to Apr. 6	29.93	67	1	11	0	3	0	6	0	0	8	11	2	—	1.02	—	—
Apr. 6 to 13	29.81	68	0	0	5	0	0	3	0	13	13	7	1	—	2.10	—	—
" 13 to 20	29.93	65	6	7	2	0	1	2	2	1	13	8	0	—	—	—	—
" 20 to 27	29.91	72	2	3	1	6	1	2	4	2	10	8	3	—	.22	—	—
Apr. 27 to May 4	29.87	79	0	3	1	0	0	6	0	11	9	9	3	22nd	1.11	—	—
			35	72	30	41	30	44	83	142	240	201	35	29th to 1st			

* Mornings of March 6th and 7th there was frost.

TREATMENT.

In what I have to say under this head, I shall refer chiefly to the grave and malignant varieties, premising however, that the same remedies were employed in the treatment of the mild varieties, but of course with less activity and perseverance.

Bloodletting.—This remedy was employed, with a few exceptions only near the beginning of the attack. It was used frequently, and boldly, without regard to the state of the circulation, that is, as readily and freely in the congestive, as in the inflammatory forms. The quantity taken at one bleeding, or at several in quick succession, was sometimes very great—on one occasion the quantity ascertained to have been taken at a single bleeding was forty-eight, and on another, forty-four and a half ounces.

In many other instances, bleedings equally large, or larger than these were had, but the precise quantities were not ascertained. In one case which I visited in consultation, eighty ounces were taken at several bleedings within twenty-four hours. In a case treated by Dr. Boling, a female, fifteen years old, with the notes of which I have been furnished, there was taken by cups from the neck and occiput, forty-eight ounces, and from the arm twenty-six ounces, all within eighteen hours. These are extreme quantities; in the greater number—the quantity taken altogether from adults in one day, varied from fourteen to forty ounces—in twenty-six cases, the average was found to be thirty ounces. The effects of bloodletting were not so satisfactory as might have been expected. Within my own observation they were never promptly decisive for much good or evil; the pulse in congestive cases rarely ever filled up, or became regular from it; in some instances it became quicker and more feeble during, or soon after rather a small bleeding taken on the first day of the attack. In the inflammatory form also, the same thing was occasionally observed, though in this form, an improved state of the pulse was more frequently the immediate consequence of the loss of blood. The most common sensible effect, however, was relief to the cephalalgia, but even this advantage was not always gained. In the case referred to above, of the loss of forty-four and a half ounces at one bleeding, the pain which was distressing to the last degree, was hardly at all relieved, although the pulse was enfeebled by it, and the face made pale at the time, and for several days afterwards. In another instance in which the symptoms, besides the cephalalgia, seemed urgently to call for this remedy, no relief whatever was obtained from it, notwithstanding that both the pulse and skin were greatly affected by it, as in the other case just mentioned, and a tendency to syncope was much complained of during and for some time after the abstraction of blood;—Nevertheless, prompt and free bleeding in the early stages, and the earlier it seemed the better, must I think have been of considerable advantage in this epidemic in both the congestive and inflammatory forms. Though the benefit was neither immediate nor decisive, yet I cannot doubt in surveying the whole ground that time at least was gained by it for other remedies more obviously beneficial to produce their effects. It is certainly true, however, that the impression of the physicians generally here, in regard

to bleeding, even in the bold, and I may be allowed to add, in reference to others, the judicious manner, in which it was applied, was one of disappointment.

Mercury.—This remedy was employed almost exclusively for the purpose of producing its constitutional effects, and so used proved to be a more efficient one than blood-letting, both in the promptness and permanency of its beneficial influence. As a general rule, this object could be speedily obtained, more so, I think, than is common in the phlegmasiæ, there being no tendency to diarrhœa; the medicines never appeared to irritate the bowels, nor did I ever observe that any unfavorable influence of any kind was exerted by it. If not always effectual, it was always safe; and the good obtained by it was more apparent and quite as permanent, as that from all other remedies. It is to be remarked, however, in modification of this general commendation, that in a few instances a salivation having been induced early in the attack and kept up for several days failed altogether in producing any perceptible influence; that in some cases time was not afforded to effect salivation, and that occasionally, though very rarely, when time was had, the specific influence of mercury could not be obtained, neither by inunction, which was a common mode of using it, nor by the internal administration of any of the pharmaceutical preparations in common use.

Rubefacients.—Among this class of remedies, blisters were found to be very valuable applied to the upper part of the vertebral column,—in mild and grave cases they seldom failed to remove, or greatly relieve the cephalalgia, even when bleeding had failed to do so. In the malignant varieties also, the relief afforded by them was frequently very great. In the congestive forms heat applied to the skin generally in various ways, mustard plasters, and frictions with oil of turpentine were frequently beneficial.

Potassæ.—This is a new remedy in cerebral affections, and as such I am unwilling to speak of its curative qualities in the disease under consideration, with the confidence I really feel in it. My experience in its use is yet too limited to determine positively its value, but I have seen enough to induce me to continue my observations, and to recommend it to the attention of the profession. I have prescribed it for several years in acute hydrocephalus, seemingly with great advantage—I say seemingly, for its effects were never marked by any sudden change for the better, and its claim to merit in these cases, consequently, must rest merely on the rather equivocal fact, that a very large proportion of those I have treated here by it, have recovered. It was at the suggestion of my friend Dr. Geo. O. Jarvis, who was detained here by sickness, to whom I communicated my experience of its supposed influence in hydrocephalus, that I was induced to administer it in the still more acute meningo-cerebritis of this epidemic.

The additional evidence which I have thus acquired is more direct, and, consequently is entitled to more weight, but is yet as the reader will observe, by no means such as to establish its remedial powers.

In the cerebral affections of children I have prescribed the carbonate in doses of from half a grain to a grain, repeated every four or six hours. In the epidemic disease, the dose for children was from three to five

grains and for adults, from ten to fifteen grains, repeated every two hours. No case proved fatal in my practice, nor, so far as I can learn, in that of either of my professional brethren who used it, in which it was freely and continuously employed. But it is to be observed, that under any treatment, the mortality was confined to the malignant forms of the disease; that the remedy was given in but few cases of this kind; and that other remedies of the most energetic kind were also administered along with it; so that it cannot be said to have had its powers fairly tested in any one of them; nor can I say with any confidence that all would not have got well without its aid, for I have not sufficient facts on which to express such an opinion; the powers of the remedy were exhibited in a much less equivocal manner in the grave and mild varieties. In many cases in which there were no febrile symptoms, properly so called present, the cephalalgia was speedily and permanently relieved; and in others, its administration was followed by a prompt reduction of arterial excitement, and the removal of intense cephalalgia and other symptoms of head disease, and this in such a manner, and under such circumstances as to leave no doubt on my mind in regard to the beneficial agency of the medicine. In one chronic case, of the grave variety, in which the patient was fast sinking into a state of dementia, convalescence began on the day that the medicine was first administered. This man had been bled, salivated and blistered without benefit.

Quinine.—I have but little to say here in favor of this medicine. Having been taught by experience, many years ago, that quinine was inadmissible in cerebral inflammation, both in children and adults, I expected to gain but little from its use, and was not disappointed. I employed it frequently in the grave variety, to which it seemed most applicable, and sometimes with partial success. When the disease was attended with a fever which was regularly remittent, the meningitis appearing as an appendage, or as if engrafted on a remittent fever, quinine did occasionally arrest the paroxysms, but more slowly and with greater difficulty than in other fevers. As a remedy in other varieties it cannot be recommended; its use here, if not hazardous, never affording much encouragement to repeat it.

Emetics and Cathartics.—Emetics were but little used. Cathartics were frequently employed as adjuncts, and to effect particular indications, but were not relied on as curative remedies. In two instances a free catharsis induced by drastic medicine was evidently prejudicial.

Narcotics.—Opium was the only remedy of this class which I used. From what I saw of it I can hardly say that it was generally safe to give it in the inflammatory malignant variety, or that it was of any use in the congestive malignant form. In the other varieties it was a safe remedy, and very valuable as an anodyne merely.

III.—*A Case of Superfætation and Mixed Birth.* By THOS. B. TAYLOR, M. D., of Princeton, Miss. September, 1848.

The following remarkable case of superfætation and mixed birth occurred in this neighborhood a few months since: and as there are but few cases of a similar character on record, and as this differs in

some of its features from any other, I have concluded to report it, provided you may deem it worthy of a place in your Journal.

Clarissa, a negress, the property of Mr. A. Kuox, aged about 35 years, in May last, was delivered of twins; one a mulatto, and the other a negro child. She had been married to a negro man on the plantation, of delicate constitution, for many years, and had had several children by him. Her menstrual discharge had occurred for several months previous to her pregnancy, at about the full of the moon. She felt herself pregnant by her customary signs, about the middle of the month; and, to confirm her suspicions, at the next period, it did not appear. About three weeks from the time she first felt she had conceived, and one week after her menses had failed to appear, she had sexual intercourse *once* with a white man. She slept with her husband every night—had connection with him the night before she had intercourse with the white man, but not on the same night. At their birth the mulatto child bore marks of being at least three weeks younger than the negro; thus sustaining the woman in her suppositions, as to the time between her two conceptions. This woman is a faithful servant, and I have every reason to believe she told the truth in relating the circumstances of her case to me.

On this extraordinary case, I beg leave to make a few remarks. It proves two important propositions; 1st. That superfœtation is possible three weeks after the first conception; 2d. That the semen of some men is more prolific than that of others.

I believe the opinion generally entertained by physiologists now is, that superfœtation is confined to a very early period after the first conception and that it is impossible after the impregnated ovule has passed into the uterus, as shortly after conception, certain changes take place in the womb, and its inner surface is lined by an efflorescence of coagulated lymph." Dr. Dunglison on this subject says: "When such a change has been effected," (that is after the exudation of lymph, or the formation of the decidua vera,) it would seem to be impossible for the male sperm to reach the ovary; and, accordingly, the general belief is, that superfœtation is only practicable prior to these changes, which may perhaps require twenty-four hours for their accomplishment."*

In the majority of cases of superfœtation and mixed conception on record,† both the white and negro man had connection within an hour of each other, and there being two ova ripe for impregnation, both would in all probability have been fecundated by the semen of the first had the second have had no connection with the woman—if not then, at least at the next copulation. Where both white and black male have connection in such quick succession, it is probable, that each ovary contains an ovum ripe for impregnation; the semen of the first, the white man, say, being before, so fills up one of the fallopian tubes, as to preclude the entrance of the negro's, which is drawn to the other tube, and thus fecundates the other ovum. And we see nothing more remarkable in

* Dunglison's Physiology, vol. 1, p. 323. 1st edition.

† I refer the reader for a notice of the most remarkable of such cases on record to Dunglison's Physiology, vol. 2, p. 324. 1st edition.

these cases, than what constantly occurs among the multiparous animals. My next door neighbor informs me that a few months ago, while his spaniel bitch was in *heat*, she was lined the first day by a Newfoundland dog, the second by a hound, and the third by a spaniel, and that on her litter she had *five* pups; the Newfoundland blood predominating in two, the hound in two, and the fifth was a full blood spaniel. Another friend informs me that, a short time since, his bitch during her season of *heat*, was lined only once by a dog, and put up, and that on littering she had *nine* pups.

But in the case of the woman Clarissa, three weeks had elapsed from the time she first felt herself pregnant, to her having connection with the white man; hence the fecundated ovum had passed into the uterus,—the os uteri and both of its cornua plugged up to a more or less extent by a tenacious mucus; and the os uteri, and the uterine extremity of one of the Fallopian tubes, besides, covered by the decidua, and yet the semen of the white man reached the ovary, notwithstanding all these obstructions; for according to the recent experiments of Bischoff, if an ovule is not fecundated prior to its entering the womb, it is no longer fecundable, and dies. But why had not both ova been fecundated by the semen of the negro? and why did the single hasty embrace of the white man effect that which had not been effected by the repeated embraces of the negro, for three weeks? It may be argued, that the second ovum was not ripe for impregnation, until the night the woman had intercourse with the white man and that it would have been fecundated by the husband, had he have had connection with her instead of him. But this is scarcely probable, as the negro had connection with her the night previous; and the exceeding rare occurrence of superfœtation notwithstanding the number of twins born (in the proportion of one to eighty,) proves, that as a general law of the uterine economy, both ova are furnished at the same time, and are fecundated by the same emissio seminis, or, at least, within a very short time of each other. And there is reason to believe, that the same ovary contained both ovules; as the semen of the white man must have entered the same Fallopian tube, through which the fecundated ovule had passed into the womb, the uterine extremity of which, from the decidua being reflected down after its descent, was left comparatively open; whereas, the uterine extremity of the other, was so closed and obstructed by it, as apparently to prevent the entrance of the semen. It would appear then, that both ova were furnished by the same ovary and were ripe for impregnation at the same time the first was impregnated—why then were not both ova fecundated at the time, or shortly after, the first ovum was, by the semen of the negro?

The investigations of modern physiologists have thrown great light upon this mysterious subject. After Leewenhoeck, or rather Hamme, discovered, that the semen masculinum contained animalculæ or spermatozoa, they were regarded as performing a very conspicuous part in the process of generation; and the experiments of several modern Physiologists, Wagner,* Bischoff,† Wharton Jones,‡ and others, clear-

* Wagner's Physiology.

† Entwicklungsgeschichte des Hunde-Eies, 1845.

‡ Brit. and For. Med. Rev. Vol. xvi., 1843.

ly show, that if they are not the prolific principle of the liquor seminis, they are at least the agents of it, by acting as carriers of the semen to the ovaria (Bischoff). Wagner, in his experiments on the dog, found that except during the season of *heat* of the female, his semen contained no spermatozoa; but that during this period, it was filled with them. Thus it would appear, that the several days preceding the season of heat, of the female, during which the male is constantly teasing and caressing her, were designed to excite the testes to the secretion of a rich and spermatozoated, and hence fecundating semen by the time she is willing to be lined. Wagner also discovered, that the semen of strong and vigorous men contained strong and vigorous spermatozoa; and that of weak and feeble men contained weak and feeble spermatozoa.*

Bischoff has suggested a new hypothesis, as to the office of spermatozoa in generation. He thinks that "by their energetic movements, they act as carriers of the liquor seminis, (as the corpuscles of the blood act upon the liquor sanguinis)" to the ovum; and that "the fluid portion of it is the material by which fecundation is effected," by being imbibed into the ovum, through the *Zona pellucida*, and acting chemically upon it. If this theory be correct, the experiments of Spallanzani upon the semen of the frog, do not disprove the necessity of spermatozoa in the process of fecundation. He deprived the semen of its spermatozoa, and applied it to the eggs *out of the body*, with a hair pencil, and found it prolific. In this case, the pencil performed the office of the spermatozoa according to Bischoff.

But whether we regard the spermatozoa as the prolific principle of the liquor seminis, and as entering the fissure in the *Zona pellucida* (as described by Martin Barry) and thus fecundating the ovum; or as carriers of the fluid portion of it to the ovaria; one thing now appears to be certain, that if the seminal fluid is deprived of them, or if they are weak and feeble, it loses its fecundating power; for if feeble even, they could neither act as the fecundating principle, nor as carriers of the semen to the ovaria.

It is highly probable then, that in the case of the woman *Clarissa*, her husband being a man of delicate constitution, the spermatozoa of his semen were too feeble to act their part in fecundating the second ovum; but those of the semen of the white man being more vigorous, effected this purpose.

According to the once popular but ridiculous theory of Leewenhoeck, which had even Charles II. of England as an experimental advocate, this mysterious case would be accounted for, by assuming, that each spermatozoa is a human being in miniature, "capable when developed of resembling the father;" that when they get to the ovaria, a regular battle royal takes place between them, during which terrible conflict the mortality is such, that they are all killed but one, and he takes possession of the vesicle,, and is developed in it,—that in cases of twins,

* I have never read Wagner's Phys.; but in a conversation with Prof. Samuel Jackson, of Philadelphia, in 1842, he informed me, that he had just read extracts from the work; and among other information from them, he gave me the above, of which I made a note at the time.

two spermatozoa survive; one taking possession of one vesicle, and the other of the other; and hence in the case above, only one survived at the time the first ovum was impregnated, and none in the subsequent discharges of the negro; and only one in the discharge of the white man, which having conquered took possession &c.

This theory, which represents us as little Sampsons, dealing out death and carnage to all around us in this Lilliputian state of existence, and ushering us into the world with our hands stained with the blood of thousands of our little fellow-creatures, to say the least of it, is too ridiculous and preposterous to be treated seriously.

I have extended these remarks much further than I intended; and forbear trespassing longer on your patience.

IV—*Excision of Diseased Mamma, under the influence of Letheon*,
By J. N. Batchelor, M. D., near Greenville, La.

The particulars of the following operation have been on hand for some time; but the paper was mislaid, and this must be our apology to the author for its non-appearance.—We publish the article, firstly, to award credit to Dr. B. for venturing to use the Letheon at a time when the profession had by no means agreed as to its efficacy or safety, and secondly, to warn the young Surgeon not to attempt an operation when ulceration is extending in a scirrhus or malignant growth, such as the one described below.—*Ed.*

On the 27th of June, 1847, I was requested (by Dr. Herbert) to see a patient of his (a Mrs. Low) living 14 miles south of Greenville and 28 miles from my house; I saw her on the 28th, in company with the Dr., his son and his partner, Dr. H. Cook. I found the patient above the ordinary size, (but much reduced) of a sallow exsanguine appearance, and suffering much pain from the tumor (malignant Ulcerative Mammary Sarcoma,) which she attributed to a blow received from the windlass of a well, while drawing water, about 12 months before. She first discovered the tumor about 5 months since—it was then about the size of a hazel nut, and $\frac{3}{4}$ of an inch to the right of the right nipple. At the expiration of some 16 weeks, Dr. Cook was consulted, he found it the size of an orange, he pronounced it malignant and advised immediate excision—the patient objected, saying that she was under the treatment of a conjuror who required three weeks to remove it; after this, it was treated with many local applications of an irritating nature, and among the rest a very strong ointment of Iodine. About the 20th week Dr. H. was called in, he found (from description) that the tumor was rapidly increasing, the top forming three cones, the superior surfaces of which were ulcerated, the whole surrounding integuments were intensely red and inflamed; he prescribed for her general system, and local applications to subdue the inflammation. On his next visit, (May 28th,) he was accompanied by Drs. Cook, Herbert and myself, I found the whole gland occupied by the tumor (but less below than above) and extending from the lower portion of the gland to the coracoid process and from the cartilage of the ribs to the anterior portion of the axilla, about 27 inches in circumference, rising upwards, and outwards form-

ing an immense mass, larger than a child's head, with a slough upon the superior portion of some 4 or 5 inches in diameter, with an irregular margin, and extending 1 or 2 inches in depth—(and very offensive.) I was informed by Drs. H. and C., that the Tumor had increased about one-third since their last visit with a more than corresponding increase of sloughing; integuments more inflamed but less so than before; her nervous system seemed much shaken and she had had a good deal of hemorrhage from the sloughed portion which had materially weakened her. She was of opinion that she could not live, but was exceedingly anxious that an operation should be performed; (after a consultation it was agreed that we should operate as soon as I could provide myself with an apparatus and Letheon.) She complained of slight chills and fever during the night followed by perspiration, and suffered much pain from the tumor. R. Quinine S. gr. viij Ferri S. gr. vj. Rhei. gr. x., Syrup Q. S. Ft. P. div. 6, to be taken during the day, (the Q. or R. to be increased as necessary,) and an anodyne at night, and the following application to the ulcer. R. Sodæ Chlo. pj. Aqua. pur. p. iv. m.; fever diet;—having provided myself with the necessary apparatus, we visited her on the 3rd June; on examination we found that nearly $\frac{1}{2}$ (half) the tumor had sloughed off, and the slough seemingly extending to the base of the tumor, the principal sloughing had been on the night of the 29th ult., accompanied by considerable hemorrhage, and high fever the night following, (she has had but little fever since) the quinine had been increased to 18 gr. per day. She appears blanched and weak, (though not perceptibly weaker than at our last visit.) Pulse 170 and small—and appears rather excited—(probably at the idea of the operation;) no enlargement of the glands perceptible throughout the system, and no other evidence of the disease than the tumor and the debilitated state of the system—yet so rapid was the increase of the disease that we were convinced that she could last but a few days, (in her present situation.) After fully explaining to the patient and her family the danger of the operation (owing to her condition and the situation of the parts) and she and her friends being still anxious for the operation to be performed, we agreed to it as the dernier resort. I then explained to her the use of the inhaler and how I wished her to use it—she said she would do her best, but that her cough would bother her, (she had had a slight cough for some length of time;) we then had her placed upon a slightly inclined plane facing the right—owing to the cough and the improper construction of the inhaler, I did not succeed in getting her under the influence of the æther until the expiration of half an hour. I then, at the request of Drs. H. and C., proceeded to operate, by making the lower incision from the lower portion of the gland around the *base* of the tumor, (the integument being in such a diseased state as to preclude my using it for the flaps as I could have wished) through the axilla and up in front to near the coracoid process—the opposite incision was made from point to point, (also around the base of the tumor, for the same reasons,) then dissecting upwards I removed the whole gland, and a portion of the Pectoralis Mj., that was engaged in the disease—(the loss of blood was not more than 8 or 10 ounces,) the vessels were numerous but small—the wound being washed and the vessels secured, the flaps were brought together and secured by two interrupted sutures

(in the lower third,) and adhesive strips, water dressing applied and covered with oiled silk. Owing to the diseased state of the integuments so large a portion was removed that the flaps could not be brought together by an inch and a half (at the upper and middle portion—she was removed to bed, she appeared a good deal exhausted. Pulse 105, and very small, skin blanched and covered with a clammy perspiration—Spts. of ammonia and wine were exhibited freely for an hour or more, when she revived—the spts. was given not only as a stimulant, but to relieve her from the effects of the æther (sickness at the stomach,) which still continued, though not to the extent of vomiting. She complained of a pain across the pelvis and sacrum, (she said as if her hips were falling apart,) which I attributed to her position and the time she lay upon a hard surface, (a door with only a counterpane spread over it;) during the first part of the operation the patient complained audibly, saying “Oh! Lord, I am going now, give me more of it,” (æther,) I immediately asked her if she suffered much? She replied, no, though there was slight evidence (muscular) of pain during the incisions; there was none during the remainder of the operation until the sponge was applied and it was then slight—during the latter part of the operation, the patient was more fully under the influence of the æther and was much sickened by it; she vomited once, though for a short time, and appeared sick for an hour after being put into bed; upon her recovering from the effect of the æther, I asked her (as did several of the gentlemen present, who were Drs. G. Herbert, C. Herbert, H. Cook, and Messrs. I. Henderson and Low,) several times during the 3 or 4 hours we remained with the patient, whether she had suffered much during the operation—she replied no, I don’t know when you done it, the last thing I remember was your putting that thing (inhaler) to my mouth—I asked her if she did not remember speaking to me during the operation—she said no, that she did not think she said any thing after she began to breathe that thing.

The success of the operation (it was successful so far as recovery from the operation, and life were concerned) and small loss of blood, is to be attributed to the ready and efficient assistance rendered by Drs. G. and C. Herbert, Cook and Esq. Henderson. Dr. H. remarked, that during the time that she was most fully under the influence of the æther, that her pulse increased almost beyond counting;—4 hours afterwards the pulse was 165;—there was 4 ounces of æther used during the operation. As Drs. H. and C. were to take charge of the case I did not see her until the wound was nearly closed; at this time the disease had again made its appearance, (near the edge of the wound under the upper edge of the sternum, and over left clavicle in small sub-cutaneous tumors); she had recovered flesh a little and strength, but still looked badly—she generally was cheerful and believed that she would recover, though at present she appeared anxious concerning the newly arisen tumors, and complained that one of them gave much pain—although I was convinced of the certain death that was approaching her, I could not but deceive her and bid her hope—this was the last time I saw her, and she died in weeks from the operation, and I am informed that as soon as the wound closed that tumors made their appearance all over the upper portion of her body;—at her death the largest one was in the left axilla and was near the size of a child’s head—in

all nearly 40 tumors made their appearance. The treatment after the operation was not noted or given to me, so that I cannot give it.—

Sorry the operator made no dissection of the tumor.—*Eds.*

5—*A Case of Perforation by Ulceration of the Transverse Colon.*

By WM. H. WINN, M. D., of New Orleans.

Francis Ward, an Irishman, rather delicate frame, ætat 22, occupation that of a ditcher, was admitted to the Charity Hospital on 18th April, 1848. He was, at this time laboring under a mild attack of typhus, and said he had been sick only two days, though this assertion was certainly contradicted by his general appearance. He complained of no pain or uneasiness, but referred his symptoms to a general debility and weakness—no unusual source of complaint with this class of patients, even when the symptoms are well marked and easily described. His general condition may be inferred from the following symptoms which presented themselves, viz: face slightly flushed; skin quite hot; eyes slightly injected; tongue very much furrowed, and coated with a hard, brown and dry deposit; some accumulation of sordes upon the teeth, and a pulse ranging at 85, and full.

All the symptoms soon disappeared under a tonic and slightly stimulant treatment. On the 21st he was allowed light nutritious diet and porter, and I supposed he would soon be able to leave the hospital and resume his duties; but on the morning of 22d he was found complaining of a very slight, uneasy sensation in the epigastric region; a feeling of fulness and tightness; and, upon an examination of the parts, a large tumor was discovered occupying the median line of the body, and immediately over the position of the stomach.

This tumor was about five inches wide at its base, considering the surface of the abdomen as such, of a perfectly formed globular shape, and remarkably well circumscribed by what seemed to be a firm and inelastic band, an inch in width. Its general appearance, location, and even its size, will, perhaps, be more readily comprehended, were we to imagine the recti muscles separated at a point, say about three inches above the umbilicus, remaining at the same time firmly attached to the ribs above, and the stomach distended with gas and protruding between the separated edges. This was, in fact, the precise appearance it presented, and, also, one of the conjectures as to where and what it was.

The walls of the tumor were very tense, thin and elastic, revealing upon percussion remarkable tympanitis, and when pressed inwards below the surface of the abdomen, fluctuation, though slight, could be detected, but quite impossible to locate the fluid. On its external aspect, there were no signs which would indicate any serious disease within; no redness; no pain upon pressure, or very little; no unusual degree of heat experienced either by the person examining or the patient himself; appetite remained good, and digestion, to all appearances, went on well; tongue clean and of a bright healthy hue; pulse 75, and full; skin cool and pleasant; bowels moderately open, having from one to three stools a day; urine free and healthy. These symptoms certainly speak well, so far as regards the good condition of the digestive organs, as well as a due performance of the functions of the secreting system.

And thus far, the mere presence of such a tumor in such close proximity to large and most important organs—in fact, occupying almost the very position of the stomach itself, affording no clue whatever of arriving at its origin, and scarcely permitting a warrantable conjecture, either of its exact location or precise nature, would, of itself, excite the curiosity and elicit the keenest interest both of the surgeon and practitioner. But that which increases our curiosity, and doubles the interest of the case, was the existence of other unnatural appearances of, and sounds proceeding from the chest: and in addition to these, the cavity of the pleura seemed in some strange way to be connected with the tumor, or with that portion of the abdominal cavity over which the tumor was situated.

The lower portion of the chest on the left side was very much distended, the intercostal spaces being raised on a level with the ribs. And here, too, percussion revealed symptoms in some respects analogous to those noticed, when applied to the epigastrium. Not the slightest doubt existed in my mind of the presence of air in the cavity of the pleura, so exceedingly tympanitic was all that portion of the thorax commencing at the upper part of the lower lobe of the left lung, and extending downwards on that side to the diaphragm. Auscultation over this part revealed nothing, save the entire absence of all respiratory murmur; nor did the lung seem to perform its office; yet, strange to say, at this time the patient suffered no dyspnea.

This is the precise appearance the patient presented on the morning the tumor was first discovered, and a change, if there was any going on, was so gradual and slow as to escape notice until the 29th, although the patient underwent a close examination every morning.

April 29th. He now complained of considerable tenderness, increased by pressure upon the right and lower side of the tumor, which showed a strongly marked inclination to point immediately in the centre. He was free from pain anywhere else; tongue indicating no inflammation of any organ, or of any of the viscera; appetite not quite so good; three small stools a day; pulse 120, and small; resonance of left side of chest much diminished, and replaced by a heavy, dull sound. Although the attempt had been repeatedly made, it was now, for the first time, that the peculiar splashing was heard, indicative of liquid in the pleura. Slight dyspnoea, and the face fast assuming a cadaverous hue and anxious expression. Various were the remedies resorted to for relief, but without the slightest perceptible benefit. Tonics and stimulants were freely administered during the whole time, and no doubt had sustained the patient up to the 29th; but they had now lost their influence, and death was inevitable. The idea of puncturing the tumor, at one time presented itself, but death would just as inevitably follow such a procedure, and, in all probability, be only hastened by it. This idea was, therefore, abandoned, and with it all hopes of human aid.

That the cavity of the pleura was connected with that portion of the abdominal cavity in which the stomach was situated, seemed almost evident, and, if with this portion, why not with the rest? These were questions which were not and could not have been answered at that time. Some very ingenious and plausible hypothesis were indulged in by one or two medical gentlemen who saw and examined the case; but the

case itself was so obscure, and the symptoms so contradictory that but one was sufficiently reconciled with his observations and reasoning to venture a decided opinion, and he, as usual, was correct in his diagnosis. (Dr. Stone.)

April 30th. Symptoms worse. Tenderness of the tumor so great as scarcely to bear the weight of a finger upon it; loss of appetite, with great emaciation; increased dulness over the left pleura, with diminished resonance; pulse 120, but growing weaker; treat. stim. and tonic.

April 31st. Still a slight change for the worse. Increased difficulty of breathing; pale and haggard; pulse 112, but weaker than yesterday; treat. same.

May 1st. Symptoms pretty much the same. Night previous vomited a quantity of dark green fluid. Treat. contin.

May 2d. Violent fit of coughing came on during the night; rest much disturbed by it; other symptoms the same; treat. syr. morph.

May 3d. Coughing ceased; breathing still labored and difficult; weak and exhausted; pulse 100, and very feeble; treat, brandy tod. tinct. cinchona. comp.

From this day the symptoms became worse, the patient weaker, and he finally died on 10th.

Post-mortem two hours after death. In attempting to remove the superficial integuments, or what there was left of them, (for they had wasted away very much,) the tumor was cut into, followed by the escape of a quantity of very foetid gas. It was quite difficult at first sight to say what was, and how affected, so jumbled up and agglutinated were all the organs and parts. But finally, the transverse colon was found very strongly adhered to the walls of the abdomen in its entire course across that cavity, dividing it into two distinct and separate cavities. The false cavity, bounded above by the diaphragm, and by the colon and its adhesions below, contained what there was of disease—surely sufficient to create a wonder how the patient survived so long. The large intestine was found perforated just at the upper part of the angle which it makes in forming the descending colon. The perforation was about the size of a dime—sufficiently large to permit the escape of the contents of the intestine, which had been poured out into this upper and false cavity. Finding an obstacle to its progress in every direction, save upwards, it had taken this, pushing the diaphragm with it, and thereby compressing the left lung into a space, which, in a natural position of the parts, would have been occupied by the upper lobe alone. The quantity of matter contained in this cavity would, I suppose, amount to two quarts.

The lungs were perfectly sound; the heart was likewise of the natural size, and in other respects healthy. Stomach perfectly sound, showing not the slightest trace of disease. The liver was somewhat enlarged and slightly congested. Spleen completely gangrenous, and easily crushed with the hand into a pulpy mass. The splenic artery was filled with hard coagulated blood, and more like something undergoing putrefaction than anything else.

All the organs below the adhesion of the colon were sound and healthy. The colon itself exhibited several patches of ulceration in its course

across the abdomen, but no other perforation was found than the one mentioned. The muscles and superficial integuments over the tumor were disorganised.

This is a plain and simple statement of facts, and any comment upon it by myself, I deem unnecessary.

VI—A Case of Strangulated Hernia, Oblique Inguinal. Operation by W. P. RUSE, M. D.

Porter, a mulatto boy of R. McQ.'s, æt. 17 had a hernial tumor for "six or seven years;" had frequently during that time been attacked with symptoms of strangulation, relieved by rest in horizontal position, bloodletting, &c., was on Tuesday, the 7th September last, seized with pretty active pain, &c., in abdomen, which gradually increased until Saturday the 11th, when the writer saw him, and endeavoured to relieve him by taxis, which was persevered in for several hours. Cold applied externally, V. S., tobacco-enemas, &c. These measures failing, the usual operation was performed, 4 o'clock P. M. next day, (12th.) The sack was about $4\frac{1}{2}$ inches in length, by 3 inches in breadth, and when opened at its inferior extremity, a large quantity of dark offensive serum gushed out. The sack itself was firmly attached to the integuments. The bowel (of which there was about five inches included) was of a dark color, but after spunging it off, was found to be *sound*. It had, owing to the degree of inflammation present, formed attachments at several points. These were broken up, and the stricture, (which was at the external ring) divided. It was found impossible to return the bowel, owing to the *permanent contraction and induration of the neck of the sack itself*, which required the *free* use of the knife. The bowel was returned, and the sack left *in situ*; wound closed; usual compress and bandage applied; $\frac{1}{2}$ gallon water, 80 degrees temperature, was immediately introduced into the bowels, and was soon followed by their free evacuation, giving entire relief. It was ordered by the writer that so soon as full and complete reaction occurred, that the patient be bled to syncope, and a constant stream of cold water (from a vessel suspended above him) to fall directly upon the wound. Pulse at this time, and before the operation, 150, small, feeble; skin cold and clammy.

13th, 4 o'clock P. M. Was bled to fainting last night; bowels have been moved naturally 3 or 4 times; fæces black and very offensive; pulse 128, irregular; skin warm and dry; tongue dry; thirst; continued cold water, and live on Slippery Elm bark.

14th, 4 o'clock P. M. Pulse 112; skin moist; tongue not so dry; continue same.

15th, 9 o'clock A. M. Tumefaction subsiding; pulse 104; tongue and skin moist; bowels regular; continue same.

18th, 4 o'clock P. M. Pulse 88; wound discharging rapidly; bowels regular; tongue and skin *natural*; secretion from kidneys has been unaffected from the commencement.

Oct. 3d. Well; truss applied, Hull's soft pad.

Lowndes County, Alabama.

Part Second.

REVIEWS AND NOTICES OF NEW WORKS.

I.—*Annual Announcement of the Jefferson Medical College.* (Philadelphia, 1848.)

"As a Southerner we cannot pass without comment the second paragraph in 'the annual announcement of the Jefferson Medical College,' because it is evidently a renewal of the attempt made in the same and other quarters to undervalue local advantages which we feel that we, in common with our Southern and South-western brethren, undoubtedly possess in the study and treatment of malarious diseases. The passage is as follows:

"The idea that a Student of Medicine must be taught his profession in the very locality in which he is destined to practice it, is now generally, as it ought to be universally, abandoned. It must be admitted that the great principles of the science are the same every where, and that the Student ought, for his own sake, [for the sake of the Jefferson Medical college?—*Ed.*] to seek for information wherever it can be best and most readily obtained."

We deny that the "idea" alluded to is "generally abandoned." Such may be the case at the North, but it is certainly not so at the South. Whilst we readily admit "that the great principles of the Science are the same every where," we do not think that any Southern practitioner will "admit" that a physician who has never seen our marshy districts, our malignant intermittent, remittent, congestive and yellow fevers, nor our negro peculiarities, can be very well prepared to practice in our latitude, and still less to teach others to do so. The "great principles of the Science are the same every where," but their application are as various as the races of man and the differences of climate. Would any one "admit" that the symptoms and treatment of the Plague can be taught as well in Philadelphia or in Georgia as in Cairo? The "idea" would be preposterous. Let the Southern practitioner look into the numerous works upon the Practice of Medicine published in Europe, or at the North, and he will at once perceive that they contain but little of value in the management of our fevers.

We will close these remarks with the candid confession of a gentleman who stands deservedly high in his own State as a man of erudition, and one eminently qualified as a teacher. "I confess one thing which I can hardly be blamed for, that, as a Northern man, and living in a region wholly free from malaria, I should not, and probably did not appreciate fully, such of the medical literature of the region where malarious fevers forms one of its chief staples." Such is the language of Prof. Oliver Wendell Holmes, of the Harvard University.

The above remarks are copied from our contemporary, the "Southern Medical and Surgical Journal," for September. We believe this is not the first time that this onslaught on Inductive Philosophy has been attempted in the same quarter. As there cannot, of course, be interested motives in the reiteration of such doctrines, we must take for granted that its authors believe in its truth, and will, therefore, proceed to examine into its merits.

The history of every science shows that what are termed its *laws*, or *first principles*, have been discovered by long and patient toil, unceasing industry and the co-operation of many minds and hands. These *first principles* are, indeed, nothing more than generalizations of the mind,—the reference, in short, of a multitude of separate facts to some common circumstance or circumstances, wherein they agree. Thus, that *bodies combine in definite and equivalent proportions*, is an established and well-known principle in chemistry, but who does not see that it is but the expression of a general fact, reduced from a multitude of particular facts.

Slow and painful, therefore, must be the establishment of general principles; for the harvest of facts has first to be gathered in. But not only so, most vexatious in the extreme must sometimes be the pursuit of science; for we cannot rest content with naked facts—it is our instinct to generalize these facts and classify them under a common head, and this classification is legitimate, as distinguished from hypothetical theory. Theorize, then, we must; but as science advances, new facts are discovered, which cannot be made to harmonize with our preconceived principles: it then becomes evident that we have by no means arrived at *first principles*—our theories must be abandoned, and we must go on laboring until a sufficient number of new facts enables us to enlarge our circle and embrace the whole, if possible, in a single principle. Could such a result be attained, we should have a perfect science; but such as yet does not exist. Even Astronomy, the one nearest perfection, has many problems yet unsolved.

Now, it is evident from what has been said, that in the application of these rules or principles, to particular phenomena or cases, there is great danger of committing errors, not only from want of skill in him who applies them, but in the inapplicability of the principles themselves. If in framing them, the very facts to which we apply them have not been taken into consideration; if in any essential particular the facts are *new* facts, our principles are at fault—they will not square with the phenomena, and we shall assuredly commit error, and, it may be, mischief. The savage who, acquainted with black-sand, rashly infers that every thing black and granular is of the same nature, and is thus led to throw a quantity of gun-powder into the fire, acts precisely like the philosopher when he applies his principles to facts not comprehended in their sphere. "There is no theory," says Dr. Thomas Brown, "which, if applied to the explanation of *new* phenomena, is not, to a certain degree, conjectural; because it must proceed on the supposition, that what was true in certain circumstances, is true also in circumstances that have not been observed. It admits of certainty in the very circumstances observed, only when it is applied to the very substances observed; in which case, it may be strictly said to be nothing more than

the application of a general term to the particulars, which we have before agreed to comprehend in it. Whatever is more than this is truly hypothetical."

Such, then, is the manner in which *first* principles (as they are often ludicrously called) are formed; and such is the law of their application to phenomena—the deduction being perfectly true if the facts are themselves included in the principle, and altogether false, if they be not so included.

And now let us turn to the assertion contained in the "announcement" "that the great principles of the science are the same every where, and that the student ought for his own sake, to seek for information wherever it can be best and most readily obtained."

The latter part of this sentence would require no particular notice, were not an insinuation contained in it, that Philadelphia was the very place and the Jefferson Medical College the very *especial* place where of all others information 'could' be *best* and *most readily* obtained. Pray, how many hospitals has the faculty of the Jefferson College at command? How many clinical lectures a week? How many subjects for anatomical purposes to each student? How many patients has the student access to during a session? How many surgical operations performed? We shall not institute comparisons but they must be made and the truth established before we can admit the claims of this Institution.

"It must be admitted that the *great principles* of the Science are the same everywhere." This is undoubtedly true;—the great principles of every Science are the same every where. But it is one of those truths that are exceedingly apt to have the force of falsehood. The applicability of these principles will depend, as we have already observed, upon the fact whether the principle to be applied has been deduced from phenomena similar to those to which they are applied. These 'Great Principles' are nothing more than certain generalizations, expressive of the progress which the science has made, and the state to which it has arrived. No one we presume will contend that the "Great Principles" of Medicine have any pretension to having reached perfection, and that they are few in numbers undisputed and indisputable. It is obvious therefore that this proposition is true in one sense, and false in another. It is true, for instance, if the "great principles" of medicine framed by a sound induction from facts in Philadelphia are applied to similar facts in New Orleans; it is utterly false, however, if applied to other facts in New Orleans which were not comprehended in the framing of the principles.

But we should have been informed what is meant by the "Great Principles of the Science"—If the meaning to be conveyed, be that Anatomy and Chemistry—(*materials* physical and mental being the same)—can be learned in one place as well as another, the proposition is true. So also with physiology, and materia medica; and so also too with regard to surgery and midwifery considered apart from the practice of medicine. Mere Lectures on the theory and practice of medicine may also be thrown in. But if the meaning intended, be, that the *practice* of medicine can be learned by the acquisition of such knowledge, or that a person qualified to practice in one locality is qualified to practice in all others, it is unquestionably untrue.

The student may have become thoroughly acquainted with all the branches we have enumerated;—he may have studied the structure of the human body until he is as familiar with it as an artizan with a piece of machinery;—he may have brought himself up to the existing state of chemistry;—he may be thoroughly acquainted with the “principles” of physiology, *materia medica*, surgery and midwifery;—and he may have listened to the most able lectures the world can produce on the theory and practice of medicine;—he may have received his diploma with éclat, and yet after all these acquisitions, and having not something more he is no physician.

The desideratum to which we allude;—the *sine qua non*;—the goal to which all his preliminary studies are but helps, is experience—“*experientia docet*.”

This experience should be imparted while the student is still under the hands of his tutors. If not so imparted, it is what he must learn when armed with his diploma he casts himself abroad upon society;—and the price which society usually pays for his education, it is any thing but pleasant to contemplate.

We are told by one of the most eminent professors in Philadelphia, (Dr. Jackson)—of the condition of things in this respect in this self-styled, so favored region.

“It is well for our profession that the *dogmatic* style of teaching is passing away, and the *demonstrative* taking its place. But a difficulty lies in the way of a thorough change—there must be hospitals for demonstrative teaching, and unfortunately there are but few of these in this country in which clinical instruction can be made available to the student. In Philadelphia, he said, they experienced this evil to a great extent. The people have inadequate conceptions of physic, and do not properly second the efforts of physicians to advance the profession. They conceive that the only business of the physician is to find out remedies for diseases, and hence they give no countenance to human dissections, and do nothing to promote the study of clinical medicine. The people—our municipal authorities—must first find us hospitals, and then we can make our teachings demonstrative.”

The value of clinical instruction consists in this, that the student learns without responsibility and hazard to the patients, that which, unfortunately most physicians learn in after life. It is in fact teaching the *practice* of medicine; by it, is gained that experience and thorough education of the senses, which all the best written books in the world, and the best delivered discourses in the Lecture-room can never impart. It is the medical education of the eye, of the ear, of the sense of smell, taste and touch. These must yield the materials from which the mind is to form correct inferences in the practice of medicine.

And now it must have become quite evident why the student should be taught his profession in the locality (or as near to it as possible) in which he means to practice. He should be so taught, because he will acquire as a student that *experience*, which otherwise he will have to acquire as a physician.

The truth of this proposition can only be contested by an assertion as desperate as untenable—It is this: that climate has no influence upon the human system; in other words, that diseases are the same every where—in short that the cold winters, and hot summers of Pennsylvania, produce no effects different from those produced by the long continued

heat of the southern States—that the miasms emanating from her long cultivated and dry lands are identically the same with those emitted from the new lands—swamps and savannahs of the South. That the manner of living and habits of the people likewise have no influence—all this may be believed “when the dog-star rages.”—Were it not contrary to every day’s experience, it would be so to the simplest law of nature, namely that different causes produce different effects—we may believe it when the alligator basks in the waters of the Delaware, and her banks are clothed with the cotton plant and sugar-cane.

If then, to bring this argument to a close, climate *does* affect the human system, and if there be such things as endemic diseases, and modifications of disease in different localities, the “great principles” of medicine taught in Philadelphia, will *not* apply to the diseases of the South—or be only applicable imperfectly, and so mischievously—admitting that the student has received the benefit of clinical instruction, his observations have been made on cases very different from those he will be called upon to treat, so that in the south he will find himself deficient in that education of the senses which observation alone can give and which is so necessary in the treatment of diseases arising from poisons. But not only this, the very principles which he would apply to our maladies are defective, as they have been deduced principally from the consideration of purely inflammatory diseases, whereas mostly those of the South are complicated with the poisonous effects of malaria. And

Note.—In Macaulay’s *Miscellanies* (Vol. v. p. 390,) there occurs a passage so apt to the matter in hand that we will here append it. We recommend it to the serious attention of those who believe that the “great principles” of medicine can be applied every where with equal facility and success. However, we suppose it will be with them, as with all who have travelled the “high *a priori* road;”—“*tant pis pour les faits*, if they don’t square with their principles.

“If on the other hand we are to deduce the theory of government from principles of human nature, in arriving at which principles we have not taken into the account the manner in which men act when invested with the powers of government, then those principles must be defective. They have not been formed by a sufficiently copious induction. We are reasoning from what a man does in one situation, to what he will do in another. Sometimes we may be quite justified in reasoning thus, when we have no means of acquiring information about the particular case before us, we are compelled to resort to cases which bear some resemblance to it. But the most satisfactory course is to obtain information about the particular case; and whenever this can be obtained, it ought to be obtained. When first the yellow fever broke out, a physician might be justified in treating it as he had been accustomed to treat those complaints which, on the whole, had the most symptoms in common with it. But what should we think of a physician who should now tell us that he deduced his treatment of yellow fever from the general theory of pathology? Surely we should ask him, whether, in constructing his theory of pathology, he had, or had not, taken into the account the facts which had been ascertained respecting the yellow fever? If he had, then it would be more correct to say, that he had arrived at the principles of pathology partly by his experience of cases of yellow fever, than that he had deduced his treatment of yellow fever from the principles of pathology. If he had not, he should not prescribe for us. If we had the yellow fever, we should prefer a man who had never treated any cases but cases of yellow fever, to a man who had walked the hospitals of London and Paris for years, but who knew nothing of our particular disease.”

so it comes to pass that the young practitioner, migrating to the South has first to unlearn many things thoroughly believed in, and then set about to acquire that knowledge which he should have derived from clinical instruction when a student.

J. H.

II.—*Annual Circular of the Medical Department of the University of Louisiana*,—Session of 1848-'49.

From this "Circular" just issued by the Faculty, we learn that the course of Lectures for the approaching session will commence on the third Monday in November and continue four months. The Faculty have wisely postponed the time for the beginning of the course until the middle of November, because at this late period students and other strangers may visit the city without fear of contracting our summer or autumnal fevers.

We extract the following remark from the "Circular," to the truth of which we can bear testimony:—

"In practical surgery, as in medicine, the advantages of this school are pre-eminent. We are satisfied, from the best authority and personal observation, that the Professors of no school in America are connected with and enjoy the unlimited use of an institution, in which there are so many interesting cases requiring surgical and medical aid, and the use of the knife, as are to be found in the New Orleans Charity Hospital. This great receptacle for thousands of the afflicted of every clime, supported bountifully by the contributions of the State, where there are no paupers but strangers, and attended gratuitously by the members of a profession who are the high priests of philanthropy throughout the civilized world, is opened wide to every aspirant who seeks knowledge among its tenants."

The new College edifice, now nearly completed, is at once a magnificent pile and an ornament to the city. It will be commensurate with the other advantages that this city presents for acquiring a practical and thorough knowledge of medicine. The number of matriculants for the last session, we learn from the circular, was one hundred and sixty-three, of whom thirty received the degree of M. D.—Twelve of the thirty States were represented in this school, the past session,—as also Canada and Cuba.

The prospects of the school are highly flattering.

A. H.

III.—*A Dispensatory or Commentary on the Pharmacopœias of Great Britain (and the United States;) comprising the Natural History, Description, Chemistry, Pharmacy, Actions, Uses, and Doses of the articles of the Materia Medica.* BY ROBERT CHRISTISON, M.D., V.P.R.S.E., President of the Royal College of Physicians of Edinburgh, Professor of Materia Medica in the University of Edinburgh, and Physician to the Queen of Scotland. Second Edition, Revised and Improved, with a Supplement containing the most Important New Remedies; with copious additions, and 213 Illustrations. By R. E. GRIFFITH, M. D. Philadelphia, Lea & Blanchard, 1848.—pp. 1008.

The reader can gather quite a correct idea of the scope and aim of

the work from its title. It differs from "The United States Dispensatory," by Wood and Bache, in several respects. In the department of therapeutics it is not so full and satisfactory; nor does it enter more fully into the chemical habitudes of medicinal agents.

The plates, illustrating the vegetable and other kingdoms of nature, employed in the treatment of disease, are very beautiful, and give pretty distinct ideas of their physical forms.

The book occupies a middle ground,—between a Dispensatory proper and a Pharmacopœia; it is, therefore, useful at once to the compounder of medicines, the pharmacist, and the general practitioner.

The author, Professor Christison, of the Edinburgh University, has long maintained a high rank both as a Chemist and Physician, in this country and in Europe.

As a work of reference, it abounds in useful information, and should occupy a conspicuous place in the bureau of every physician and apothecary who may be desirous to learn pharmacy and materia medica.

A. H.

IV.—*A System of Human Anatomy, General and Special.* By Erasmus Wilson, M.D., Lecturer on Anatomy, London. Fourth American, from the last London Edition. Edited by PAUL B. GODDARD, A.M., M.D., Professor of Anatomy and Histology in the Franklin Medical College, Philadelphia. With 251 Illustrations, by Gilbert. Philadelphia: Lea & Blanchard, 1848.

This work first saw the light in 1840, under the modest title of "*Vade Mecum*;" and the fourth London edition still retains this name; but the American Editor, in order to do justice to the high claims of the author, as well as the merits of the work, very properly designates it a "*System of Human Anatomy.*" To have passed through four editions, both in England and in America, in less than eight years, is the best proof of the intrinsic value of the work, and therefore requires no further commendation from us.

It is by all odds superior to any thing yet published on anatomy, and is at present, we believe, the text-book in all our medical schools and colleges. The plan upon which the work was conceived is admirable, and the subject is greatly simplified and brought within the comprehension of ordinary minds. From this work one may acquire a respectable knowledge of anatomy without dissection; and this is the only objection that can be urged against the book. No tedious description of unimportant parts, or elaborate efforts at an unnecessary display of learning encumber the pages of Wilson's "*Human Anatomy.*" The plates, though small, are correct, and will aid the student greatly in acquiring a knowledge of particular parts of anatomy. The American Editor, who to a thorough acquaintance with anatomy, adds that of all the sciences, is entitled to much praise for his part of the labor.

The Publishers have our thanks for a neat copy of this work, through Mr. White, Canal Street, who has the book for sale.

A. H.

V.—*The Ohio Medical and Surgical Journal*. Edited by JOHN BUTTERFIELD, M.D., Professor of the Practice of Medicine in the Starling Medical College. Columbus, Ohio, September 1848.

This is a very handsome bi-monthly, of ninety-six pages; and the first number contains five or six valuable original articles, besides critical notices, and a fair proportion of American and foreign medical intelligence.

The Editor seems quite familiar with his duties, and writes with much power and perspicuity on the various subjects falling within the scope of an editor's pen. We think, from the specimen before us, the *Ohio Medical and Surgical Journal* will prove quite an accession to the list of American Medical Journals.

We shall rejoice at the success of this new periodical, and cheerfully enter it upon our exchange list. The work is issued every second month, at Two Dollars per annum, *in advance*. A. H.

VI.—*Valedictory Address to the Medical Class and Graduates of the Indiana Medical College, (Laporte University,) February 19th, 1848*. By A. B. SHIPMAN, M.D., Professor of Surgery in said Institution.

This Address embodies much good sense, and some excellent suggestions on medical ethics. Professor Shipman is already known to us by his writings, and this last Address, conceived in good taste, and expressed in neat and generally choice language, has given us a yet higher opinion of the sound judgment of the author no less than of the elevated moral feelings of the man. His advice to the young graduates, who were about to enter upon the active duties of life, is exceedingly appropriate, and we trust those to whom it was addressed will remember it, and carry it with them through life. Had we space, we would gratify our readers with some beautiful extracts. We thank Professor S. for his Address. A. H.

VII.—*A Practical Treatise on the Diseases of Children*. By J. FORSYTH MEIGS, M. D., Lecturer on the Diseases of Children in the Philadelphia Medical Association; Fellow of the College of Physicians, Philadelphia. Lindsay & Blakiston, Philadelphia, 1848. pp. 575.

This is the third number of the "Medical Practitioner's and Student's Library," just issued from the press of Messrs. Lindsay & Blakiston of Philadelphia.

We have already spoken freely, but not favorably, of some of the preceding numbers, and, however much we may regret the necessity of condemning the work, yet as a firm determination to discharge our duty as journalists, forces upon us this latter alternative, we are resolved to express our candid opinions without fear or affection; because if an author, like a politician, thinks proper to thrust himself before the world, he

must abide its verdict. Dr. Meigs has certainly written a book, and no small one either; now let him plant a tree, and he will have done something for his country.

In his "Preface" our author thus delivers himself: "In the composition of the work the author has availed himself, as fully as possible, of every authority of importance placed within his reach, always, however, endeavoring to judge what came under his notice, by the knowledge derived from his personal experience in private practice." If we are not mistaken, Dr. Meigs is quite a young man, and his "personal experience in private practice" cannot, in all probability, be very great; if so, he has been much more fortunate than many young practitioners, we wot of in Philadelphia. It is well enough for an author to appeal to his personal experience, either in refutation or confirmation of any particular mode of practice; but of what avail is the experience of a single individual when arrayed against the combined and recorded testimony of thousands? Experience does not always teach correctly: two men may witness the same phenomena under similar external circumstances, the one having superior analytical powers, will classify and arrange such phenomena, according as they may agree or differ from other phenomena or facts; whilst the other, less gifted, may, through misconception, both of the nature and bearing of such facts or phenomena, pervert or misapply them. We leave it to the readers of the work to decide whether Dr. Meigs belongs to the former or latter class of observers.

We talk of the "*diseases of children*" as if, like the adult, they had not a liver to suffer from a hepatitis—lungs to be afflicted with a pneumonitis, and bowels to be tortured with an enteritis, etc., etc.; and we maintain now, as heretofore, that, with but few exceptions, the child and the adult are visited by the same diseases, and require, with slight modifications, the same general plan of treatment. Yet the profession is slow to acknowledge this, as we honestly conceive, important truth. The physician is sometimes asked if he understands the diseases of children; and this question from the unprofessional, shows that the public has likewise imbibed erroneous views on this subject.

To enable the reader to understand the great value of Dr. Meig's practical experience, we shall transcribe the particulars of a case described in his chapter on "acute hydrocephalus." Speaking of the beneficial effects of free bleeding in this disease, Dr. Meigs says: "A very stout and hearty girl, aged 12 years, was seized with malignant scarlet fever, of which she was extremely ill from the 3d to the 9th day. She then improved somewhat, but on the 12th day was attacked with general anasarca, accompanied, however, with severe nervous symptoms, and which nearly disappeared on the 16th. From the 21st to the 25th day she did very well. There was merely slight oedema of the face, and she sat up the greater part of the day in good spirits. At eight o'clock, on the evening of the 25th day, as she sat in an arm chair taking her tea, she said suddenly to her sister, 'There is some one sitting on my arm,' and her sister saw that on endeavoring to take hold of a tea-spoon, the hand no longer obeyed the will. Her speech then became mumbling, and she fell back in a slight convulsion. I saw her," continues Dr. Meigs, "in half an hour from the beginning of the

attack, and found her unable to speak, almost insensible, and slightly convulsed. I bled her immediately to the amount of *twenty ounces* from the arm, applied cold to the head, gave a purgative enema and ordered a cathartic dose of calomel and jalap. In a *few moments after* the bleeding she was attacked with terrific general convulsions (not surprising!). The bleeding was repeated in half an hour, to the amount of at least *sixteen ounces* more, but *without any effect*.

"The convulsions continued, with very slight intermissions, for ten hours, when they ceased, and were followed by *profund coma*, and death in twenty-three hours from the onset of the *nervous symptoms*."

We have *italicised* that portion of this truly extraordinary case, to which we beg to invite the attention of the reader.

Has Dr. Meigs read Hall on the loss of blood? Has he examined the paper recently published by Dr. Beck, of New York, on the effects of blood-letting in children? If so, we think he has read them to little purpose; as he seems to have forgotten the valuable lessons which they inculcate. Shall we refer him to the experiments of Majendie and others, by which it has been demonstrated beyond a reasonable doubt, that free and repeated bleedings of animals produce serous effusions both in the brain and pleural cavities? With such facts staring our author in the face, he, nevertheless, tells us that from a little girl, 12 years of age, who had suffered from the 3d to the 25th day, with a *malignant* attack of scarlet fever, attended with severe "nervous symptoms, general anasarca, œdema," etc., he took 36 oz. of blood in the course of half an hour, simply because the little creature, in a state of partial syncope, perhaps superinduced by slight serous effusion upon the brain, the result, in all likelihood, of metastasis,* exclaimed to her sister—"there is some one sitting on my arm!" Poor soul! little did she dream that this exclamation would cost her 36 oz. of precious blood!—that blood of which she stood so much in need. "In twenty-three hours from the onset of the *nervous symptoms*," says Dr. Meigs, "profound coma and death followed, in spite of two spoliative bleedings." Were not the two bleedings, one of 20, the other of 16 oz. practised, too, on a little girl 12 years of age, who had already laboured under malignant scarlet fever for 22 days, sufficient to produce terrific general convulsions, coma and death, in an attack characterised as "*nervous*!" Let the author himself answer this question, for we have not the hardihood to do it for him.

We would fain hope that our author, as well as others who may read the particulars of the preceding case, will learn a valuable lesson from such "personal experience;"—*ex hoc omnes discunt*.

Let not the reader suppose from the preceding remarks, that we are disposed to be hypercritical; we, in glancing over the work, commenced reading at page 367, and this single case arrested our attention; we have commented upon it, and leave it to the author himself to determine whether we have or have not done him justice. A. H.

* The cerebral symptoms, in this case, were caused probably by an excess of urea in the blood; as the œdema, general anasarca, etc., demonstrate serious lesion of the kidneys. It has been long known that a superabundance of urea in the circulating fluid, frequently one of the sequelæ of *scarlatina*, will produce serious effects upon the brain, such as serous apoplexy, symptoms of hydrocephalus and epilepsy.

VIII.—*Lectures on the Theory and Practice of Physic.* By JOHN BELL, M. D., Member of the American Med. Association, and of the Med. Soc. of the State of Pa.; Fellow of the College of Physicians of Philadelphia; Member of the American Philosophical Society, and of the Georgofili Society of Florence, etc. etc.; and by WILLIAM STOKES, M. D., Lecturer at the Medical School, Park Street, Dublin: Physician to the Meath Hospital, etc. etc.—Fourth edition, revised and enlarged. In two volumes; Ed. Barrington and Geo. D. Haswell, Phila., 1848.

In a comparatively short time these "Lectures," as they are called, have gone through the third and reached the fourth edition,—certainly gratifying proof to the authors that they are acceptable to the medical public. This edition of these lectures has been revised by Dr. Bell, and many of the subjects embraced in former editions reviewed and otherwise retouched and remodeled. In the present edition, Dr. Bell tells us he has introduced for the first time, the following subjects:—*Diseases of the Eye—Diseases of the Blood-vessels and Dropsy.* Besides he has availed himself of the present advanced state of pathological anatomy—of organic chemistry,—microscopy and histology, to enhance the value of the work.

Dr. Bell contends for the identity of *Typhus* and *Typhoid* fevers,—believes them to be different degrees of the same disease, but that there is no specific or organic difference between them. To us, it has always appeared to be aiming to make a distinction where there was but a trifling difference; nor is it of much consequence whether they be regarded as one and the same disease or different diseases; in both, the symptoms, if not the same, are at least closely allied and the lesions after death will differ about as much as the symptoms during life. The treatment is nearly the same for both affections.

These lectures afford abundant evidence that Dr. Bell is no drone in the profession—that he watches with no ordinary interest the progress of medicine, and its kindred branches,—that he reads the Journals of this and other countries, and gathers knowledge wherever it may be found. To embody such information in the form of a book, is no trifling undertaking, for to say nothing of the mere labor, it demands a sound judgment, and much practical information; we therefore leave it for the reader to determine, after examining the Lectures, whether Dr. Bell has, or has not performed his task with ability and judgment.

We feel authorized to say, however, that this edition is greatly superior to those that preceded it, and have no hesitation in pronouncing the work a good one.

We are indebted to the *Publishers*, through Mr. White of this city, for a handsome copy of the 4th edition.

A. H.

Part Third.

EXCERPTA.

I.—*Ideas on the Molecular Grouping of Organic Combinations.*

Among the different opinions which have been put forth with some success concerning the molecular grouping of organic compounds, two, particularly, have contributed to lead science into error and have been obstacles to the development and pursuit of truth. These are metalepsy with its chemical types, and the doctrine of poly-basic acids.

Each of these doctrines offers, for certain phenomena of organic chemistry, a superficial and easy explanation, which, when regarded not only as a last resort of giving an account of unintelligible phenomena, but as a theory which demonstrates the intimate relations of these phenomena, prevents all further search after truth:—for he who embraces it from conviction, believes that he has grasped a clear explanation of the rational state of things, when, in truth, the idea he has conceived is purely empirical. It is a kind of scientific faith, and like all faiths, it possesses its enthusiastic apostles who preach it at every turn. The chemists who have gone farthest in this respect, are *M. M. Gerhardt* and *Laurent*; but they twist about, and reform scientific notions in so different a way, that *their* efforts must indubitably produce the conviction that they are not in the right path. In the meantime, the deduction *ab absurdo* is not the argument to which it is best to have recourse to when we can do otherwise. Consequently, let us let enthusiasts pursue their own road as they best can, and let us ourselves follow a better path.

Some of the chemists who had embraced metalepsy and its types with more or less conviction, have already begun to perceive, that it disguises the true state of things, without explaining them. Thus, in the preceding Report, p. 477, we saw that Mr. *Cahours* had discovered that in the substitution of hydrogen by chlorine and bromine, certain atoms of hydrogen are easily exchanged,—others are so with difficulty, and that there are yet others, which cannot be substituted at all;—that consequently there must be a difference in the manner in which these last exist in the combination. This experiment is such as may enable us to look more sharply through the veil which metalepsy and theory of types spreads over the molecular grouping of organic combinations, and so far we must give credit to Mr. *Cahours*.

M. Millon, on his part, has presented some views on the molecular grouping of organic nature. He has observed that some organic combinations have a tendency not to obey immediately the laws of combination to which inorganic compounds are subjected. It is known, among others, that the combinations of oxide of ethyle, of oxide of methyle, of oxide of amyle, etc. etc. with acids, may be mixed with caustic alkalies, without the alkalies immediately combining

with the acids, which ought to occur if the base were purely inorganic. In the second place, he also observed that the hydrogen, of compounds of hydrogen and carbon, was with difficulty separated from the carbon when these hydro-carbons were treated with metallic salts, whilst hydrogen is oxidated with the greatest facility, when combined with sulphur, selenium or phosphorous.

This phenomenon is, in fact, very remarkable. The cause is unknown; it is evident that it must reside in some circumstance of the molecular grouping; but we cannot conceive what that circumstance can be. It is not alone the chemical affinity which is inactive within the limits of ordinary temperature, for the electric current, even, exerts no action on these combinations, such as tends to separate the elements, nevertheless, this phenomenon is not peculiar to organic combinations;—it also presents itself in some inorganic combinations; such as the dithyonates,* the trithyonates, and the tetrathyonates, which, mixed with acids more powerful, are only decomposed slowly and with the assistance of heat, and this must be owing to a similar cause.

In relying upon the example of carburetted hydrogen, the hydrogen of which is not oxidized by metallic salts, M. Millon believes that the cause of it is, that in inorganic combinations, the atoms are in juxta-position, whereas in organic combinations they interpenetrate;—that it is the carbon, (exception being made to all the other elements,) whose atoms possess the property of being penetrable, and that it is on that account that there exists no organic combination without carbon. M. Millon seems not to have thought of the incompatibility of the idea of penetration with that of atoms, and that consequently the explanation is but an illogical hypothesis.

Here, according to M. Millon, are the consequences of the penetration of the carbon. "This element, associated with the other elements, forms with them a compound which acts no longer by its different constituents but as a whole. It is, as it were, a new body which assists in the production of organic beings. By an abuse of this arrangement some chemists have been led to frame a multitude of hypothetical bodies, formed by the union of carbon with nitrogen, hydrogen and oxygen. The name of *radicals* have been bestowed on these substances which are for the most part imaginary, and they have been thus made to play, almost always in spite of reactions, a very strange part, whilst it would have sufficed in these different cases, to have pointed out the perfect union of many organic elements and to indicate, in accord once with the phenomena, their general tendencies of combination or decomposition. Carbon, thus penetrated by other elements, enjoys a peculiar stability. The organic molecule opens to the substitution, but permanency is found in the number."

It is on this foundation, placed upon moving sand, that M. Millon has constructed a kind of theory of types, which comprehends but a very limited number of types, in the supposition that the multitude of possible exchanges rests upon the fact that the type persists even when hydrogen and oxygen in the proportions to form water are abstracted, or when carbon and oxygen in the proportions to form carbonic acid, or when both at once are abstracted. Mr. Millon gives a small number of examples, but he has entered into no details, and I think what I have just described may suffice to enable us to form a correct judgment of this opinion concerning the mode of combination in organic substances.

As for a long time so many opinions have been advanced—all different from each other, and as metalepsy and the theory of types which have had the

* Dithionates, etc.—These are salts of acids composed of Oxygen and Sulphur; there are 4 of them—viz:

Pentathionic acid— $S^5 O_5 = S^4 O_5 + S$.

Tetrathionic acid— $S^4 O_5 = S^4 O_5 + S$.

Trithionic acid— $S^3 O_5 = SO_3 + SO_2 + S$.

Dithionic acid— $S^2 O_5 = SO_3 + SO_2$.

most success have not been able to accord, the time may have arrived for us to agree upon, at least, the true direction which may offer some probability of our approaching the object aimed at, even if we shall not be able to attain it.

It seems so evident that we shall not attain our object by imagining intimate relations and the laws which govern them, that we might not comprehend how a single investigator could adopt such a method, if it were not so easy to abandon one's self to the caprices of the imagination and so tempting thus to dazzle the multitude, which takes no trouble to examine the solidity of the edifice, and in its amazement renders homage to what it considers the bold sallies of genius.

But truth must be painfully sought in reality—and no one finds her without submitting the results of his labors to a severe and minute examination. He who seeks for truth by giving free wings to his imagination will be constantly drawn into paths which do not conduct to her.

For a long time I have endeavored to call the attention of chemists to the immutability of the laws which preside over the combinations of the elements, and to the fact that they are applicable to organic as well as to inorganic nature—which, besides, is contested by a very few persons. As to the manner in which organic and inorganic nature conform to these laws, we know that certain accessory circumstances intervene, and that they are more numerous in organic nature, in which even their mode of action is often unknown, so that the products of combination offer a very great diversity. But in order to discover the manner in which the elements combine with each other, we should begin with the most simple compounds of inorganic nature, and rest ourselves upon what is known in order to search out the unknown. By employing as guide the laws of combination in inorganic compounds, we shall discover more and more the mode of combination in organic substances. I have given my ideas on this subject in various places in many Reports anterior to this, and have collected them in a Memoir which I lately communicated to the Academy of Sciences of Stockholm, with the purpose of endeavoring, if possible, to give a more judicious direction to the efforts which we made to arrive at something decisive. I will here give an extract as short as possible.

When we examine the mode of combination of inorganic substances, it is found that hydrogen and nitrogen, as likewise carbon and nitrogen, combine together to form peculiar bodies, *ammonium* and *cyanogen*, the first of which unites like a metal with mercury, sulphur, and oxygen, and these combinations with sulphides* and acids, exactly like sulphurets and the oxides of an alkaline metal; and the second of which, acts like a simple halogen body, combining with metals to form salts, and with oxygen to form acids, we have here three examples of inorganic compound bodies which enjoy the properties of elementary substances: that is to say, we have models of compound radicals. Hence has naturally resulted the idea of the possible existence of compound radicals; and as we possess acids as well as bases which are composed of carbon, hydrogen and oxygen, we have been led to admit that the acids and bases of organic nature must also have radicals composed of carbon and hydrogen, or, of carbon, hydrogen and nitrogen. In course of time, we have in fact succeeded with some of them in combining them not only with oxygen, but also with sulphur and with other halogen bodies, so that the idea of compound radi-

* *Sulphides*.—"The compounds of sulphur greatly resemble the oxides, but they are named sulphurets and not sulphides. Berzelius indeed applies the term sulphuret to such binary compounds, of sulphur only as are basic or compound with basic oxides; while sulphide is applied to such as are acid or compound with acid oxides. Hence, he has the *sulphuret of potassium* and the *sulphide of arsenic* and *sulphide of carbon*. Compounds of chlorine are distinguished by him into chlorurets and chloride, on the same principle; thus he speaks of the *chloruret of potassium* and of the *chloride of phosphorus*. But these distinctions have not been regarded by French or English chemists."—*Graham's Chemistry*.

cals has risen from a simple probability to be a certainty perfectly well established in chemistry. But why have people closed their eyes on this certainty? Why, as we have just remarked concerning the ideas of M. Millon, has it been possible to regard this notion as imaginary? It is evidently because, though we are acquainted with a very large number of organic oxides, there are but very few whose radical can be transported from the oxygen to another elementary body, and but one, cacodyle, which has been obtained in an isolated state. Instead of setting out from a state of things perfectly demonstrated, in search of the causes which prevent us from reaching an explanation equally clear for all the phenomena, the idea, which might serve as guide, is rejected, and new explanations are invented, such as metalepsy and the theory of types of M. Dumas, which have been twisted and falsified in such a strange manner by M. M. Gerhardt and Laurent. The attempt to explain the phenomena of combination by means of these theories has led to perfectly false views concerning the intimate relations of bodies which have thus been covered with a veil impenetrable even to the partizans of the theories.

If, on the contrary, we direct our efforts in search of the reason why we know so little of organic or compound radicals, we find immediately that it is impossible at first to regard as radical—of an organic acid, for example—whatever contains no oxygen; for the rational composition of organic oxides present certain circumstances of which we can give as yet no explanation. A grave error would be perpetrated, if it were assumed that in oxamic acid, and in sulphyxymylic acid, the radical is formed by the union of the elements, without the oxygen, as occurs with sulphuric and phosphoric acids. We know, in fact, from its preparation, that oxamic acid is oxalic acid conjugated* with oxamide, and as to sulphyxymylic acid, we are enabled to recognise the nature of the conjugate, because the composition of sulphuric acid is known to us; in this way we have acquired a sufficiently exact notion of the rational composition of these acids. But if in the second of these acids, the radical of the acid was composed of carbon and hydrogen, like that of the conjugate, we evidently could not form any idea of its rational composition.

The very small number of organic substances which do not produce conjugate combinations with sulphuric acid, and the knowledge of the multitude of conjugate acids to which, nitric, nitrous, phosphoric, arsenic and oxalic acids give rise, prove that conjugate combinations are much more numerous than we had reason to believe a few years since, and that they are infinitely more numerous in organic than in inorganic nature. But when the acid and the conjugate are composed of the same elements, though in different proportions, it is impossible that the empirical composition can make known the rational composition; recourse, evidently, then, must be had to facts similar to those already mentioned, and which prove that acetic acid is a conjugated oxalic acid. We know besides that certain conjugates, chiefly those which contain no oxygen, accompany the acid in all the combinations to which the radical of the acid gives rise, in exchanging oxygen for sulphur or halogen bodies; the elayl chloride of platinum presents an example in this, that the elayl when separated from the chloride by means of zinc, remains in the combination with the primitive radical of the chloride, that is to say, with the platinum. This reaction enables us to understand why the oxygen of acetic acid may be replaced by sulphur or chlorine, as if the radical of this acid was $C^4 H^6$; for the conjugate $C^2 H^6$ remains combined with $C^2 S^3$, or with $C^2 Cl^6$, as it was before with $C^2 O^3$.

Now, if acetic acid is a conjugated oxalic acid, how many other acids that we are acquainted with ought to possess also an analogous rational composition, although we have not yet succeeded in discovering the acid nor the conjugate.

* *Conjugated.* The words *accomplement*, *corps copulé*, were introduced into science by Gerhardt. He remarks that many chemists have incredibly abused them.

According to this, it might be easily believed that the number of vegetable acids, not conjugated, was not very large, and that the distinguishing properties of the multitude of vegetable acids with which we have become acquainted, depends principally on the different conjugates which they contain.

These considerations seem, then, to prove that there exist compound radicals, and that a large portion, not to say the greater part of organic combinations is formed by an active oxide of a compound radical, which is coupled to a body chemically inactive, a compound radical or its oxide, or its chloride, etc., etc ; or even a body already conjugated; but they explain also, at the same time, why it is so difficult to isolate compound radicals and to have a clear and precise idea of them. It is true that our knowledge with regard to compound radicals is not yet very extensive, and that the use which we can as yet make of it, is very limited; we have at least become acquainted with the road which we have to follow; that which we should endeavor to discover, and we have learned that the theory of chemical types and metalepsy cannot at all lead to correct ideas; that they are but a deception, a system of illusory explanations which has no other merit than that of being able to adapt themselves to an empirical result, which they put forth as a rational result, by admitting in spite of all possibility and mechanical probability, that an equivalent of an element which forms a part of a type, can be replaced without producing a marked change in the type by 2, 3 or 4 equivalents, or one or two other elements. The time will come when it will be difficult to understand how such absurdities were able to gain any favor.

Let us consider more the important phenomena of the substitution of hydrogen by the halogen elements, or of these last by the hydrogen, and we shall see that, leaving aside these monstrosities of metalepsy and of chemical types to which it gave birth, it will present itself to us with remarkable clearness and simplicity.

This substitution is effected either in an organic oxide not conjugated, or in an oxide combined with a conjugate. In the first case, the radical of the oxide is destroyed, it is converted into a radical less hydrogenated, combined at once with chlorine and oxygen, and forms an oxychloride. The metamorphose of oxide of ethyle and the oxide of picramyle, (essence of bitter almonds,) are known examples of this substitution. When upon the subject of the oxide of ethyle, I will revert to the metamorphoses which it produces with chlorine, and which terminate in the oxalic chloride $C^2 Cl^6$, which remains as the last product, whilst it disengages itself from carbonic acid gas.

In the second case, when the oxide is combined with the conjugate, it cannot be supposed that the hydrogen is simultaneously substituted in the active oxide and in the conjugate. Either the one or the other should be first metamorphosed, whence it follows; and it is this fact that excites the astonishment of the partisans of metalepsy, that certain atoms of hydrogen appear to be combined with an organic body in a manner different from the other atoms of hydrogen. The experience which we have up to the present time acquired concerning these phenomena, leads us to believe that it is the conjugate which experiences the first substitution. If it is a compound radical it is converted into a chloride of a less hydrogenated radical, and then the substitution is gradually effected, so that for each equivalent of hydrogen which is exchanged there is formed the chloride of a new radical; until, finally, there remains nothing but a combination of carbon and chlorine; but the new combination does not abandon, during the progress of these metamorphoses, either its place or function of the conjugate, which it invariably preserves. If the conjugate is an oxide, it gives rise according to the degree of substitution, to different oxychlorides, which likewise preserve their place. During this time. the active oxide, properly speaking, the acide does not experience any change, it maintains the same affinity for the bases, and produces with them salts which present certain analogies, but which, under certain points of view, offer certain marked differences, and which are the consequence of the modification

supervening upon the composition of the conjugate. The phenomenon of substitutions has given rise to a series of conjugate acids in which the acid is the same for all, but the conjugate is different. It is this fact that gave rise to the idea of chemical types. *M. Dumas* having discovered trichloroxalic acid, found that we might regard it as acetic acid, in which hydrogen was replaced by the same number of equivalents of chlorine, playing the same part as hydrogen. There is much truth in the views of *M. Dumas*, for these conjugate $C^2 H^6$ has been converted into $C^2 Cl^6$; but it does not exercise any influence upon the acid properties of the new combination, and the whole error of this theory, which is daily sinking into desuetude, proceeds from the fact that we are deceived as to the genuine acid. From the first I have endeavored to remove and expose this error; I have indicated what should be the probable composition of that acid which has been actually placed beyond a doubt, as far as this can be done by chemistry, by the experiments of *M. Kolbe*, which were referred to at the commencement of this report; but no attention has been bestowed upon my observations, neither at the commencement nor later. Now that the error upon which this theory was founded has become manifest to all the world, we should wait until this last one falls with the false basis which sustains it.

It is clear that since the halogen bodies may be replaced by the same number of equivalents of hydrogen, as shown by *M. Kolbe* (p. 49), these substitutions ought to produce combinations in an inverse sense. The examples of substitution which the experiments of *M. Kolbe* present are of a very decisive character, because the elements of an acid body, dithyonic acid, can by no means be confounded with those of the conjugate, and there can be no sort of uncertainty as to the rational manner of regarding the result.

It often happens in these substitutions, that the conjugate is an oxide which is converted into an oxy-chloride of a radical less hydrogenated, without the supervision of other change. At other times, the substitution metamorphoses only the conjugate, and the active oxide remains intact whatever may be the time during which the simultaneous action of the chlorine and the solar rays may be continued. But it likewise sometimes happens that the substitution extends itself to the active oxide, and then the whole loses the decided character of an acid. It was in a case like this that *M. Dumas* admitted that the type was destroyed. We may consequently see that this theory, such as was developed by *M. Dumas*, enables us to see that it was, at bottom a vague presentiment of the true state of things; but instead of shedding light on the subject and enabling us to form intelligible conceptions, this presentiment has been, on the contrary, buried beneath the productions of the too fruitful imagination of the author.

The phenomena of substitution still present without contradiction many circumstances which cannot be explained in a satisfactory manner by the aid of ideas which I have expressed; but let us distrust the explanations which have been invented, and come to the truth by experience.

Polybasic Acids.—I have reached now the theory of polybasic acids. It is known that *M. Liebig* conceived this theory to explain the loss of water which citrates and double tartrates undergo at a certain temperature with oxide of antimony. It is also known that I have shown by experiments the true cause of this loss of water, and that this theory is not applicable to acids for which it has been invented. Notwithstanding this it has been maintained and adopted not only by *Mr. Liebig* and his school, but likewise by other chemists. This theory being no longer applicable either to citric acid or tartaric acid, has been applied to other acids, the atomic weight of which has been doubled and the acid salts of which have been regarded as unibasic salts, so that the neutral salts have become bibasic salts. The reasons upon which it is relied for asserting that an acid is bibasic were in general very slightly founded; thus they admitted that an acid was bibasic when it formed an acid salt with barytes or the oxide of silver, or even when it gave rise to a salt containing 1-2 atom of water of crystallization—that is to say, 1-2, 11-2, 21-2, etc., etc., atoms of water of crystallization.

It is a very arbitrary supposition to declare an acid bibasic, because it produces an acid salt with a certain base which rarely gives rise to acid salts. According to this, sulphuric acid should hold the first rank among bibasic acids, although it has never been considered as such; as to the combinations of 2 atoms of salt with 1, 3 and 5 atoms of water of crystalization, they present only very ordinary proportions. *M. Fresenius* has submitted the different causes which could lead one to consider an acid as polybasic, to a careful investigation, and he has shown that they are not of a nature to enable us to draw from them any satisfactory conclusion.

I have no intention of denying the existence of acids which saturate 2 or even 3 atoms of base. It was well known for a long time before *Liebig's* theory was promulgated that arsenic, phosphoric, stearic and other acids, form with 1 atom of base an acid salt, whilst in the neutral salt they are combined with 2 atoms of base, and if the base is an alkali the neutral salt has even an alkaline reaction. We do not understand the cause of this phenomenon. The sulphosuccinic acid produces, with two atoms of base, a salt which is feebly acid, and with 3 atoms of base, a neutral salt. But this acid is not on this account tribasic, as *M. Leibig* supposes it to be. It is composed of three atoms of acid, two of which are sulphuric acid, and the third is organic acid, $C^3 H^6 O^5$; when the two atoms of sulphuric acid are saturated, we have but a single salt to which the non-saturated organic acid communicates an acid reaction, and if this is also saturated, we have a tribasic neutral salt. However, in this latter case, it is very evident that each atom of acid will saturate but an atom of base. We possess many conjugate sulphuric acids which are composed of 2 atoms of acid and 1 atom of the conjugate, and which likewise should be bibasic acids. The completely inorganic tribasic acids which have already been described, under the name of sulpho-nitrous acids, present precisely the same circumstances, and further on, we shall find another example of this kind in chelidonic acid, which is very interesting.

I shall here repeat that in the report of 1839, I entered into a number of details upon the existence of conjugate combinations, which had not at that time any particular name, and that I have endeavored to point out the influence which this species of combination must exercise in understanding the rational composition of organic substances as calculated to lead us to a more exact interpretation of the idea of polybasic acids which have been introduced into the science. But the observations which I made at that time, did not excite the least attention, hence for the interest of science, I deemed it necessary again to call attention to this subject.

[Translated from Berzelius' Report on the Progress of Chemistry for 1847.]

Part Fourth.

MEDICAL INTELLIGENCE.

FOREIGN.

1.—*Injuries and Diseases of Bones.*

One of the most valuable works which has ever issued from the press on the subject of fractures, by Dr. R. W. Smith, of Dublin, is now before us; we are given to understand that it is the result of the careful observation and continued labor of years. It embraces some of the most difficult points connected with the subject, which are handled in the most masterly style; it is eminently both philosophical and practical, and its general utility is greatly enhanced by numerous illustrations which convey to the reader, more effectually than any description in words could do, the nicer shades of difference presented to the view by some of the more obscure cases of injury to the bones in the vicinity of the joints. The substance of some of the more important chapters is summed up in several series of corollaries, which have been introduced into a former part of our present volume (Art. 40, p. 80). We are induced, however, again to refer to the work, and to place before our readers some additional practical observations.

15. *Fractures of the Neck of the Femur.*—Mr. Smith's view of the value and diagnostic import of the two much disputed signs—shortening of the limb and inversion or eversion of the foot, will be found in the article just referred to. He remarks that the surgeon who supposes the difficulties of diagnosis slight and easily overcome, can have but a very limited experience of such injuries; on the amount of shortening which occurs in the two varieties, the intra and extra-capsular fracture, respecting which so remarkable a difference of opinion even now exists, he feels certain that the *degree* which immediately succeeds to the injury, may, with proper precautions, be considered as diagnostic of the *seat* of the fracture, this being *greater* when the lesion is external to, than when it is within, the capsular ligament. When the line of fracture, in intra-capsular fractures, is perpendicular to the axis of the neck of the bone, or when it has passed from the superior part of the corona of the head obliquely downwards and inwards, the inferior fragment is drawn upwards,—or at all events there is nothing to prevent its being so drawn upwards. But when the fracture runs from the inferior part of the corona obliquely downwards and outwards towards the summit of the trochanter major, then, if there be no displacement as regards the diameter of the bone, the ascent of the lower or external fragment is opposed by the superior, and the amount of shortening is less than in either of the other cases.

If the force that acts upon the neck of the femur be inconsiderable, the fibrous membrane which encircles it—"the cervical ligament of the femur"—may escape uninjured, in which case the retraction of the limb will be inconsiderable, and will be at its minimum when the fracture has traversed the bone obliquely from the inferior part of the head downwards and outwards, as just now stated. The synovial and fibrous membrane remaining entire, may have the effect of keeping the fractured surfaces firmly together, and the limb may be thus secured from any change in length or position; or remaining entire on the anterior side, eversion may be wholly prevented, and again remaining entire on either side of the neck of the bone, shortening of the limb will be counteracted.

This author gives very cogent reasons against the assertion of Dupuytren, that the occurrence of shortening, at a period more or less remote from the receipt of the injury, as in an instance referred to by the surgeon of the Hôtel Dieu, in which, at the end of four months, it was said to take place *suddenly*, is attributable to the "yielding of the callus." Mr. Smith attributes the occurrence to the gradual process of absorption going on in the neck of the bone, though it might have escaped observation as long as the patient remained in bed with the limb inclosed in an extending apparatus. In the case here referred to, he doubts the fact of its having taken place *suddenly*. (p.13.)

Mr. Smith has never seen an instance of fracture external to the capsule in which there was not shortening of the limb from the very moment of the occurrence of the accident;—there is in many instances a primary and immediate shortening; and the so called consecutive displacement is merely an increase in the amount of shortening already existing. His experience also leads him to deny that a fracture of the neck external to the capsule ever occurs without injury to the trochanter; these fractures are always in the first instance *impacted* fractures, and all impacted fractures are necessarily accompanied by a fracture traversing some part of the trochanteric region. In a hundred specimens examined, without a single exception, a second fracture was found in this region. This is the necessary result of the impaction of the broken cervix into the shaft of the femur, and occurs secondarily in the order of time. The forces in play, and the manner in which this complicated injury is produced, are admirably discussed (p. 17), the illustrative plates are most complete and instructive. They fully explain how it happens that the shortening is sometimes greater and sometimes less in the extra-capsular fracture.

From what has already been stated, it will be seen that Mr. Smith does not agree with those in opinion, who maintain that the shortening of the limb is a symptom destitute of value in determining the seat of the injuries with respect to the capsule; cases, it is true, frequently occur in which this symptom is not of itself sufficient to determine the question; but suppose a surgeon meets with a case in which the shortening does not exceed half an inch, he knows that this may indicate a fracture either within or without the capsule, but he also learns from it, that if the fracture be external, it is also an impacted fracture; he then examines further, and if he finds it impossible, or extremely difficult to restore the limb to its natural length by extension, that he cannot elicit crepitus, that the loss of power is not as complete, or absolute, as in the fracture within the capsule, he at once connects these symptoms with the slight degree of shortening, and from their union he forms the diagnosis of extra-capsular impacted fracture of the neck of the femur.

Roget's diagnostic sign is rejected, viz: the direction of the force by which the fracture has been produced, as a means of arriving at a differential diagnosis as to the seat of the fracture.

As respects the difficulties of diagnosis in those comparatively rare cases in which decided and prominent *inversion* of the foot occurs, Mr. Smith, after stating that he regards them as being most frequently extra-capsular fractures—in five cases out of seven this having proved to be their seat—proceeds to show that it is these cases which are specially liable to be confounded with luxations.

Whenever the fractured portions of the trochanter can be brought into contact a crepitus may be produced; but when, from the direction of the fracture, one portion of the trochanter has been drawn towards the great ischiatic notch, no crepitus may be discoverable; and a source of error will exist, from the resemblance of the fractured portion of the trochanter to the head of the femur; and if, with this circumstance, there should happen to be inversion of the limb, the difficulty of diagnosis will be increased; but the presence of this inversion should never be allowed to embarrass our diagnosis—the facility with which the limb can be brought to its natural length by extension—the recurrence of the shortening when the extending force ceases to act—and the possibility of flexing the thigh upon the abdomen, establish the diagnosis between fracture external to the capsule and further displacement of one or both trochanters.

The inversion of the foot in these cases is not produced simply by muscular action, as taught by some surgeons. Mr. Smith states, that the deformity having been removed by extension, as soon as the force ceases to act the limb is again shortened, but the foot will now be found to remain everted. There is no instance in which, under similar circumstances, a fracture will exhibit opposite characters; and, with Cruveilhier, Mr. Smith believes that the inversion is attributable to the relative position of the fragments of the bone, rather than to the influence of muscular contraction. In every instance of fracture of the neck of the femur, accompanied with inversion of the foot, which Mr. Smith has had an opportunity of examining after death, the inferior has been placed in front of the superior fragment, and the author makes the suggestion, that in this position, the direction of the fibres of certain muscles being changed, the inversion is produced secondarily by muscular influence, but the question is one still open to investigation. Turning to the pages of systematic writers for an account of the various causes to which the occasional inversion of the foot has been attributed by different surgeons, we find them given by Mr. South.* The doctrine of “partial fracture” of the neck of the femur, as laid down by Mr. Colles, and also by Mr. Adams,† according to Mr. Smith, has not been established; in all the cases of supposed partial fracture external to the capsule, there has been unequivocal testimony of the existence of fracture of the trochanter; and they are all cases of the impacted and complete fracture, rather than of partial fracture of the cervix. With respect to partial fracture within the capsule, as described by Mr. Colles, Mr. Smith is manifestly sceptical; he is disposed to believe that some mistake has been committed, the exact nature of which, since the specimens cannot be found, it is now impossible to ascertain.

The question, whether osseous union ever takes place in fracture within the capsule, is assumed in this work to have been satisfactorily answered in the affirmative. Bony union is not effected through the medium of a provisional callus; but, as in some other instances in the animal economy, is effected by direct union of the broken surfaces confronted to each other. Mr. Bransby Cooper's opinions on this subject were placed before our readers in our second volume; these opinions are rejected by Mr. Smith. Eight cases are given in the text illustrative of the affirmative of the question, but on this part of the subject we may refer our readers to our former reports.‡

15. *The Treatment of fracture of the Neck of the Femur.*—Mr. Vincent has some philosophical views on this subject.§ The injury is so close to the centre of gravity of the whole body, that every slight movement must produce motion between the broken parts, but these motions are only likely to take place in actions where there is a movement forwards, as in the movement of the head and limbs, which are nearly all forward and backward; while, therefore, the patient

* Notes to Chelius, vol. i., p. 565.

† Cyclopædia of Anatomy, art. “Abnormal Condition of the Hip Joint.”

‡ Report on Surgery, vol. ii., p. 201, and vol. iv., p. 226.

§ Lib. cit., p. 51.

is on his back, there is a continual interruption to the curative process; but, on the side, there are so few continuous lateral movements, and perhaps not one in which the movement is about the centre of gravity, that in this position there is the least possible interruption to the uniting process; the centre of gravity is directly over the injury, and the whole weight of the body presses on the bones, and keeps them in apposition. Mr. Vincent has treated cases by this method, and they have turned out much better in restoring the powers of the limb than the plan usually adopted. The lateral position requires that the thigh should be bent on the trunk, and the leg on the thigh. The position of placing the patient half on the side and half on the back is doing little. The sound hip should be vertically over the injured one. However, the fact is, that the age of the subjects of this accident compels us to adopt the position on the back, and the inclined plane, as it is only in this way the functions of life, in the advanced stages can be even tolerably well carried on. Moreover, as on the side the whole weight of the body is concentrated on the trochanter major, the chances of sloughing are much greater than when the pressure is spread over the large surface of the back.

The cases where the fracture takes place at the root of the trochanter, so that this process is still attached to the shaft of the femur, and the neck remains with the head, are not so common. The treatment of placing the patient on the side is the best, as it secures him from the jars and displacements that must occur when he is on his back; and as the cases are usually in individuals of less advanced age, as in the fracture of the actual neck, it can in general be adopted. In this injury the fracture unites well as to strength, but usually leaving the limb shortened; and if treated on the back, without great care, with the foot much turned out.

16. *Fractures of the Lower Extremity of the Radius*.—Mr. Smith has some valuable remarks on Colles' fracture. In the first place he has never seen it so high up as originally described by Dr. Colles; the most usual seat is from three quarters of an inch to an inch above the radio-carpal articulation; sometimes it is only a quarter of an inch above the joint, but he has never seen it higher than one inch; it always appears to be higher than it really is, but should the lesion of the bone take place at two inches or more above the joint, it no longer presents the peculiar and remarkable characters which distinguish the injury which has been designated after Dr. Colles. This particular fracture has also been described by many surgeons as an impacted fracture;* Mr. Smith's reasons for dissenting from this opinion are given in our extracts (art. 40).

17. *Fractures of the Humerus*.—No surgeon can have been long in considerable practice without having met with difficulties in the diagnosis and treatment of injuries at the shoulder-joint. These injuries, so far as fracture is concerned, are most satisfactorily elucidated in Mr. Smith's work.† This surgeon defines clearly the fracture as seated at the *anatomical neck* of the bone, at the *line of junction* between the epiphysis and the shaft, and those which traverse the surgical head of the bone. The corollaries under this head, in the present extracts are well entitled to the attention of the practitioner. Instances are given of *fractures of the greater tuberosity*, one of which was inserted in our fifth volume (p. 79). The diagnosis of this particular case is laid down as follows: The acromion more prominent than natural, but the finger cannot be sunk into the glenoid cavity; no difficulty in approximating the arm to the side; the breadth of the joint, greater, "nearly double" that of the opposite one; the existence of two tumors, the inner and larger placed under the coracoid process, and evidently constituted by the head of the humerus; the external and smaller apparently formed by the greater tuberosity,

*Miller's Practical Surgery, p. 313.

† Page 176.

corresponding in situation to the glenoid cavity; these tumors separated by a deep and well-marked sulcus, following the direction of the bicipital groove.

At first sight, the appearances resemble those of dislocation of the head of the bone forwards, the facility with which the elbow can be brought to the side, and the great increase in the breadth of the joint, are sufficient to establish the differential diagnosis.

The *extra-capsular* impacted fracture, occupying the situation which marks the junction of the epiphysis with the shaft, and accompanied by penetration of the superior by the inferior fragment, is extremely difficult of diagnosis; the principal points upon which this is to be formed are given in our extracts: but in the text Mr. Smith further directs, that, in order to form a decided opinion, let the surgeon, with both hands, grasp the head of the bone with firmness sufficient to maintain it as nearly as possible in a fixed position, while an assistant rotates the elbow, by which method, in a majority of cases, crepitus can be produced.

The diagnosis of the *intra-capsular* impacted fracture, as compared with that of the extra-capsular impacted fracture, is simple; this is the fracture which traverses the anatomical neck of the bone, in which the superior fragment is driven into the inferior fragment, one of the tubercles being usually broken off from the shaft; thus this particular fracture of the humerus is analogous to the *extra-capsular* impacted fracture of the cervix femoris, while the former is analogous to the *intra-capsular* impacted fracture of the latter bone.

In former volumes the subject of bony union of intra-capsular fractures of the cervix femoris has been laid in full before our readers; it is interesting to know the result of Mr. Smith's observations as respects this question in analogous fractures of the humerus. Mr. Smith states that, notwithstanding the unfavorable circumstances in which the bone is placed, as regards bony union, when a fracture has traversed the anatomical neck and there is no impaction, there is abundant evidence to prove that osseous consolidation may still be accomplished; but it is highly probable, where this fortunate result has occurred, the vascular communication between the fragment has not been entirely cut off, and that the margins of the fragments have remained here and there connected with each other, by the attachment of the capsular ligament, the vascular supply derived from which proved adequate to the preservation of the vitality of the head of the bone. Bony union in the impacted form is much more certain in consequence of the impaction.

The impacted fracture always unites with a certain degree of deformity, and as regards the intra-capsular variety, it would be imprudent to restore to the joint its natural form, since we should thus materially diminish the chance of osseous consolidation. In the treatment of such cases, it is therefore sufficient to bandage the arm to the side, and to support the forearm in a sling; but the prudent surgeon will never omit to announce to the patient that a certain degree of impairment of the motions of the joint will be a permanent result of the injury.*

There are some other varieties of these injuries, and most important and interesting pathological and practical points, which we may have opportunities of referring to in future volumes.

18. *Ununited Fractures; their treatment by a modified application of the Seton.*—After allusion to the irrationality of the methods by friction of the ends, of cutting down upon and sawing the ends, and the pressure of a seton between the ends of the ununited fragments, Mr. Francis Rynd published some cases in which a seton was applied successfully in the following manner. In an un-

united fracture of the tibia and fibula, a curved seton-needle was passed into the inside of the leg, exactly opposite the fracture, through the integuments, so deeply as nearly to touch the posterior internal edge of the tibia; it was then directed in a semi-circular course anteriorly, over and close to the prominent extremities of the fractured bones, and was brought out on the outside of the limb, so that the fracture lay between the points of its entrance and exit: the seton not touching or passing between the fractured extremities of the bones. An ununited fracture of the humerus, of fourteen months' standing, was cured by this method; also a case of a ligamentous union of a fracture of the femur, of fifteen months' standing; also an ununited fracture of patella was cured by the same method.*

19. *Badly-united Fracture.*—Mr. Rynd describes a very interesting case of deformity from a badly-united fracture of the bones of the leg, treated by resection of portions of the bones, and resulting in perfect recovery with deformity.† An incision was made four inches in length, commencing two inches above the deformity, parallel to and behind the posterior edge of the fibula; this incision severed the connections of the soft parts with the bone in this direction; a similar incision was made along the posterior edge of the tibia. Those incisions were connected inferiorly by a transverse one in front, passing through the skin and integuments; the portion thus incised was dissected up, and formed a flap, which, being raised, exposed completely the deformed bones; a chain saw was then passed round the fibula, keeping close to it, in order to avoid the vessels, and the bone was sawed through above the deformed part, then below it, in a similar manner; the piece was firmly attached to the angular portion of the tibia, and so not easily removed; the deformed portion of the tibia was removed in a similar manner. The limb was then placed straight, the extremities of the bones in apposition, the flap was drawn down, it covered the whole wound, and was united by a few points of suture; the limb was placed in a case prepared for it, and the man put to bed; there was not a blood-vessel divided, nor was there an ounce of blood lost.

Profuse suppuration ensued; in the fourth week after the operation, erysipelas set in, which extended all over the leg to the knee; in two days afterwards mortification set in along the line of incision, and soon engaged the greater part of the flap. At the termination of the seventh week after the operation, the aspect of the case was so bad that, after consultation, amputation was resolved on; the poor fellow begged for time, which was assented to; he struggled on, and, in a little more than a month after this, a portion of the tibia exfoliated. He then began to get better, and, after nearly ten months' confinement, the cure was complete, his leg being straight and of the same length as the other. The patient's anxiety to have the deformity removed and the use of the limb restored, and the intolerable pain he suffered, were the circumstances which justified the operation.

20. Dr. Stark describes a "*Case of dislocated Head of the Radius successfully reduced two years and one month after the occurrence of the dislocation.*" The author recites the opinions of Astley Cooper, Flanbert, Marx, and others, that dislocation of ball-and-socket joints may be reduced at a much later period than those of hinge-joints, but limiting even the former to a few months, and that the latter become irreducible within a very short period after the accident. As the chief danger in reducing old dislocations is said to arise from the risk of rupturing the muscles, blood-vessels, or nerves, by violent efforts at reduction, it was determined, in this case, to extend the arm firmly but gently, day by day, till the new adhesions at the head of the radius were so much lengthened, or the head so loosened from its new site, that by the employment of not much additional force, the bone could at last be replaced. The extension was effected by seiz-

* Dublin Quarterly Journal, Nov. 1847, p. 273.

† Ibid., p. 288.

ing the hand of the patient with the right hand, bending the elbow joint so that the forearm formed a right angle with the arm, and applying the counter-extension by pressing the left hand close above the elbow-joint, and thus fixing the humerus. The extension was continued until slight uneasiness was complained of. It was repeated daily for three weeks, when the head of the radius had become loosened, and could be pulled to the edge of the articular head of the humerus. When brought into the latter position, the ball of the thumb of the left hand was pressed against it, and bending the forearm on the arm, the bone quietly slipped into its place.*

21. *Abscess of the Tibia*.—Dr. Hutton publishes cases of this disease, from which it is to be inferred—that inflammation of the cancellated structure of the bone may occur without terminating in suppuration—that after suppuration a cavity is formed, lined by an organised membrane, and containing pus alone, or pus with small fragments of the cancellated structure—that in most cases the osseous walls become denser and thicker, and the medullary canal blocked up, but where spontaneous openings occur, there, of course, the walls are absorbed—that it is probable in most cases where the abscess heals, the cavity remains, secreting fluid, which is again absorbed—that the temporary variations in the swelling depend upon the condition of the soft parts, but the firm swelling, which slowly extends itself along the shaft of the bone, depends upon the enlargement of the bony structure—this *progressive* enlargement, taken with tensive pain, aggravated at intervals and not yielding to treatment, with impaired health, supplies a valuable means of diagnosis—that in abscess of the cancellated structure, the swelling and pain occupy the extremity of the bone, and, unlike necrosis, the periosteum is often not sensibly influenced at first. As respects treatment, spontaneous openings, when they happily occur, bring relief, and the surgeon should not certainly hasten this consummation; and although in simple purulent abscesses small openings may suffice, it is generally judicious to make a free opening to clear the cavity of all *debris*, and the probability of large articulations in their vicinity becoming implicated, is an additional reason for promptly giving exit to the confined matter.†

AMERICAN MEDICAL INTELLIGENCE.

- 1.—*A short account of an Endemic fever which made its appearance in Lowndes county, Alabama, in the spring of 1846.* By T. P. RUSE, M. D.,—(July 1848.)

This is a lime county, not much water ordinarily—but the spring and summer of '46–47 and up to the present writing an unusual quantity of rain has fallen. The disease has, as a general rule, been confined to those who inhabit old wooden tenements, in a state of partial decomposition, without reference to particular localities,—and from the fact that whole families in many instances and nurses contracting it, has been by many considered contagious. The writer does not so consider it, though quite as much so as the cases of yellow fever reported in the April No. British and Foreign Medico-Chirurgical Review, on board the "Eclair," nurses or those exposed to the same exciting causes can be *infected*, should not strict attention be paid to cleanliness and ventilation. Children are not so liable to the disease as adults, generally mild in the former and more malignant in the latter and in negroes than in whites.

* Edin. Med. and Surg. Jour., Jan. 1848.

† The Dublin Quarterly Journal, Feb 1848, p. 279.

In the onset, the patient complains either of pain, or fullness of the head, and weakness, otherwise he "feels quite well." Appetite unimpaired,—pulse of natural frequency generally; rarely above, sometimes below 35 per minute. Tongue slightly coated; point and edges always red. Eyes dull; skin remarkably dry and harsh; temperature nearly natural, except over the bowels which is generally *above* natural, *without tenderness upon pressure*, abdominal muscles contracted, hard and resisting, even when the legs are flexed, in all the cases in which the writer has noted the latter feature. He would here remark that the degree of temperature of the bowels, redness of the tongue and dryness of skin, indicates the malignancy or not of the disease as these are greater or less. *Most of the secretions healthy.* He also remarked a peculiar and characteristic fetor of the breath—which is very offensive, particularly when several are confined in the same apartment.

As the disease advances the above symptoms increase with the exception of pain in the head and frequency of pulse. The former usually subsides after the first 4 or 5 days; the latter will depend, in the writer's opinion, entirely upon the treatment instituted. Anything which has a tendency to increase the intestinal irritation, will accelerate the pulse, in fact aggravate all the symptoms. After the subsidence of the pain in the head the patient does not complain of anything except weakness,—will at each visit express himself as feeling much "better;" this will he do as long as he lives. Duration of the disease varies from 10 to 70 days; the milder cases, under the ordinary treatment, will be greatly prolonged and the less mild will terminate fatally much sooner. The writer does not know anything of the pathology of the disease, except what he has learned from witnessing the effects of medication. A *post-mortem* was made in this neighborhood by two or three physicians,—one of whom stated to the writer that "there was no organic disease; the mucous surface of the stomach and bowels being only slightly injected." In the writer's opinion the same state of things obtains in this to some extent as in Typhus, or Typhoid fever, with some modifications as will be seen by reference to the symptoms above enumerated; not however sufficient to exclude it from coming under the head of Typhoid fevers. *It is a continued fever.*

The treatment as far as internal remedies were concerned, when the disease first made its appearance, was the same as in ordinary fever. It was regarded as "the same disease in another form." Mercury, quinine, opium &c. were tried in combination, and alone, in large and small doses, without benefit, but in the writer's opinion with infinite harm. This may be surely said of mercury, in cases the least disposed to malignancy, producing even in minute doses, dryness of the tongue, increased, if possible, dryness of skin, and in larger doses increased irritation of bowels, as will be indicated by frequent and watery evacuations and by increase of heat and if not actual discharge, great commotion of the bowels. Should its use be persevered in as the writer has known it to be, it will produce "Black tongue." The fever under consideration has been variously styled—Typhus, Typhoid, Slow, Nervous, and Black-tongue fevers, in this and other neighborhoods where it has made its appearance.

The conclusion to which the writer has arrived with regard to the use

of mercury has not been hastily formed. His opportunities for observation have been ample, and from its first application, the disease has elicited no little interest among practitioners and the community. From 50 to 75 per cent proving fatal, each one endeavoring to pursue that course of treatment which best accorded with his notion of its pathology, and this with many was little better than mere conjecture, being guided alone by the observable effects of remedies employed. Upon the re-appearance of the disease in May of last year, the writer, dissatisfied with former treatment, and not yet prepared to give up *all* internal treatment, and regarding it as he yet does as a local disease of the bowels, treated it with counter-irritants,—small doses of Laudanum* and Spts. Turpentine,—Laudanum and Starch Enema, also calomel or Blue Pill,—in some cases sponging with cold water, and finally cold water. Enemas, and with much better success. The latter remedy was used with happy results in all cases in which calomel or Blue pill had not been exhibited—* the great difficulty of producing and maintaining diaphoresis (as it was observed that speedy convalescence always followed this condition of things,) and as all other agents had failed, induced the writer to prescribe large doses of quinine. This had the effect; but unless it was at the very commencement of the disease, patients could not bear its sedative action; and besides even when combined with opiates it is too apt to disturb the already too irritable intestinal canal, (whether given by the mouth or *per anum*; to allay which you are compelled to resort either to the laudanum and starch or what in the writer's opinion will have a more permanent effect, cold water, particularly if you have much heat. Another objection to the former and in favor of the latter, is the known tendency of opium to check secretory action.

To cut short this article and to come directly to the object of it, viz: to warn the profession against giving medicine in this fever (*en passant* it is something similar to Ship fever,) and to make known the fact that since the adoption of the writer's present course of treatment he has not lost a case of this fever—having treated eighteen or twenty cases, most of them adults and some of which were very malignant, with subsultus tendinum, coma, and almost at the commencement, he has seen no black tongue, sordes, thirst nor watery evacuations, (unless it had been thrown into the bowels) unusual heat &c.

In the first place your patient should have the entire surface of the body thoroughly washed, using soap freely, brush or coarse cloth *a là valet d'écurie*, cold water enemas, rendered soft by the addition of some alkali (*salærat*us,) the only precaution necessary is simply to empty the instrument *slowly*, and without regard to quantity, as much as your patient will bear comparatively, or until he complains either of the weight or distension; the repetition to depend upon the return of heat, the al-

* The writer has never known a patient to be brought under the *specific* or *constitutional* effects of mercury, until after convalescence had been decidedly established; though he has known it to be given in all forms of the disease, for fifteen or twenty days and nights, consecutively, without accomplishing the end for which it was given. This is easily accounted for; the bowels being the seat of diseased action, the function of absorption from this surface has ceased, and the consequence is that the calomel &c. merely acts as a mechanical irritant.

laying of which is the immediate effect as well as all redundancy of action whether vascular or nervous, one thing is certain, the skin will never act while this excess of action is concentrated on the bowels—and another certain thing is, that, when you shall have succeeded in allaying the intestinal irritation, the skin will act, in the writer's experience, particularly if the patient is placed in a Hydropathic Sheetbath. The length of time which the patient is to be kept in the bath to depend upon the comfort of your patient and the judgment of the physician; the object being to produce free diaphoresis; this as above intimated will depend upon the internal heat &c., or whether active internal remedies have been exhibited or not. The writer not only proscribes all internal remedies, but the introduction of anything into the stomach, except soda (solution sup. carb. soda) or salaratus water. Starvation and cleanliness will alone answer in all mild cases.

Sanatary reform is what we want to prevent its occurrence.

Note.—It may be, that some little moral courage is necessary to publish the term "Hydropathic-Sheetbath" but for myself I can say, I am in search of TRUTH in the practice of my profession—and I can assure you that the above operation will not prove anything but agreeable to your patients and satisfactory to yourselves. R.

2.—THE LATE FEVER IN NEW YORK.

To the Editor of the Boston Medical and Surgical Journal.

DEAR SIR,—Since I wrote you, a few days since, the yellow fever panic has materially subsided, although a few new cases have occurred on Staten Island, most of which have been of a comparatively mild type. I have visited and examined closely many of these cases, and witnessed the post-mortem appearances in some, and have no hesitation in pronouncing the most malignant of them true *yellow fever*. On Wednesday last, Dr. Ashbel Smith, of Texas, read a very interesting paper on the disease, before our "Academy of Medicine," in which, after diligent personal investigation of the malady on Staten Island, he pronounced it identical with the yellow fever of Galveston, of which he has published a very graphic memoir. Dr. S. is undecided as to its origin, but is inclined to the belief that it is occasioned by local causes, and not by an imported virus. He is a decided non-contagionist; while Prof. Dickson, who has also had very extensive opportunities of observing the disease in Charleston, S. C., is equally confident as to its imported origin. "Who shall decide," &c.

Now look at some of the facts. A fever, of comparatively a mild type, spreads gradually through a population scattered over the eastern border of the island for a mile or more along the shore, and half a mile inland, opposite to the anchorage ground of vessels detained at quarantine, and these cases have appeared, from time to time, since the first of June, without the suspicion of their being yellow fever cases, until several of a more aggravated type occurred, attended with black vomit, when the disease is recognized and declared to be the yellow fever. These cases occurred, in most instances, among those who had never had any connection with the shipping; and in some, who had been confined for a long period to their own dwellings, as the "Retreat" where a lady died with black vomit; and in no case, it is believed, can the disease be traced to contagion, understanding by this, the communication of the disease from one person to another. In one instance, it is true, a washer-woman, at the Quarantine Station, was seized with the disease after washing the clothes of a yellow fever patient, who died on board one of the vessels which brought

in some of the returned volunteers from Vera Cruz, and she died, after a short illness, with black vomit. But the disease did not spread to the attendants or nurses of the hospital. Nor can the disease be traced to the shipping, except by supposing, which is highly improbable, that the virus was carried by the wind, flowing landward, the vessels being anchored a quarter of a mile from the shore; and this is the way in which Dr. Whiting and the Committee of our Board of Health suppose it to have originated. On visiting the Island, however, the senses cannot but recognize other very powerful causes of febrile disease, such as exhalations from stagnant waters, accumulated filth in the streets and yards of the houses; and especially in the putrefying and horribly stinking masses of sea-weed and refuse garbage on the shore of the island, which alone would seem sufficient to produce a plague of the most virulent kind. And yet the inhabitants, blind to all these prolific sources of pestilence, are petitioning to have the Quarantine Station removed lower down in the harbor, without being aware, as it would seem, that there can be any other source of disease than imported contagion. It is true that the Quarantine Physician and the Board of Health have very unwisely and unfoundedly, as I think, given currency to such a belief, by promulgating the opinion that the specific cause or virus has been blown from vessels on to the land; but this opinion can hardly be sustained. Who has ever before heard of such a phenomenon, or witnessed such a fact?

What is to prevent yellow fever from originating on Staten Island, or any where else, if the causes which occasion it in Galveston or Vera Cruz exist there? Are we to believe that the virus originated from some accidental combination of causes, years ago, which can never again happen? It seems to me, we ought to know better than this, or that it must necessarily originate within the tropics. It may be said the specific cause of yellow fever is too subtle or refined for our gross senses. Grant it; but then can we not know, do we not already know, the several conditions, external and internal to the body, which give those causes power? Is it not conceded, on all hands, that yellow fever cannot exist in a pure atmosphere? And is there a city in the world which cannot, by using the proper means, be made so cleanly that no yellow fever could find a foot-hold? How much more rational, then, as well as philosophical, that our civic authorities should direct their attention, not to measures of quarantine, non-intercourse, and embargoes, but to hygienic regulations by which the evil will be arrested at its fountain-head! Far easier this, than to dam the current, swollen by a thousand tributaries. At any rate, it is high time the profession, at least, should advocate the truth, and no longer lend their influence to sustain the exploded doctrines of quarantine and cordons sanitaires, knowing full well that the evil which springs from the bosom of nature, needs for its removal no other rules than those which nature herself reveals.

MEDICUS.

We extract from "*De Bow's Commercial Review*" for August, the following interesting reminiscence of the "*Olden time in New-Orleans and Yellow Fever*:" It was written by Maunsel White Esq., an old and highly respectable citizen of New Orleans.—(Ed.)

"The morning was calm and bright. A cloudless sun poured down its rays, and the waters of the Mississippi were smooth and still, except when slightly agitated by the ripple made by the stroke of our oars. We landed our fleet of flat boats a little above Gravier, and within ten steps of Tchoupitoula streets, at 10 o'clock in the morning on the 1st of August, 1801, after a *passage* of 60 days from Louisville in Kentucky. The writer, then a mere lad, thought New Orleans to be a very queer looking place, and was astonished at the jargon of strange tongues. The city at that time was in the hands of the Spaniards, and Don Manuel Salcedo, Governor. It was a fortified town. There was a

moat or ditch cut around it, and an embankment with palisades and several small redoubts placed at convenient angles. These works were in a state of decay at that time, and were of no defence to the city. They ran from the river where Canal street now is, back to Rampart street, thence down to Esplanade street, and from Esplanade to the river, below where the mint now stands. There were three gates, through which people passed from 6 o'clock in the morning until 9 o'clock at night, when they were shut. At each was placed a sentry-box with a corporal's guard. In those days of Spanish gate rule a corporal was an important man, and marched before the Governor, when he took his evening's walk on the levee, to clear the way should any one dare approach him, without a low bow, or without leaving a wide birth. The free and easy Kentuckians were particularly obnoxious, as they could not comprehend why a mere man in the shape of a Governor should be any better than themselves, and looked with astonishment at the deference paid by the Spaniards to such a decrepid old fellow as Governor Salcedo then was.

The day after our arrival a case or two of yellow fever was reported in the city, and we hired a house in the Faubourg St. Mary, in Poydras-street, between Magazine and Tchaptoulas streets, the only house at that time in that square except one. This place was then considered in the country, and beyond the limits of yellow fever! It was a low, one story building, with four rooms in the body and two galleries. While the family with whom I resided were arranging this residence, I was occupied unloading the boats then laying at the levee in front of Tchaptoulas street. Our cargo was rolled out and across that street into the stores of Mr. Lylle Jaspie, who then lived at the corner of Gravier street, fronting the river. On the other side of Gravier street lived the captain of the port, in a wooden frame building. On the corner of Common street there stood a small sailors boarding-house, and between that and the gate or the ramparts of the city there was no building of any kind, but only an open piece of ground, extending from the river to the woods, at that time not cleared away beyond where the State-house now stands. All that space of ground between Common, Poydras, Magazine, and Carondelet streets was used for vegetable gardens. Where the St. Charles Hotel now stands, and all the buildings between Gravier and Common streets, was old Mr. Percy's cabbage garden!

But I am digressing—I must return. After we got out our cargoes of flour, tobacco and pork, and the hands were paid off, and all our bold Kentuckians had started for their far homes, we began to think of selling out. In the meantime, the yellow fever began to spread, and a gentleman from Kentucky, who came down and lived with us, and who was continually out in the city, took it, but escaped after eight days' illness. Mrs. E. was then taken down, and died in a few days. This event broke up our house-keeping, and Mr. E., with his only child, and the writer, took up our abode in the city at Madam Chabbot's, a house as well known at that period as is the St. Charles now. The fever continued, and several were taken to their long homes from this house. My room was opposite to what was called the Hospital Room, out of which I saw a fellow-boarder taken the day before. Alas! there was no help for it, but to take my chance. On Monday night, after about one week's residence here, I found myself sure enough "a case," and in the morning, not having made my appearance at breakfast, a servant, dispatched to my room, told the story of my fate. Doctor Flood was sent for; he came, looked at my tongue, felt my pulse, gave me some medicine and left me. In the meantime, in came Nanny, a great, stout, strapping Negro woman, who wrapped me up in a sheet, took me up in her arms as one might take a baby, and carried me off *volens volens* to the hospital-room, where she placed me in a bed, occupied, perhaps, before, by hundreds who had died. I became very sick. The medicine had its effect, and Nanny was very attentive. Wednesday passed with one other *dose*—and on Thursday morning, Madam Chabbot, a good hearted lady, and by birth an Irish woman, came

to see me. I was dozing away. She lifted the mosquito net, and the room being dark, took a long, steadfast look at me, pursed up her mouth, shook her head, and departed. I remember that look to this day; but I said to myself, somewhat out of humor, "I am a better fellow yet than you think for, old woman—zounds!" and turned myself in a hurry in the bed. In a few minutes in came Nanny, bringing something on her shoulders very much in my fancy like a coffin. I exclaimed involuntarily, "What now?" The energy with which I spoke, roused the old woman, and she came quickly to the bed-side, drew up the net, and looked at me in seeming surprise. "What's the idea, Nanny," said I; "are you about to bury me alive?" "No," said she, "don't you see it is nothing but a bathing-tub!" suspecting at once that I had mistaken it for a coffin. "Ah! well," said I, somewhat relieved, I must confess, by the announcement. "Where is the Doctor?" "He is coming, and will be here before I get the water ready. Doctor Deluge soon made his appearance, and ordered Nanny to place me in the tub. No sooner said than done; for in Nanny's arms there was no use in kicking, and down she set me in the tub, with a wench as stout as herself on each side, with a large bucket of cold water in their hands. On they poured it, unsparingly, bucket after bucket. It seemed as if the very Mississippi were pouring over me. The shock was terrible, but I had no time for reflection. I was quickly taken out, wrapped in a dry blanket, and placed on a table, like a dead man. All now commenced rubbing with dry towels, with all their might and main. They then put me into bed, on a new well-aired mattress and clean sheets, and covered me over with blankets, until I thought I should have been suffocated. The doctor left without uttering another word. I begged Nanny to take off some of the blankets, telling her I was better, and did not wish to smother. She indulged me in this very natural wish the moment she found my skin warm. I began very soon to perspire freely, and finally fell asleep, and did not wake until late on the morning of the next day, (Friday;) Nanny was near me, and with a smile on her grumpy countenance, asked how I felt. "Hungry!" said I, fiercely, "you imp of darkness." The good Madam Chabbot, heaven rest her, was soon at my bed-side, took me by the hand, and seemed happy to think I was safe. "But don't be imprudent," she charged me. The next morning, Saturday, I was up, rather weak, to be sure, but still cured of yellow fever. Thanks to God, a good constitution, the Doctor and Nanny, etc.*

I am your obedient servant,

M. W.

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- 4.—*On the Medical Treatment of Cataract.*—By WM. G. SMITH, M. D. Communicated by letter to Dr. JAMES BRYAN, M. D., to the College of Physicians and Surgeons of Philadelphia, Pa.

[We invite the attention of our readers to the following letter of Dr. Smith, communicated by James Bryan, M. D., President of the College of Physicians and Surgeons, and Chairman of Committee on Surgery. Dr. Bryan has published several cases of Cataract successfully treated upon the plan laid down in the following communication, and we understand that he has several now under treatment, which promise the same success as have attended his published cases.—*Ed. Buf. Med. Jour.*]

* Cold water should never be applied in the last stages of yellow fever, or on a weakly constitution. But I sincerely believe, if applied at the proper moment, it would prove an efficacious remedy. Several were treated in 1801 in the same way, and nearly all recovered.

To the President, Officers and Members of the College of Physicians and Surgeons of Philadelphia:—

In the month of April, 1848, I sent a letter to Dr. James Bryan, your President, in which I gave an account of the medical treatment which I adopted in a case of cataract, which, together with several other cases of cataract cured by Dr. Bryan, and remarks on the same by him have been published in Dr. Houston's valuable Medical Journal (July number, 1848.). Having since had two other cases of cataract, which I have cured by medical means, I think it my duty to report them, to be read at one of your meetings.

When I say that I have cured two cases of cataract, I do not wish it to be understood that the patients had entirely lost their sight. You will perceive by reading the cases, that they could see a little when placed under my care.

I am aware that the editor of a Medical Journal in your city has said, that my curing a case of cataract was "all humbug." I hope it has not come to this, that a man must be put down because he has done what some other men supposed impracticable; if it be so, slow will be the advancement of medical science. This is a time of free thought, and it is the duty of every medical man to think and act for himself, and not be trammelled by any man, or body of men. But I have said enough on this point.

Case 1st.—May 4th, '48.—Mrs. D., aged 23, of delicate appearance, called on me to inquire if I could do anything to help her sight. She said she had been a hard working woman, had taken care of a large number of boarders, for a long time, doing the washing, ironing and cooking for the whole of them, in consequence of which her eyes had been much exposed to the blaze and heat of a large fire. She could not distinguish her own children from others, even when in the same room, unless they were close at hand; she could see best in cloudy days, at the close of day, and in the morning when she first waked up; every thing she saw appeared to be in a cloud. She complained of pain in the head and eyes. Tongue was coated, bowels costive, no desire for food of any kind. By looking into the eyes I saw (instead of that black appearance which we see in the pupil of a healthy eye,) a dirty appearance, being more observable in the centre of the pupil than at the circumference; the lens of the left eye being more opaque than that of the right.

I then told the woman and her husband who was present, that the disease was cataract, and that there might be a chance for improved vision. They then told me that they had called on a physician of this place, who, after examining her eyes, said that the disease was cataract. I then directed her to wear a pair of gray glasses during the day, and remain quiet in a room moderately dark; to live on a farinaceous diet, with half a pint of milk daily.

May 5th.—Applied four large foreign leeches to the external angle of the left eye, and three to the external angle of the right, applied a blister behind each ear, and at night gave her six grains of blue mass, followed next morning, with two compound cathartic pills, which operated well.

May 6th.—The pain in the head and eyes is diminished, blisters have drawn well. Still she has no appetite. Gave her the following powder three times a day until the 17th instant, when the mouth became sore:—hydrarg. cum cret. grs. ii, sup. carb. soda iv grains, and sulp. quinine grs. ss.

May 7th.—Feels some better. Applied five leeches to external angle of right eye, and three to external angle of left eye, which bled freely.

May 8th.—Eyes and head feel much better, and can see better, bowels regular, tongue clean, and appetite improving; blisters behind the ears have healed; I now put one on the back of the neck, which drew well.

May 9th.—Patient is comfortable. Can now distinguish her children from others in any part of a light room. She continued her medicine, kept the blister discharging until the 17th, when I suspended the mercury and continued soda and quinine, and, in addition, gave two grains iodide of Potassium three times a day. I this day, May 17th, applied three leeches to the external angle

of each eye, and placed a blister behind each ear. I continued her present medicine until the 29th May, occasionally moving the bowels by sulp. magnesia and senna, and keeping a free discharge from the blisters which I applied, alternately, behind the ears, and to the back of the neck.

May 30th.—I called on her; said she could see well, and also felt well. I then discontinued all treatment. Before I left the house I saw her pick up a small cambric needle from the floor without the aid of glasses. This day she had read a letter and wrote an answer to the same. She now, July 28th, does the work of her family, consisting of ten persons.

Case 2d.—*May 9th.*—Mrs. H., aged 50, called on me, complaining of pain in her head and eyes; said she could see but very little, and remarked that objects appeared to be in a mist. She has been subject to rheumatic pains in her limbs and head, with severe pain at the same time in her eyes. Her uncle has been blind, with cataract in both eyes, for the last fifteen years. She says she is afraid she shall be blind from the same disease, because, for several years past, she has suffered with the same symptoms as did her uncle for several years previous to his loss of sight.

Present state:—pulse strong and full; plethoric state of the system; bowels costive, tongue little coated, moderate appetite; pain in the limbs, joints, head, and eyes; cannot see any better than Mrs. D. The eyes presented the same change as those of Mrs. D., and the lens of each eye as much altered in color, the left one being the worst. As she could not remain in the place, but must return home, I advised her to send for her physician as soon as convenient, and request him to bleed her thirty ounces from the arm, which she did, and also to apply 12 American, or 6 foreign leeches in the immediate vicinity of each eye, and a blister behind each ear, directed her to take an infusion of salts and senna, with ten drops of the wine of colchicum seeds, every other morning, so as to procure three evacuations during the day, and to take 6 grs. of blue mass, every night; to live on bread and water, keep in a moderately dark room during the day, and wear gray glasses. She came again to see me on the 19th of May, and reported herself much better, had not applied the leeches, being unable to procure any in her town, but sight was improved a little. I immediately applied 4 large Spanish leeches to the external angle of each eye, the bites of which bled freely for 8 hours. She then becoming faint, I was obliged to arrest the bleeding. The next morning she left for home, with instructions to take the medicine as before, the mercury to be suspended when the mouth became sore, and to live on bread and water. *May 28*—sent me word that the mouth was a little sore, had discontinued the mercury; and that all her bad feelings and pains had left her, and her sight was rapidly improving—sent her word to take only the salts and senna every third morning, keep blisters discharging, and to live on more nourishing diet. *June 12*—this woman called on me, and reported herself well. I then examined her eyes, and found that the opacity was entirely removed. Last week I saw this woman's relations, who informed me, that she could see as well as when I last saw her.

I believe any medical man, after reading the above cases, and those published by Dr. Bryan, will come to the conclusion, that there are some forms of cataract which may be cured in their early stages, at least, without an operation. This being the fact, I think it is the duty of every medical man, when a case of cataract is brought to him, to do all that he can to arrest, or remove it by constitutional and local means, and not say 'I can do nothing, you must wait till the cataract is matured, and then run the risk of an operation.'

All writers on diseases of the eye, concur in the opinion that cataracts may be produced by congestion, common, or rheumatic inflammation of the different coats, or internal structure of the eye, their formation being attended in those cases with all the symptoms indicating an unusual determination of blood to the head, and frequently a general fulness of the system. This being the

fact, why cannot those cases of cataract be removed in their early, or immature state, by alteratives, counter-irritation and antiphlogistics?

Again, the eye contains specimens of all the animal tissues, and, of course, the same morbid changes take place in it, in consequence of inflammation, as in other parts of the body, and are as frequently removed by the same remedies. When we have inflammation of the pleura, we have the secretion lessened, but, most frequently there is an effusion of serum, and, sometimes, an excess of the nutritive secretion appears on the exterior of the membrane in various forms, and the lung may become secondarily affected. Now, inflammation produces the same morbid changes in the crystalline capsule. (which is a "sero-vascular membrane," and which "contains lymphatics on its inner or serous portion") in consequence of which we may have capsular cataracts, and if the lens becomes secondarily diseased, we may have capsulo-lenticular cataract. All physicians know that bleeding, purgatives, calomel, &c., will cure disease of the pleura, and regulate its secretions; and why will not the same remedies remove the same morbid changes in the crystalline capsule and lens, if resorted to before the absorbing properties of the capsule are much altered by disease? Sir David Brewster regards cataract, "as a disease which arises from the unhealthy secretion of the aqueous humor," and thinks that cataract may be cured in its early stages, and in proof of this, he adduces the case of his own eye, in which the disease had made considerable progress, and was cured by paying the greatest attention to diet, regimen, and abstaining from reading at night, and all exposure of the eyes to fatigue or strong lights. (You will see his views on this subject fully explained on the 626 and 7th pages of Lawrence on the eye, by Hays, published 1847.)—*Buffalo Medical Journal*, Oct. 1848.

5.—THE NOSTRUM TRADE—ITS INFLUENCE ON HEALTH AND MORALS.

EXTRACTS FROM THE MANUSCRIPT OF A MEDICAL FRIEND.

[Communicated for the Boston Med. and Surg. Journal.]

* * * There is, however, another department of the subject, not hitherto touched upon, of too much importance to be omitted; I mean the pathological relations of the organism, or its relations where already diseased, and especially its relations to remedies, real or supposed. Undoubtedly such a knowledge of the laws of life and of nature, as all men are capable of acquiring, might, if acted upon, diminish the present amount of disease to a comparatively very small remainder, perhaps eradicate it, or nearly so; but, while disease remains, it does not seem likely that any science attainable by the human mind can qualify unprofessional persons to prescribe remedies. Because, what is called the science of medicine is, and I think, must always remain, such a combination of science with art, skill, tact, and a medical sense, which can only be acquired by much observation of disease and experience of remedies, that, if all men were equally physicians, all must be unqualified and unskillful. The requisite observation would be no longer made—and the characters and changes of disease are often such as no science can enable the unpractised eye to detect; and the instinctive adaptation of remedies, which marks the man of skill, is impossible to inexperience. Only on the supposition of medicine becoming a science in the strictest sense of the term, can the treatment of diseases be committed safely to other than professional, that is experienced, hands. But, if we suppose all diseases to be curable by strictly scientific methods, that is, with infallible certainty, would it not be to have discovered a natural connection between transgression and impunity? To have found natural antidotes to the natural consequences of carelessness, indolence, vice, and every form, and degree of sensual indulgence, does it not imply a contradiction? Such a provision for the body at the expense of the soul, would it not imply, in the order

and design of Providence, a preference of the lower to the higher, of health to virtue, not compatible with the probable, or manifested, character of the Deity? An arrangement so necessarily and inevitably debasing in its influence, would it not impeach the goodness of God? Is the discovery of such a science of medicine probable, or even desirable? But not to insist on this view, to the abatement of the enthusiasm of those who reckon pain to be evil, and enjoyment good, it is plain that, until such a science is attained, or the elixir of the old alchemists invented, the management of pathological relations will be most safely intrusted to those professionally qualified for that most difficult duty. But if every man cannot, without danger, become his own physician, such a general knowledge of pathology as will convince him that he cannot, as will enable him to know when he really needs advice, that he may ask it in season—such an acquaintance with the nature and effects of medicines as will make him cautious and indisposed to poison himself in the use of them, as will unfascinate him in regard to the magic virtues of mysterious remedies, as will make him less intermeddling and thwarting of the wisdom of nature than ignorance is ever disposed to be, as will assure him that science promises to vice, and self-indulgence, and even to error, no inquiry; this amount of knowledge is within the reach of all, and is, for every one, among the most indispensable conditions of the preservation of health, and of the cure of disease. The tendency of science is to diminish the use of medicines even in sickness, and to bring back the patient, as fast, and as far, as possible, to the conditions, and obedience to the laws of health; that of ignorance, to expect not only the restoration, but the preservation also, of health, and counteraction of the natural consequences of loved indulgences, by the mysterious agencies of occult and wonder-working mixtures, spells, passes, pills and antidotes—still, as of old, believing that the sorcerers's mystic craft can control the powers of nature, and circumvent the ordinances of heaven.

The natural difficulties of finding, attaining and possessing the conditions of health are sufficiently great, the natural causes of disease are sufficiently numerous—but, as if these were not enough, men have found, in the all but universal resort, “both in sickness and in health,” to the use of secret and slow poisons, in the form of drugs of their own prescribing, an extra-natural cause, which, if we measure its effects, direct and indirect, immediate and remote, will be found entitled to take rank next in place to those most destructive of human life. It seems to be a sort of infatuation, under the influence of which men become equally the victims of their own ignorance and foolhardiness, and the dupes of deceptions so transparent, so gross and stale, and at the same time so adroit as rarely to lose their credit, and withal so wicked, that, perhaps, on no other occasion, do men manifest, on the one hand, such an incredible facility of being made fools of, and on the other, such a cool unblushing impudence in lying, and such a diabolical disregard of its consequences to those whom it deludes—a swindling process so monstrous, a reckless greediness of gain, a wholesale robbery, and taking of purse, of health, and of life, that would not hesitate to include also among its means the pistol and the poignard, if they promised rich returns with equal impunity. These are severe charges to bring against a class of men which has become both large and respectable, so far as wealth and consequent power and influence can confer respectability; a wealth, often almost without parallel, and an influence and power which, at the same time, exercise and extend themselves by subsidising and controlling (and it were more true than polite to say, bribing, and making particeps criminis in furtherance of their schemes of plunder) almost the entire periodical press of the country. The extent to which the pockets of the community are laid under contribution by the use of patent medicines may be inferred from the incredible amount of advertising, puffing, and other printing and engraving, to which they give rise (and if I am correctly informed, that kind of advertising generally pays an extra price,) this amount being, from the nature of the case, a pretty correct index and measure of the number of inventors, manufacturers,

venders and consumers, to which last, the circumstance of being obliged to foot all the bills, is the smallest and least harmful consequence of the purchase. It is but justice to say, that many of the agents of these results can plead the same ignorance as the ultimate sufferers, an ignorance, however, under the circumstances, little more excusable than the worse motives (we may say motives rarely paralleled in depravity,) from whose influence too many cannot plead exemption.

That I may not, by men willing to know the truth, be thought guilty of slander, let us examine somewhat the character and natural effects of the miracle-working nostrums, which, if their authors were not liars, would be, at least, an equivalent for the lost elixir of life. And we shall, perhaps, be enabled, at the same time, to account for the perennial delusions in regard to them, notwithstanding their palpable imposture, and the impudent, and sometimes blasphemous effrontery of their assurances.

The most common form of these poisons is, probably, that of cathartic pills; and in this form, from the greater extent and frequency of their use, I am inclined to think the amount of deleterious effects is greater than in any other. All these, with one consent, are commended to the stomachs of their dupes and victims, whether sick or well, by setting forth, beyond the doubt of all minds adapted to such logic, the stereotype quack pathology, that all disease, and all danger of disease, arise "from impurities of the blood," Hence it follows, inevitably, that these very pills, which are expressly adapted to that sole and all-sufficient end and purpose of purifying the blood, cannot fail to cure, or prevent, any, whatever disease, or danger of disease the purchaser may either feel, or fear. This conclusion is irresistible. They promise also, probably, to remove all gross obstruction, to improve the appetite, and enable the stomach to digest food without inconvenience. Persons suffering from the effects of indolence, sedentary labor of study, improper diet, unventilated dwellings, or some other transgression of the laws of life, sick or "not very well," find, in these adroitly-penned documents, which meet their eye in the daily or weekly paper, or some pamphlet put into their hands, their very feelings and symptoms so minutely described, and the obvious method of relief so kindly pointed out, that they cannot but make trial of so promising a remedy. And forthwith the promise, however extravagant, of the knowing impostor, is, much to the satisfaction of the patient, fully realized—for to one suffering from accumulated bile, or torpid and obstructed bowels, or overloaded bloodvessels, or the ennui of indolence, what, more effectually than an active cathartic, affords decided and immediate relief? The bad feelings, however, soon return, and probably with augmented annoyance; but the blood is not to be purified in a day, and the accompanying certificates show cures after the use of ten, twenty, thirty, fifty boxes!! Perseverance, therefore, is one of the elements of success; and he perseveres, with improving appetite, better spirits, a disposition to commend his new-found panacea to his neighbors, and a readiness to express his gratitude, and at the same time promote the good of "all others afflicted in like manner," by a "certificate" of benefits received. Partly, then, from a predisposition to faith in the unknown and mysterious, which is rarely wanting in the consumers of nostrums, "*omne ignotum pro magnifico*" (for the same preparations when they become known lose all their popularity—partly from the feeling, only apparently inconsistent with the other, that now they are admitted into the very secrets of nature—partly because the very extravagance of the lie gives it, to such minds, the air and power of truth, and because common honesty has no conception of pure fraud and villiany—and partly because excited expectations and hopes are (apparently) confirmed by experience, we may understand how the use of secret remedies is often commended, and why it is likely to be continued. There are also other reasons in the case of cathartic pills why their use should not be soon discontinued. For what really happens when their apparent good effects confirm delusion, and experience is the ignis fatuus which allures to destruction? Cathartics are substances whose

natural relations to the alimentary canal, the stomach and bowels, are such, that they stimulate, some more or some less, both their muscles, and their lining membrane, with which, when taken, they come in contact. Their effect, therefore, is to propel their contents, at the same time that they excite secretions into them; that is, they remove unhealthful, depressing, and otherwise annoying accumulations from the body, and by diminishing the quantity of its fluids, they relieve whatever organs are oppressed by a too plethoric, or an obstructed condition of the system. Hence it is manifest, that, to one who would gladly neglect the conditions and disregard the laws of health, or who ignorantly does so, the occasions would not be unfrequent on which he might find them a convenient, easy, and seemingly successful, substitute for both knowledge and obedience. They seem to ignorance, a natural provision for natural ills; and to disobedience, a gentle penance for any dear indulgences; and thus they encourage, and increase, by promising all but impunity to, such indulgences. It is not likely, therefore, that pills will cease to be popular, so long as the wonder-loving are made to believe that they work miraculous regeneration of the blood—the lovers of indolence, of study, and of sedentary habits and employments, that they can take the place of labor and of exercise—the lovers of the table, that they obviate the necessity, not only of fasting and abstinence, but of temperance, sobriety, and all observance of the natural laws of diet—and the lovers of dissipation, social and other excitements, that they can cure the consequent constipations, ennui, and nausea, neuralgia and migrains—and especially, so long as they not only undertake, but actually perform, all those kind offices.

But, moreover, it is true of this class of poisons, in common with all other unnatural stimulants, that their use constantly increases the seeming need and utility of them. At every repetition of the excitement of the muscles, and mucous membranes of the digestive canal, by these substances, they become less and less susceptible of the natural but milder stimulus of food, and of their own ordinary secretions. Their excitability is undergoing a gradual, but constant process of exhaustion. Hence, not only does the use of the more stimulating substances easily, and almost necessarily, become habitual, and nearly impossible to be laid aside, but their quantity or their strength must, for the same reasons, be gradually and constantly increased. Accordingly, the patent directions are to be sure and take enough, with the assurance that the pills cannot fail to cure you if only you take enough; and that the more you need, the more certain evidence it is of the *impurity of your blood*, and of your need to take more—that is, the more you need, the more you need, which is true for once, and proof of the truth of the proverb, that “the devil can speak truth.” These repeated excitements, however, and consequent exhaustion of the nervous energy of the stomach and bowels, and the frequent contact with their surfaces of such pungent and acrimonious stimulants, produce at length, as might naturally be supposed, still other results. Their mucous membranes, which are among the most finely organised parts of the body, become, first irritated, then inflamed, thickened, ulcerated, softened, corroded—their innumerable small glands destroyed or enlarged, swollen, tender, painful, permanently excited, and pouring out unhealthful fluids instead of their own natural secretions. But the perfect integrity of the structure and functions of the mucous membrane is indispensable, more, perhaps, than that of any other organ, not only to the existence and conscious feeling and enjoyment of health, but to the due performance of the functions of other organs; and their diseases often produce, sooner or later, both derangement and disease of the other great organs essential to life, of the kidneys, the heart, the brain, the lungs or the skin. Hence, as a natural and inevitable result, the whole miserable train of dyspeptic, hypochondriac, nervous and other complaints and diseases of the digestive organs and their consequences, too many, even to be named, except in a nosological catalogue. These, for the most part, come ultimately into the hands of the physician, too often to exhaust both his patience and his skill, and

to return again to some newest form of quackery, alternating between hope and fear, until Nature, who *never forgives*, and will not tolerate transgression beyond a certain point, puts an end, at length, to life and delusion together. Thus we see how these poisons, notwithstanding the palpable humbug written on the face with which they come forth, are, nevertheless, with a very devilish art, adapted to the character and circumstances of their victims—their apparent good effects, and the seeming success of the attempt to circumvent nature, extending and confirming their influence and control over the whole system, until it has no longer power to set itself free. And when the natural effects and ultimate consequences appear, they are so remote in time, and it is so uncertain when, and where, they will first, so decidedly as to awaken alarm, manifest themselves, that they are very rarely, in the mind of the sufferer, referred to their true causes, and men here as frequently on other occasions, father the legitimate offspring of their own ignorance, folly and vice, upon the inscrutable decrees of Providence.

The same misguiding experience, and the same final effects in kind, and for essentially the same reasons, follow the use of "Anti-dyspeptic Bitters," and other mixtures professing to cure indigestion, especially when prepared, as many of them are, with alcohol. They are a sort of natural symptom and accompaniment of the disease of pill-making. Cough medicines, in regard to the amount consumed, are probably second only to pills, and exceed them, if that were possible, in the outrageous extravagance of their pretensions, in which they "out-Herod Herod." These maintain their credit by a somewhat different method. They almost uniformly contain opiates and alcohol, and the first effect, generally, is to diminish the cough, and make the patient "feel better." And if, bye-and-bye, as a natural consequence in many coughs, they excite inflammation, and the cough returns with more violence than before, it is attributed to exposure, imprudence, accident, or any other cause than the true one. And when the disease is thus kept up to a fatal termination, as not unfrequently happens (an attempt with delusive opiates to smother the very fire that they often keep burning), the cheated victim dies with regret that he had not procured the medicine sooner, since, "it did him so much good, that it would have cured him if he had taken it in season." Frequently, too, they are used in coughs which are going through a spontaneous process of cure, which is natural to most coughs, and if they do not happen to prevent it, they of course carry off the credit of the recovery, for with ignorance the "*post hoc ergo propter hoc*" argument is one which may always be confidently relied upon. Sometimes, also, undoubtedly, when given in a stage of the disease to which they may happen to be adapted, very unpromising cases have, unfortunately, done well under the use of them. I say unfortunately, for every such case of success gives them a credit by which they kill fifty. A great variety of diseases, differently seated, owing to different causes, and requiring different treatment, often go under a common name, and this is especially true of those attended with cough. Almost every disease, too, is a process going through different stages, and requiring, if any treatment, different treatment in different stages, inasmuch that what would cure the patient in one stage may kill him in another. Now it must be obvious to all, except believers in oracular quack wisdom, that if a medicine, which happens to be suitable for some one stage, of some one disease called a cough, and directed to be cured by cough mixtures, is given indiscriminately through all the stages of the whole number of different diseases, that the chances of benefit are to the chances of injury, as one is to the whole number of stages of all the diseases. And such is just about the proportion of cures effected by this class of nostrums. In what proportion of cases they destroy life, or prevent spontaneous or other cure, and otherwise injure or destroy the general health, cannot be known, until certain accounts are rendered, likely hereafter to be called for. It is not without good reason that almost every patent nostrum claims to be a *panacea*, the one all-sufficient remedy for every disease; for otherwise, unless it can on all occa-

sions give directions in regard to itself, when it ought to be used, its author cannot by any possibility absolve his conscience of the guilt of a most malevolent disregard of human life, not to say a malevolent taking it away. And it cannot be pleaded in extenuation that the medicine is wholly inert, (though many of them are so,) and therefore harmless; for one of the great dangers in the use of self-prescribed or irresponsibly advised medicines, arises from the fact that often, in attacks of disease, such that life depends upon prompt, efficient and skilful treatment, they are tried and trusted in until all remedy is too late. And he whose wilful lie, its probable consequences being known, indirectly causes death, how is he less wicked than he whose false witness or whose dagger takes the life of his neighbor?

There is still another class of these accursed drugs which bids fair to outrun the popularity even of pills. This consists of various compounds, in the form of syrups, or tinctures, which come forth thick as the spawn of Egypt, under the common name of sarsaparilla. These profess, not only to "purify the blood," and cure all the ills that flesh is heir to in general, but to purify the skin also especially, and in particular to eradicate *scrofula*, that perennial quack bugbear, and to have power over old sores, rheums, scabs, itches, ulcers, cancers, tumors, and other like such unseemly ailments. As the concoctors of sarsaparilla (which, however contains none of that inert substance) have learned that cases of this class, often found difficult to cure, are sometimes benefited by the most active and dangerous articles which physicians can employ, and which need to be used with great caution in order to avoid future bad consequences, they do not hesitate, with utter contempt of human health and life, so they "put money in their pockets," to drug their otherwise wholly inefficient mixtures with corrosive sublimate, iodine, arsenic, &c., with the cool expectation and hope that by bringing them into indiscriminate, and as far as in them lies, universal use, some lucky chance-cure may increase the credit of their poison, and of course the profits of their trade. If they succeed in this, as, in punishment of the sins of men, they now and then do, the whole press teems with puffs, pictures and pamphlets, and a wholesale poisoning commences of as many as fear or fancy that they may be guilty, either by acquisition or inheritance, of any "impurity of the blood." In the meantime, if, as is likely, the dose is made palatable with spirits and bitters, every body "feels better," "cannot do without it," while the poisonous effects are insidious, remote, unrecognised, and probably (an undesigned homœopathy) their cure attempted by the continued use of the very agents which produced them. If the god of gain in any other men prompts a more purely devilish depravity than is here manifested, they are gentlemen of whom I have not heard.

Such are the character and natural effects of a few of the untold multitude of pick-pocket preparations with which, more unfortunate than Egypt of old, "the whole land stinks." So that from the infant, who, if drugs and other "dunned practice" can do it, is murdered in its mother's womb, or quieted in its cradle, to him who escapes longest, few escape wholly the arrows of this artificial pestilence, which both walks in darkness and poisons at noon-day.

The dangers from the popular and general use of these boastful compositions are of three kinds—dangers which do not arise from natural domestic remedies, of which it is known what and how much they are capable of accomplishing. The first is the natural and inevitable tendency of many of them, as pills, bitters, and preparations of opium and alcohol, when their use becomes habitual, to destroy health and ultimately life; and the nature of their effects almost ensures their use becoming habitual if it be once commenced. To these may be added the more active poisons which still sooner infect the system, although their effects may not always sooner manifest themselves. The second danger is of ignorance in prescribing and giving medicines, which might sometimes, in skilful hands, be useful, in the wrong disease or in the wrong stage, in which cases the effects may be fatal by being directly the contrary of those expected and promised. The third, and perhaps the greatest danger of the three,

is inseparable from the whole miserable list without exception, that they are liable to be confided in, and especially in attacks of acute disease, until the remedies which might have saved life are no longer available. Most appropriate here would be the proverb which forbids to play with edged tools, for medicines are two-edged instruments, which may be hurtful or fatal both in the misuse and the non-use. Yet however obvious it may be to one acquainted with the subject, or even to a moment's reflection, that he has need to be well instructed who gives either direction or advice where health and life are involved, how few of either sex, even the least competent, scruple to take upon themselves that responsibility, with a readiness and officiousness, which, whether they indicate the more profound unconsciousness of their own ignorance, or the more stupid disregard of consequences to others, it were not easy to determine. But if it ought not to be thought strange that ignorance, which hazards its own life, should hesitate to hazard that of another, ought it not to be reckoned passing strange, when physicians, and that not the mere rabble of the profession, but men of character and reputation, lend their names to promote the circulation of sarsaparilla? "The contents of the bottle have been made known to *them*, and *they* think it may be useful in some cases—they would advise physicians to use it." And *that* appended to a mixture puffing its way into popular use! A thoughtful man's astonishment can be equalled only by his indignation, for if we could suppose (which would be very exemplary charity under the circumstances) that they intend to encourage its use by the profession only, do they not degrade the rest of us at once to quacks? For who else than a quack is inspired to know when, or how, to use safely a medicine of which he knows not the ingredients? Can a man of science, or of conscience, prescribe unknown nostrums, unknown to himself, because, perchance, something contained in them may hit the nail on the head? Nothing, surely, can explain such facts, except the supposition of the most incredible thoughtlessness; or else, which in some cases cannot be supposed, of the influence of the power which is so potent over the press.

There is another fact very incredible to an honest man. Physicians themselves have put forth patent mixtures for popular use; but, by way of salvo to their consciences, they have directed—at least, some of them—that the vendors shall make known, *to all physicians*, the ingredients of which they consist. Alas! my brother, had you not first forgotten to include in your morning prayer, "Lead us not into temptation?" Do you think that your apparent frankness and willingness to appeal to the faculty that it is a "*good medicine*," will diminish its danger when prescribed by others than the faculty—the danger of its indiscriminate, injudicious, ill-timed and ill-adapted use? Is it a *panacea*? Has it such wonderful properties, that, taken in health, it works no harm, and that given in *any* disease, in *any* stage of it, in *any* quantity, it cures it so infallibly that no other treatment can ever be necessary? so that it can never be ill-judged or ill-timed? That, sir, is the medicine to put into the hands of ignorance; a sharp instrument indeed it is, but the edge is always on the right side, and the most careless man can neither hurt himself or any body else with it. The out-and-out quacks are more consistent in their pathology—one disease and one remedy! That, sir, is the true law of humbug—it is simple, beautiful, universal! Like the law of gravity, not encumbered clumsy exceptions on which unskilfulness may stumble. But yours is a medicine only for coughs. Well, then, will it cure any coughs? or, not to make the test too severe, will it cure any curable cough? Will it cure any curable disease, in any stage of it, of which cough is a symptom whether that disease be in the head, the throat, the lungs, the liver, the stomach, the spine, the kidneys or the heart? for disease in any of these organs, and others besides, as you very well know, or ought to know, may excite cough. That is, your cough-mixture, in order to be safe for unskilful use, must be *the* one medicine for the one disease. Or until that discovered, or at least a specific for each disease, and people have learned to distinguish one disease from another, is it likely to promote the health or morals of the community, that innumerable pois-

onous compounds, mendaciously promising impunity to every vice, and exemption or relief from every disease, should be used carelessly and freely as water, until they are supposed to be more necessary, and are found to be more expensive, than daily bread, so that new clause would be appropriately added to the Lord's Prayer, "Give us this day our daily medicine?" Prythee, dear doctor, for the good of your neighbors and the credit of your profession, leave quackery to the quacks, and study the laws of life, and perhaps you will find that many diseases may be relieved with very little medicine; and where that cannot be, since it is always an evil, let it be the least of the two, that the remedy, if it cannot be avoided, may yet, which does not always happen even in professional hands, prove somewhat less dangerous than the disease.

If, now, we consider the various aspects and bearings of this subject, will it not be obvious to reflection, that nothing can deliver the community permanently from the constant invasion of ever new swarms of the unclean and all-consuming locusts of quackery, in which its being annually robbed of untold millions is but dust in the balance in comparison with the waste of health and of life, that nothing can uproot this stupendous scheme of fraud and villany, except such a knowledge, universally diffused, of the laws of life and health, of the general principles of pathology, of the natural relations of the substances called medicines to the human body, and the ways in which, here, experience is so liable to become delusive, that there shall be nowhere left any darkness of ignorance in which these workers of delusion and iniquity can hide themselves? For what else than such a light of knowledge in their own possession, can give vision to men who not only are readily and as it were willingly led astray, but perversely and wilfully reject every proffered light and guidance derived from the knowledge of others? It is quite in vain to assure them that they are being cheated in their pockets, injured in health, and endangered in their life. Ignorance is jealous of knowledge—sometimes not without reason—and it is often the more confirmed in error the more you attempt to dissuade it. If the physician opens his mouth against quacks and quackery, the suspicions of those he would benefit are at once awakened:—it is evidence that his craft is in danger;—and the mischief he seeks to diminish is only increased. And it avails nothing to attempt to convince them that quacks are our fast friends, and furnish us more business than any other single cause of disease. They will understand it that the practice of medicine is a monopoly, and that "free trade" in pills is its natural enemy. It is a common opinion among mankind, and has been since the days of faith in signatures—a faith not yet extinct—that medicines are natural remedies, in the bounty of Providence, for natural ills, diseases being the inscrutable infliction of Heaven. Nothing, therefore, is more difficult than to persuade a patient to be cured without, or with very little, medicine. He will swallow any quantity of drugs, but just in proportion as you withhold *them*, he withholds his confidence. And advice and direction to return to the conditions, and to obedience to the laws, of health, besides that they may require what is out of his power, and certainly require a very difficult energy and self-denial on his part, indicate also to him, on the part of the physician, ignorance of the "healing virtues that reside in plants medicinal," and he dismisses him for a less scientific but more pragmatical adviser, or launches himself, under his own pilotage, upon that sea, which, like Sir John Mandeville's sea of sand, "no man mought pass," the shoreless sea of quackery.

An honest man might be well nigh tempted, as probably some have been quite tempted, to undertake the cure of humbug *nomœopathically*; for if he but combine science with some harmless charlatanry, which, like the stone in Gil Blas's broth, shall serve as food for wonder, and moreover keep up his credit, and convince gainsayers that "some things can be done as well as others," he will find comparatively little difficulty in enforcing obedience to what science may direct. A strict regulation of diet and sleep, with active exercise or labor in the open air, and other conformities to Nature, will cure many chronic complaints incurable by mere medicine; and the millionth part of

a grain, even of chalk or magnesia, will not often interfere with the process; and a reasonable quantity of hydropathic dipping and drenching, not only does not impede it, but, in many cases, is quite indispensable to its success.

Thus it is manifest beyond all, or at least beyond all other than captious, question, that knowledge, the active and energetic exercise of the voluntary function of the brain, is, for every individual, a condition never to be forgotten, of the realization of the very first ends of the organism—its own perfection and preservation. This function can never safely remain latent. The machine cannot work out these products without it. For if Nature required of us less exemption from error, if she were more lenient, and there were less danger from her displeasure, how should we defend ourselves against *men*, always, and every where, the most dangerous enemies of *man*? But not only must there be cultivation and activity of the intellect, there must be knowledge of these very laws and relations—of the human body of which we have been speaking. Yet here, out of a part of the medical profession, ignorance is all but universal. An ignorance which even public education almost, or altogether, overlooks. Hence educated men often have so little of this knowledge, which is the condition of the value and efficiency of all other knowledge, that they not only forfeit their faculties, health and life, and at the same time, not unfrequently, reckon themselves martyrs to duty while in the violation of its very first principles; but for the same reason, the otherwise better informed classes of the community, and especially the clergy, are by no means among the least liable to medical delusions, and are certainly the most efficient, because the most influential, promoters of the interests, and of course of the mischiefs, of medical empirics and mountebanks. Two popular errors are, if not innate, ingrained in the human mind. They descend, if not by propagation, by some other transmission, from generation to generation. Like original sin, they are an hereditary taint of the race. One of these is, that diseases are an arbitrary and mysterious infliction of the Deity; and the other, that health is, often, to be pursued, and always restored by the use of substances called medicines, preventive or remedial, externally or internally applied. The consequence is, that instead of investigating accurately the condition of health in order to prevent disease, we remain, for the most part, stupidly or wilfully ignorant until the evil has befallen us, and then, with officious and intermeddling distrust, instead of co-operating with the restorative powers of Nature, by placing ourselves under their influence, and yielding obedience to her laws, we place ourselves in still more unnatural conditions, do ourselves up in outside skeletons, and commence to swallow in succession all known substances, if perchance the antidote to our disease may be found among them. Before these false opinion, the basis of our delusions, can be eradicated—before we can conform to all the conditions of the preservation of health, and of its restoration, when the offence against nature is not capital—before we shall avoid increasing the ills we ignorantly attempt to remove—before we can understand the natural relations of our own organism inward and outward, so far as they are directly or indirectly under our control, of how many departments of Nature and of man must we first acquire knowledge! And before all these relations are so adjusted that man shall attain to his natural period of life, we shall be convinced that Nature does not intend the brain to be, for any, a super-numerary organ, and that the full performance of its voluntary function, not less than that of the voluntary muscles, is one of the indispensable conditions of our physiological well-being.

6.—*On Milk Fever, and Abscess of the Mamma.* By J. H. SHEARMAN, M. D., of New-York.

In a former number of the Journal, I treated the subject of pregnancy showing that they all arose from the altered condition of the blood, consequent on, or incident to, the progress of *gestation*. I propose in this article, to show that the diseases consequent on *lactation* arise from the same cause—the altered abnormal state of the blood.

I have previously laid down the position, that health invariably requires the blood to be in its natural condition, as respects the number, the quantity and the quality of its constituents. Excess, deficiency, or impropriety of the constituents of the blood, is fatal to the pretensions of health.

All poisoning—all fever—all inflammation, indeed all disease, is attended with, preceded by a change in the condition of the blood. If a person die from taking opium, arsenic, prussic acid, or any other mineral, vegetable, or animal poison, the cause of the mischief is, the poison in the blood, and the change produced in its constituents.

It is true, that certain philosophers have propounded to us that there are some poisons which do not operate upon the blood, but simply on the nerves, or even—finer still—the nervous influence. For my part, I have no fear of such poisons. If they do not chemically act upon the blood, there is no cause to be afraid of them; water itself cannot be more harmless. In what way do laughing gas, chloroform, and ether operate, but on the blood, by being inhaled? In what way do these substances cease to operate, but by being exhaled, or passed off from the blood some other way? The philosophy of matter, whether organised or unorganised, is one of tact and contact—composition and decomposition—formation, deformation, and transformation; and all attempts to elude this—the true Baconian philosophy—end in transcendental fooleries, and lead men to those regions, where “entity and quiddity, the ghosts of defunct bodies ily.”

The condition of lactation is a natural one, not necessarily diseased. The glands of the breasts have been enlarging and multiplying during gestation, preparatory to their function being called for; and as soon as the sustentation of the *fœtus* in the womb, by the direct use of the mother's blood ceases, the process of sustentation at the mother's breast commences; by the infant imbibing, digesting, and assimilating the mother's milk.

In this phenomenon of lactation, two opposite processes take place in the mother and child. In the mother, the blood which formerly went by the uterus to supply the wants of the child, now goes to the breast to form milk for that purpose. In the child, the blood which formerly was supplied direct by the mother, has to be made from the milk by the process of digestion and assimilation.

Both are chemico-physiological processes, and neither of them ought to be attended with disorder—yet how often both are. So frequent, indeed, is the occurrence, that it is regarded as the natural condition. I hope to show that this is an error of a gross and dangerous kind; and that manifest impropriety and mismanagement on the part of the mother and her attendants, or both, are the remote, if not the proximate cause, of most of the ailments of lactation, as well as of gestation.

The process of labor is a violent action of the parturient organs, but it is a natural process. It is not like fever, nor inflammation, nor a rupture, nor a luxation, nor a wound, nor a fracture. Any of these, indeed, or all of them, may accompany labor, but none of them is the labor.

The functions of mastication, deglutition, digestion, defæcation, and micturition, are not more strictly natural than parturition. Any one of them may be accompanied with disorder and danger, and may terminate in death; but it is by no means the necessary consequence of such functions.

On the birth of a child, and expulsion of the placenta, there is of necessity, an escape of a certain portion of blood from the placental attachment of the uterus—in most cases sufficient to be considered a large depletion—ample enough to relieve the turgescence of the vessels, whose functions of nourishment have just ceased so suddenly. I suppose that in every natural labor, the quantity of blood lost is exactly what is needful to be abstracted. The lochia are simply the drainings which ooze out from the divided vessels, until they are united and closed.

Now begins a new process—the formation and secretion of milk. This is as natural a process, and ought to be as healthy a one, as the formation and secretion of bile or urine. The milk is a composition which differs from blood in many important particulars. It contains more water, more of the phosphates of lime, potash, magnesia and iron; and contains caseine butter and sugar, which blood does not contain; and it does not contain fibrine, albumen, and hematosine, which are found in blood.

The elements of nutrition were *decerned* from the blood of the mother, in the body of the child, whilst in the uterus; now they are *secreted* from the mother's blood in the breasts. The condition of the child in the uterus did not require more than a portion of the contents of the blood which was sent there, and that condition is still existing.

Bone for the frame-work of the body; brain and nerve for the sensations; skin and fat, for covering and warmth, are the principal things required. The milk contains the elements requisite for these purposes. The muscles of an infant, are a very slight species of fibrinal texture, compared with those of a more matured person, and every tissue is less firm and solid, than in those of man and woman. The flesh of a child, indeed the flesh of all all young animals, is tender and delicate.

The food of young persons is, therefore, of necessity, different from that of older persons, and infants require lactaceous food, not merely because it is provided for them, but because it is their appropriate sustenance.

The secretion of milk from the blood, takes some elements away from it which are necessary to its integrity, and, by consequence, disorders it, if not restored. When this first occurs, there is a change in the sensations and appetite. Usually there is some headache, loss of sleep, and slight fever. Is this wholly natural? Let us examine.

The elements which are taken out of the blood, are the phosphates of lime, and alkalies, a highly nitrogenous compound, caseine, and the triple compounds of butter and sugar. The blood, then, instantly lacks these elements; and to keep it in its proper order, must have such food as contains them. What is the usual food for women under such circumstances, as prescribed by grave and learned physicians, growling and dogmatic nurses and grandmother-taught mothers?

Gruel, composed of the *albuminous* products; fluids containing the *farinaeous* or starch products; panada or bread and water; and *gelatinous* fluids; some of them with salt, some with sugar and a little spice. These substances cannot supply what the blood needs, and they are very frequently so distasteful that they are taken as medicines, or, as it is curiously termed, "dieting." Of all ignorant assumptions of our vein conceit, that of interfering with the instinct of appetite—which is the infallible demand of infallible laws—and not only placing our arrogant veto upon compliance with it, but reversing its orders; this is certainly the height of impudence in "learned ignorance."

Happy is it for the mass of mankind, that they are so seldom under "sound medical surveillance; or they would never have one day's comfort nor enjoyment; and by consequence, never have a day's health. Do but deprive a man of his agreeable food, and give him a little calomel and antimony at night, and a little Epsom salts in the morning, and if he ever be well, a perpetual miracle must be wrought for him.

If a woman, performing the important function of lactation, at any other

period of its existence than the parturient one, is compelled to live on such diet as the lying-in one, she is pretty certain to have fever, with headache and diarrhœa.

How utterly unobservant the mass of medical men are, on the subject of food and appetite. The daily occurrence of person's loathing one kind of food, and yet longing for another, of person's persisting in long-continued fasting on one sort of food, with a constant appetite for food of another sort, of irremediable disorder attending one sort of diet, and restoration to health attending another, seems to have no effect on the mass of medical men. Until an authoritative precept on the subject occurs in some standard work or is uttered by some famous lecturer, all the phenomena of nature, are worth nothing as instruction to the practitioner.

A woman, of apparently good health, but anxious countenance, carrying a child in her arms, called on me for advice for a distressing headache. I inquired if she suckled the child which she carried. She said she did. I wished to know what her food was, and she informed me that it was bread and butter, or crackers and tea. I inquired if she could not take any other kind of food. She said she longed for good meat vegetables and fruits, but that she had been forbidden to use them. I told her to consult her sensations and trust to those, in spite of all the advice in the world. She did so, and her headache left her.

A lady of my acquaintance, but not my patient, who had suckled two or three children, and took great interest in poor married women, thought that there was no necessity for catering so much for the appetite when suckling; and that if the rich were to show how easy it was to suckle a child through the night, on a basin of gruel, instead of mutton chops and other substantials, it would be a good example and lesson for those who could not so easily procure them.

She therefore commenced her gruel regimen, and in spite of some qualms of stomach and quakings of head, with womanly perseverance continued her gruel, until vomiting, purging and fever ensued. Her husband being a man of good understanding, traced the whole of her disorder to the change in her food. She returned to her former substantial diet, and was cured of her maladies, and meddling with the diet of the poor.

To me the horrors of the Inquisition, where the ignorant bigotry of the priest racks the body with tortures, in the vain hope of crooking the mind that the divine laws of intellect make straight, are not more dreadful than the ignorant presumption of the "medicine man," who inflicts upon his "*patients*," (too true alas,) the denials of his prohibitions and the endurances of his administrations, in the vain attempts to force an organised being into order and health, by violating the immutable and irresistible laws of nature.

If, when a woman commences the functions of lactation, she have that kind and quantity of food, which her appetite requires; if she have good air to breathe, light enough to minister to the activity and gratification of the senses, warmth enough for comfort, and no more, as much of the external application of water to the skin as will purify and invigorate it, as much of the internal use of water as is required by her sensations, and "the liberty of the functions;" there is little doubt in her being in health. Woman was designed by God to bear and suckle children, and it is high treason against the wisdom and goodness of the Most High, to suppose that she must forfeit her health to perform her duties.

In most cases of lactation that have come under my notice, there has been a constant strife waged with nature, by the mother and her advisers, from first to last. The woman who undergoes the process of parturition and performs the office of suckling, is removed from the influence of the benign laws of her nature, and put under the care of some man who has no knowledge of the right or wrong of her condition, acquired by observation; but is a very obsequious follower of the "routine" of "obstetrical science," "practice of midwifery," and "puerperal observations," of renowned authors, or famous lecturers.

She is required to give up her judgment and feelings to the direction and control of old wives and nurses, and almost everything which she desires is forbidden, and everything which she dislikes is forced upon her. A drink of cold water, though longed for like air by a fainting person, is denied her, on the pretence that it would chill her milk, and disorder her infant. Fruit is forbidden on the assumed ground that it would cause "acidity." Butter and fat are "bilious." (Oh, that some one would tell us what "bilious" means!) Vegetables are "windy." pastry is "heavy," new bread is "poison." There is nothing left but dry bread, lean beef or mutton, oysters and crackers.

This is not the place for a dissertation on food; but as the food of a body composes the blood, the fluids and the solids of that body, the more delicate and important the processes and functions to be performed, the more discriminating and particular will be the appetites and desires, and the more certainly will disorder ensue, if the proper food be withheld or unsuitable food be administered.

Once, let the blood lack some of its requisite elements, or contain too much of others, and there is at once a breach of the corporal soundness and organic integrity. The particles of the solids will not be deposited as in health; the fluids will not be secreted in their normal condition; the caloric of the blood will not be expended on the life and activity of the organs and muscles of the body; and, therefore, of necessity, will accumulate, and shivering and heat, headache and nausea, lassitude and pain, will necessarily occur—fever will appear.

In most cases of lactation, when the milk begins to flow, there is swelling of the breasts, with some heat and pain. The swelling is occasioned by the large quantity of milk which is secreted, compared with the quantity which is used by the child. The heat is caused by the pressure obstructing the circulation, so that the caloric is not only *mechanically forced* out of the solids and fluids, but also *chemically accumulated*, and given off in a free state, instead of being *chemically united* with the solids and fluids in the processes of life. The pain is produced by the two former conditions.

It is to me extremely doubtful, if any case of painful swelling of the breasts ever occur to a woman living according to the instincts of her being—obeying the laws of her nature. The only cases of it that I have ever known, have been invariably caused by, or at least accompanied with, the most marked neglect of, or resistance to, the demands of nature.

The room has been kept hot, close and dark; the body covered with a disagreeable excess of clothing—the breasts especially have been loaded, and all air carefully excluded from them. The smallest portion of cold drink has been forbidden like poison; and hot drinks administered, as though the preservation of life depended upon them. When the breasts began to fill, heating applications were applied to them, as brandy, oil, camphor, and other stimulants. If, indeed, the breasts had not swelled, they would most wonderfully have resisted every inducement to swell.

To such extremes of opposition to nature is this puerperal treatment carried, that it is thought dangerous to be clean and wholesome. To wash the body, and change the clothes and sheets, would be deemed an act of outrage to the woman—inevitably giving her cold and producing fever!

Let us now see what part the medical attendant commits in these outrages on nature. He administers some sort of medicine, usually such as opium, antimony, calomel, nitre, rhubarb, senna, castor-oil, magnesia, &c. If he do not, what sort of a doctor is he thought to be? The observation of a nurse to me once, during my attendance upon a very sensible lady, who knew that obedience to the requirements of her being was of more value than many medicines, is precisely in point. "Of what use is a doctor," said this nurse, "if he do not give medicine, and interfere with diet?" Well-spoken ignorance.

The medical attendant has been educated to meddle with every thing in his way. He directs the light to be excluded—air to be carefully shut out—extra

covering to be put on—"a gentle perspiration to be kept up" (what a sweet simpering piece of cataphasmal advice!)—recommends some particular sort of gruel as very "nourishing"—chicken soup, or tapioca pudding as very "strengthening without leading to fever or inflammation"—and suggests that too frequent change of bed and body linen is "very weakening!" "An absolute gentleman, full of most excellent differences, of very soft society, and of great showing!"

If there be any fever under all this maltreatment—and why should there not be?—some calomel and castor oil are given as "revulsives;" and some "neutral salt" as "refrigerant." If there be aphthous mouth and tongue—as how could it be otherwise?—some soda and magnesia are administered, in order that the "acid crudities" of the offending stomach may be "corrected." Profundity of ignorance!

Were the physician but for one week compelled to suffer his own regimen, he would learn more of physiology and true pathology than by a three years' course at any College in the world. Suppose a person call upon his watch-mender for advice and remedy for some disorder of his gold repeater; and after looking, listening, thinking and considering, the watch-mender prescribes for it, "after the art of the apothecary."

He drops a little oil into the machinery, and tries what that will do. If that do not succeed, he next drops in a little caustic alkali, some potash or ammonia. If that be unsuccessful, he tries some powerful acid, the nitric or sulphuric. If this be unavailing, he boils it like an egg for fifteen minutes. If this should fail, he puts it into ice. Should none of these succeed in remedying the mischief, there is nothing left but to submit to the condition, after such a display of science has been expended without any sensible avail.

Such is the treatment of a woman in confinement and lactation. Darkened and suffocated, sweated, purged and starved, forbidden all agreeable food, and forced to take what is disagreeable, and swallow more or less of poison. What can a woman be under such circumstances, but a mass of misery and wretchedness? Once let her follow the unerring instincts of her nature, and parturition is at once relieved of half its dreaded sufferings. An Irish peasant's wife, or an Indian Squaw, if she can have what her unpoisoned, unperverted instincts want, is better off in the performance of her functions of gestation, parturition and lactation, than the queen of any empire in the world.

I shall pursue this subject in the next number, when I shall treat more of the practical application of my views.—*N. Y. Jour. of Medicine, Sept., 1848.*

7.—*On the internal use of Nitrate of silver in obstinate Diarrhœa and Dysentery.* By THOMAS AIKEN, Esq. (Dublin Med. Press.)

The author of this communication remarks, that the topical application of the nitrate of silver to inflamed or ulcerated mucous surfaces, is confessedly a most efficient mode of treating such cases. The knowledge of this fact may have induced physicians to employ the same remedy internally, against disease invading the mucous surface of the hollow viscera. Accordingly, we find that ample testimony is afforded to the efficacy of the nitrate of silver in certain morbid conditions of the mucous coat of the stomach; but no English writer, Copland excepted, (*Dictionary of Medicine*), sanctions its employment as a therapeutic agent in morbid conditions of the mucous surface of the intestinal tube. The author's object in the present communication, is to adduce such testimony in favor of its sanative power in these affections, as may stimulate further inquiry into the action of this salt in certain obstinate forms of diarrhœa and dysentery, which occasionally resist the action of the most esteemed remedies, wielded in the ablest manner.

Boudin, (*Gazette Med. No. 51, 1836*), physician to the Military Hospital at

Marseilles, treated fifty cases of typhoid fever (dothi-enteritis) in most of which severe diarrhœa was the most prominent feature, with the nitrate of silver, thus:—When the lower portion of the intestinal tract was presumed to be the seat of ulceration, enemata, containing from one to three grains, dissolved in distilled water, were administered. In most cases, one enema sufficed, the symptoms undergoing speedy amelioration. In other cases, the remedy was given by the mouth, in half grain doses every half hour, [?] formed into pills with gum tragacanth, or starch, until from two to four grains were thus taken. In some instances, these two modes of treatment were combined: the results were, that only two of the fifty cases succumbed. Examination showed “many ulcers” on the mucous membrane in a case of incipient cicatrization—“*en voie de cicatrization*.” There was evidence of the solution administered per rectum having passed the ileo-cæcal valve, and producing effects on the lower portion of the ileum precisely similar to those resulting from its action on the surface of the colon.

Kalt confirms Boudin’s statement, having treated twenty-two cases of dothi-enteritis with the nitrate of silver. Of these, one died. He gave it in mixture (grs. ij. to vj. in decoct. salep. oz. vj.) a tablespoonful of which was taken every half hour, or hour, according to circumstances.

Hirsch, of Königsberg, (Hufeland’s Journal,) found the nitrate of silver to succeed in obstinate cases of diarrhœa, on the failure of ordinary remedies. It proved specially useful in the diarrhœa of newly weaned infants. In “the advanced stage of such cases, when emaciation was extreme, the dejections being frequent, fetid, and consisting of a variously colored, sometimes greenish, or bloody mucous, and wanting altogether the fecal character. When aphthous ulceration pervaded the mouth, and when prostration was extreme, the action of the nitrate was brilliant.” He gave it to children thus.—R. Argent nitrat. crystall. $\frac{1}{2}$ gr., aqua distill. oz. ij., gum mimosæ scruple ij., sacch. albi, ij. drachm. Misce. Ft. mist. A teaspoonful of this mixture was given every two hours, and an enema, containing a quarter grain of the salt, with mucilage and a little opium, was administered. The good effects of this treatment were occasionally visible in a few hours—sometimes not until the second day. He pronounces it a specific in the diarrhœa of infants. He found it almost equally efficacious in severe forms of diarrhœa and dysentery occurring in adults. He administered it to the latter in pills, in doses varying from one-twelfth to one-twentieth of a grain every two hours. For this purpose, he recommends liquorice powder as preferable to the vegetable extracts, which effect its decomposition. He also gave enemata, containing half a grain or a grain with mucilage and opium.

Canstatt also extols the nitrate of silver, as prescribed by Hirsch, in the diarrhœa ablactatorum.

Since the author became acquainted with Hirsch’s observations, opportunity presented for testing the powers of the nitrate of silver in a severe case of diarrhœa, occurring in a child of a year old. Vomiting and purging set in, and continued with almost unabated intensity for five days.—The stomach at length retained fluids in small quantities, but the purging continued. Chalk mixture, kino, opium, and acetate of lead, were tried, and all, with the exception of the last, seemed to increase the irritation. The dejections were frequently greenish, sometimes bloody, and very fetid. On the sixth day, prostration was very great; there was a tendency to stupor, and quantities of greenish mucus were voided. Under these circumstances, he gave the mixture as prescribed by Hirsch. The first dose seemed to increase the discharges; however, in about six hours, the character of the dejections was improved; they became feculent, and every symptom underwent a corresponding improvement.

Should the foregoing observations induce practitioners, in this country, to subject the action of the nitrate of silver, in the diseases of the mucous surface of the intestines, to a more extensive trial, they may arrive at results confirmatory of those already obtained by the authorities which the author has quoted, and thereby extend the application of an agent of great therapeutic energy to

forms of disease, occasionally so intractable as to baffle the powers of ordinary remedies.

[The advantage of the nitrate of silver in the diarrhœa of infants, of which we have had considerable experience, is also acknowledged by Bouchart, (Manuel Pratique des Nouveaux-nés,) and by Troussseau. We have given it frequently, and with much benefit also, in the "irritable" bowels of the adult. We generally prefer to exhibit it in solution, more especially in children; since, if given in pill or powder, we have no guarantee that it will not, by suddenly dissolving, exert all its effects, which, in that case, may be too powerful, upon a circumscribed portion of the mucous membrane. This is a point which is not sufficiently attended to in prescribing the nitrate of silver in gastrodynia, and sufficiently accounts for the diversity of opinion respecting its benefit in this complaint. It may be readily conceived that it makes all the difference, whether half a grain of solid nitrate of silver lies in a corner of the stomach and dissolves, or whether, originally, in solution, its action is distributed throughout the entire irritable mucous membrane.—*Ranking.* (*Mo. Med. Journal.*)

8.—*Extension of the Lecture terms.*

We have received the annual circulars of a considerable number of the Medical Colleges in the United States, from which, and the advertisements in the Medical Journals, we have gleaned the following information, by which their action in reference to an extension of the lecture term may be seen.

Institution.	No. of Professors.	Length of term.
Harvard, (Boston.)	7	Four months.
Berkshire, (Mass.)	6	Fourteen weeks.
Dartmouth, (N. H.)	6	Fourteen weeks.
Castleton, (Vermont.)	6	Four months.
Yale, (Conn.)	6	Sixteen weeks.
University of New York,	6	Four months.
Buffalo, "	7	Twenty weeks.
Geneva "	6	Sixteen weeks.
Albany, "	7	Sixteen weeks.
Col. Phy. and Surg."	7	Five months and a half.
University of Penn.	7	Five months and a half.
Jefferson Med. Col.	7	Four months and a half.
University of Maryland.	6	Four months and a half.
Hampden Sidney, Va.	6	Nearly five months.
University of Virginia.	3	Eight months.
Winchester Med. Col.	5	Seven months.
Med. Col. of S. C.	7	Five months.
Georgia Medical College.	7	Five months.
Memphis Med. Col.	7	Four months.
Transylvania Un., Ky.	7	Four months.
Un. of Louisville, "	7	Four months and a half.
St. Louis Univ. of Mo.	8	Four months and a half.
Univ. State of Missouri,	6	Four months and a half.
Ohio Med. Col.	6	Four months.

The advertisement of Transylvania University states, that "the session will open on the first Monday in November next," and the Medical department of the University of the State of Missouri "on the 16th of October," but without mentioning when they will terminate; we infer, therefore, that the Lectures in these institution will conclude about the first of March, as heretofore.

From the above table it will be seen that in one of the schools, the lecture term is eight months; in another, seven months; in two, five months and a half; in three, five months; in five, four months and a half; in six, four months; in three, sixteen weeks; in two, fourteen weeks.

It would appear that a majority of the schools, including all of those in the New England States, and three out of five of the New York schools, continue to adhere to their former term. Much disparity likewise prevails in the number of lectures delivered during a course. The report of the committee on this subject, submitted to the convention which assembled in Philadelphia in 1847, argues that "It is next to an impossibility, that the strongest intellect can receive and well digest some half a dozen discourses or more a day, embracing subjects which have sometimes little or no immediate connection with each other," * * * * "With a lengthened period for teaching, a double advantage will be gained; a wider extent of information may be imparted to the student, while his time will be occupied with fewer lectures during the day."

Some of the schools which have most extended the period of their sessions, we understand have diminished the number of their daily lectures, so that there is not a corresponding increase in the amount of instruction given; while others, with a less extension, continue to give the same number of daily lectures, and thus add materially to the number of lectures, and consequently the amount of instruction imparted during the term.

The editor of the *Annalist*, one of the most able and zealous of the advocates of reform, referring to "the actions of the conventions on this subject," remarks: "We acquiesced, but very reluctantly, in the fixation of the term at five months, by the American Medical Association, and yet live in hopes that the future will see the period further increased."

Commenting on the annual circular of the University of New York, the editor observes: "The circular goes on to oppose, as if it were a *sine qua non*, the plan adopted in some institutions of reducing the number of daily lectures, while the terms of attendance are increased. With this plan we have no fellowship, and shall not stop to exhibit its demerits. It is, it appears to us, no improvement on the old system."

"We fully agree with the Faculty of the University, that six lectures a day, are not more than an industrious student can attend without injury to his health, or risk of becoming inattentive from fatigue. We did it during the three latter years of our student-ship, taking notes of several of the lectures, attending to the hospital practice, and having besides leisure of an evening to write out some of the notes taken during the day, and for dissecting during the session; and we were never better in health, nor happier, than when thus busily engaged."—*N. Y. Annalist*, Aug. 1st. 1848.

The Faculty of Jefferson Medical College, in their last *Annual Announcement*, hold very nearly the same language: "The prevalent idea, that too much is attempted to be taught in the four months generally allotted to the medical session, is of more recent origin."

"The time usually employed in lectures during four days in the week is six hours, and it is acknowledged in all professions, and has been especially so by lawyers from Lord Coke downwards, that six hours daily ought to be devoted to professional reading. To lecture may be regarded as synonymous with 'to read;' consequently the medical student who listens to six lectures in the day may be looked upon as having been 'read to' for six hours; and there can be no essential difference between *reading* and being *read to*, except in the circumstance, that the latter is much easier for the student. The well informed and able lecturer adapts his elucidations more readily to the comprehension of his hearers than can be done in the best books. He has an opportunity of perceiving whether he is understood; and should he think he is not, he modifies or repeats his instruction."

Our individual experience confirms these views. We have had a pretty extensive opportunity of judging of the operation of the system of full lectures and moderate sessions, and we are fully persuaded, that indolent young men with long purses, prefer long sessions and few lectures, while the more industrious, with moderate means and correct habits, are unwilling to lose an hour in the day, and prefer to learn as much as possible within the shortest practicable period.—*Philad. Med. Examiner*, Sept., 1848.

9.—*Case of United Fracture of the Femur of one year's standing, successfully treated by Resection, Denudation, and Retaining the ends of Bone by means of wire.* By DANIEL BRAINARD, M.D., Professor of Surgery in Rush Medical College.

About the middle of February, 1848, Sewell Rice, of Allegan County, Michigan, aged 34 years, laborer, of pretty good health and constitution, applied to me on account of an ununited fracture of the left femur.

State of the Parts.—On examination, the fracture was found to be situated seven inches above the knee, very oblique, the superior fragment placed in front, the ends overlapping, when no extension was made, not less than four inches, and the fragments so moveable that they seemed to have no connection with each other or with the surrounding parts. The extremity of the upper fragment in particular, when acted on by the muscles, and drawn inwards, forwards, or outwards, could be felt as if beneath the integuments only, and seemed to move as if in a sac. Both ends could be felt rounded; there was no tenderness and the member could be bent almost to a right angle at the point of fracture, without giving pain. The member hung dangling, entirely useless; the thigh, from the want of use and the pressure of an apparatus which he kept buckled around it, was atrophied and attenuated to a remarkable degree, while the foot and leg, from the obstructed circulation, were swollen, œdematous, and indurated, so that it but partially regained its natural appearance when the pressure was removed, and the patient placed in a horizontal position.

History.—On the 15th of March, 1847, a tree fell upon him, and not only fractured the thigh, but severely bruised the knee, leg and ankle of the same side. There was much bruise at the point of fracture, but the skin was not broken.

The limb was laid upon a double inclined plain, the leg enveloped in a roller, and a piece of flannel pinned about the thigh. Extension was made by means of "half a brick" suspended from a cord attached to the foot, and passing over the foot of the bed, and counter extension by means of a "flat iron" suspended from a cord attached to the pelvis and passing over the head of the bed. At the end of five and a half weeks, the weight attached to the foot was increased to about sixteen pounds, and the limb was placed in a different apparatus. During the whole of the treatment the roller upon the leg was removed every three or four days, and the member at such times subjected to considerable movement. At the end of eleven weeks, the apparatus was taken off, and the patient removed about a mile, and directed to rise and walk on crutches. This, however, he was unable to do. After sometime the surgeon examined it and finding it ununited, moved the fragments freely, and re-applied the apparatus for seven weeks.

During the whole of this time the point of fracture remained very tender; and during the night or when it was moved, there was spasmodic action of the muscles, which drew forward the upper fragment, "so that it seemed as if it would come through the skin," and shortened the member very much. Such was the history as obtained from the patient himself.

Treatment.—This case presented nearly every unfavorable circumstance for successful treatment which could be met with in a patient of tolerably good health and constitution. Hence, in selecting the plan of treatment, it was necessary to look at once to those of a severer character.

Thus, rest and immobility had been tried to a certain degree, but what could be expected from this when, far from being able to bring the ends of the bones together, their sides could scarcely be so approximated as not to have soft tissues intervening between them. Pressure, rubbing the bones together, cutting down and applying caustic to their ends were all evidently insignificant in this case.

The seaton, regarded in this country as *the remedy* for ununited fractures is hopeless in cases of this kind; and it is from its having been used indiscriminately in these and others of a similar nature, that it has lost, in foreign countries,

the credit to which it is entitled. Resection was the only means offering some chances of success, but when we consider that the want of union was due, in all probability, to the spasmodic action of the muscles, there was evidently no security against the recurrence of the same cause after the operation. In order to guard against this result, I determined to put in practice a means which, as far as I have been able to learn, has not been used in similar cases. This method consists in cutting down to the bone, denuding or removing the ends, and then passing a wire around the extremities slightly overlapping each other, and twisting the ends of the wire together so as to bind the fragments immovably in this position.

I am aware that the wire has been recommended in cases of fracture of the jaw, being carried round the teeth, in cases of fractures of the patella and in cases of compound fractures; but all these cases are widely different from the present. In case of fracture of the jaw, its application to the teeth is always harmless and often useful. In case of simple fracture of the patella, which, with good treatment, unites to form a useful member, cutting down to perforate the fragments, and hold them together with wire, is a useless and most dangerous operation, converting, as it does, a simple into a compound fracture, with opening into the articulation of the knee. But the principles applicable to that location are not to be generalized. It is obvious that in cases of ununited fractures with great mobility, the use of the wire may be made to answer the purpose of a season in exciting inflammation, while it holds the bones immovably in contact.

In cases requiring resection or denudation it may be used as an additional means of security, or as a substitute, without increasing the danger. In many cases of superficially placed bones, its application is free from danger; and while it holds them firmly in contact, its presence excites a moderate degree of inflammation.

Any one who will take the pains to examine the report of cases pseudarthrosis, given in different periodicals, will not fail to notice a certain number in which the use of this means is obviously indicated according to the well established principles of surgery. In some rare cases it will enable us to succeed where other means alone would fail.

These cases present themselves under such a variety of forms, situations, and circumstances, that no method of treatment is adapted to more than a small number, and new resources are still required for several varieties rebellious to the ordinary resources of art. When such cases present themselves to the surgeon, he should rather follow the indications and views which they suggest to his mind, than abandon it because the methods most approved are either inapplicable or unsuccessful. Guided by such views which we hope may be sufficient to justify us in using a means which some surgeons have, from its application in particular cases, condemned, we proceeded on the 1st of March, 1848, with the assistance of Drs. Herrick and Blaney, and in presence of the class in attendance on our spring course of lectures, to perform the operation in the following manner:

An incision was made four and a half inches in length, upon the anterior and exterior side of the thigh, and carried down to the fracture. The ends of the bones were exposed and found to be covered with a tough fibrous tissue of great firmness. This was removed by means of the bone nippers from the end of the lower fragment; the end of the upper piece being very sharp, and having great tendency to project forwards, a chain saw passed around it, and about an inch of it removed.

Extension being then made, the two extremities were approximated to each other, and a piece of silver wire, (like that used for constricting polipy,) doubled and twisted, was slipped over each end so as to encircle the two bones. The ends of the wire were then twisted together with sufficient firmness to prevent movement without exercising much pressure on the surface of the bones. The limb was then placed in an angular splint, the sides of the wound brought together with stitches of interrupted suture and splints applied so as to restrain

its movements. The operation lasted perhaps half an hour, was well borne by the patient, and no vessel required a ligature. It was evidently, so far as the immediate depressing effects were concerned, much less severe than amputation of the thigh. Using the chain saw, instead of turning out the ends of the bones to saw them off, renders the operation less severe. Considerable inflammation followed, which was combated with anti-phlogistic treatment. The wound healed by the first intention excepting at two points of sufficient extent to allow of the escape of matter. On the third day the spasm of the muscles of the thigh, before mentioned as having followed the fracture, occurred, being excited by the slightest touch or motion. It was so severe as to shorten the limb somewhat, and was attended with such violent efforts at displacement as to show conclusively the necessity of the wire, or some equally effective restraining means. The suppuration diminished, the patient's appetite returned, and he was in every way comfortable.

The object of the wire—that of holding the ends of the bone firmly in contact until adhesions might be formed—having been accomplished, it was desirable to remove it. This was done on the 24th of March, as carefully as possible, but some inflammation followed, and on the 4th of April, fluctuation was perceived on the inside of the thigh, and a puncture was made. Under the use of astrigent injections, the abscess contracted rapidly, and on the 16th of April there remained only a fistulous opening on the outside, and another on the inside, through which a few drops of serum could be pressed. At the end of twelve weeks he was allowed to move the limb in bed, and in four months he walked on crutches, the union being complete. At the present time, August 1st, he moves the member with great facility; the muscles atrophied from long inaction and pressure, are being developed by use; and, although, considerably shortened, it is far superior to any artificial limb.

It will be obvious that the use of the wire in this case was the only means of avoiding a resort to that extreme resource of art, amputation.

This case is offered to the consideration of the profession, as indicating an additional curative means in a class of cases which sometimes resist the skilful application of every plan heretofore made use of.—*North-West. Med. and Surg. Journal*, Sept., 1848.

10.—*Neglect of the Medical Corps of our Army and Navy.*

Why have there been no promotions in the Medical profession connected with our Army and Navy in the recent campaigns in Mexico? We have looked in vain for the first surgeon's name, over the long list of promotions among the regular and volunteer troops. There have been created some 500 brevet-commissions during the last two years—indeed, scarcely an officer of our army, it matters not in what capacity he has served, so he was not in the medical staff, who has not been promoted. We read of most onerous duties performed during the whole war, of gallant services during engagements with the enemy, of several, many sad deaths occurring among the surgeons of the Army and Navy, but not of a single promotion. It is thus ever with the medical profession; whether in civil or military life, we must toil on, toil ever—labor, day and night, before, during and after battle—and do this without distinction or reward. The soldier who performs one daring deed, is rapidly advanced and adequately requited; but the surgeon, equally self-sacrificing who renders duties far more arduous, is seldom noticed—never promoted. We are familiar with the heroes of every battle fought in Mexico, but, with the occasional or general commendation at the close of an officer's report, we find no other notice of that highly respectable corps of medical men, who have rendered most efficient service to their country, and who have saved many a valuable life. Again we ask the proper authorities, why have there been no promotions among the Surgeons of our Army and Navy? Have they alone failed in their duty; if not, why are they alone neglected?—*Southern Med. and Surg. Jour.*, Oct., 1848.

11.—*Internal Use of Chloroform.*

When the inhalation of this substance as an anæsthetic began to attract universal attention, we made at the hospital, at Dr. Wood's suggestion, some investigation into its effects when taken internally. Although very sweet it is so pungent as to require a large proportion of gum water to obtund its effects on the tongue, throat and even stomach.

Seventy-five drops gave me a sensation of general diminution of consciousness and sensorial capacity. Sight, hearing and touch were all made less impressive, but no feeling of exhilaration or of derangement occurred. Drowsiness was positive. The pulse was not at all accelerated; in fact, it was two beats less in a minute. The same effects followed in two other medical gentlemen, from two or three times the quantity. One swallowed a drachm, or over 200 drops. He became very heavy, and after a short interval went into a sound sleep. The effect passed off in all of us after an hour or two. No one felt exhilarated or deranged as by alcohol, and in none was the pulse increased. The conclusion was positive in all our minds that it is a *direct sedative* to the cerebro-nervous system—a sedative narcotic.

A woman who suffered with a most obstinate and painful neuralgic affection of the side and head, took 75 drops at night. She slept better than she had for weeks, even after inhaling ether or chloroform, as she had repeatedly; and was also unusually comfortable the next day. She then continued to take it, in a somewhat larger dose, for two or three weeks, every night; and improved under this more than any other treatment.

From the pungency of chloroform I was induced to expect carminative powers. Upon trial in a number of cases of flatulent colic, I found it in the dose of 75 drops, sometimes repeated, to give prompt relief. It was then used as a substitute for Dover's powder as a soporific at night in rheumatism; and there answered admirably. There was but one patient, a woman with cancer of the uterus, to whom it was given as an anodyne, who alleged that it disagreed with her, causing headache and sickness of stomach without sleep or relief.

Believing it to be less constipating and more purely sedative than morphia, I tried the addition of it to cough mixtures in place of the latter. But here the objection presents, that so large a quantity of mucilage is required, as to oppress the stomach; or, if given in alcoholic solution, the stimulus of this vehicle might be undesirable. Still, as it is very pleasant to the taste, it might be added in small quantity to such mixtures, lessening the usual amount of opium in them.

It might be worth while to try it by enema in strangulated hernia, as a powerful relaxant to the system generally, and at the same time an excitant of peristaltic action. It is our impression that it is somewhat laxative even when taken by the mouth; but our experiments did not render this certain. There is no doubt, however, that as a purely sedative narcotic and carminative, having the advantage of a sweet, pleasant taste, it might often be made a serviceable medicine.—*American Jour. of Med. Sciences, Oct., 1848.*

12.—*New Operation for the Radical Cure of Varicocele.* By S. D. Gross, M. D., Professor of Surgery in the Medical Department of the University of Louisville.

The following operation for the radical cure of varicocele I have performed eight times within the last few years. The patients were all young men of good constitution, and they all recovered without a single bad symptom. The cure, so far as I have been able to learn, promises to be permanent in every instance.

During the operation the patient may lie down, sit in a chair, or stand up,

as may be most convenient. The scrotum, previously divested of hair, is rendered tense by grasping it behind with the left hand. A vertical incision, scarcely an inch in length, is made over the anterior part of the tumor, down to the enlarged veins, which are next carefully isolated from the accompanying duct, artery and nerves, by a few touches with the point of the scalpel. This constitutes the first step of the operation. The second consists in passing a short thick sewing-needle—a No. 1 of the milliner, underneath two or three of the large trunks, and winding around it a stout thread, either elliptically, or in the form of the figure eight. The ligature is drawn with great firmness, so as to indent the coats of the vessels, and put an immediate stop to the circulation. The operation is finished by closing the wound carefully with one or two twisted sutures, or a few strips of court-plaster. The patient is now put to bed, the scrotum is supported with a silk handkerchief, and light diet is enjoined. At the end of twenty-four, or, at most thirty-six hours, the blood in the constricted veins is sufficiently coagulated to justify their division, and the removal of the needle. This is readily effected by insinuating a narrow, sharp-pointed bistoury underneath the vessels, with its back towards the needle.

Should symptoms of inflammation arise after the operation; or, in other words, should the parts become red, tender, and swollen, recourse must be had to antiphlogistics and to the application of cold water, or solutions of acetate of lead and opium. The patient may usually sit up in five or six days, and in a few more he may be permitted to walk about. The little wound soon cicatrizes; and the induration caused by coagulation of the blood between the testis and the seat of the constriction, gradually disappears by absorption. The period required for this rarely exceeds a month.

The advantages of the above operation are, first, its perfect simplicity and the facility with which it may be executed; secondly, its freedom from pain and hemorrhage; thirdly, the certainty with which we may avoid injury to the spermatic artery, duct and nerves; fourthly, the little inconvenience or suffering which the patient experiences after it has been performed; and fifthly, the rapidity of the cure. These considerations will, I think, be found sufficient to recommend this method to the favorable notice of practitioners. Most of the operations described in the books are complicated, severe, and dangerous.

It occasionally happens in this affection that the scrotum is very flabby and pendulous. When this is the case the cure will hardly be complete unless the surgeon retrenches the redundant structures. I have been obliged to resort to this expedient only once in my operations. A portion of the scrotum, nearly of the size of a large hand, was excised with a large scalpel, and the wound closed by a continued suture, which I consider far preferable, under such circumstances, to the interrupted or twisted.—*Amer. Jour. of Medical Sciences*, October, 1848.

NEW ORLEANS, NOVEMBER 1, 1848.

HEALTH OF THE CITY.

In our editorial comments upon the health of this city, in the last number of the Journal, we remarked, that serious apprehensions were entertained by many, that we should be visited by an epidemic yellow fever. We are gratified however, to be able to state that although fever prevailed to a considerable extent, yet it was mild, and amenable to treatment, and for this reason, as well as on account of the comparatively small number attacked, the Board of Health declared it not to be an epidemic; besides the yellow fever, another affection called the *Dengue*, also made its appearance in our midst; in its onset, it resem-

bled in many of its features the yellow fever. It was ushered in by slight chilliness, a sense of malaise, pains in *all* the joints, limbs, muscles, eyes, head, back, &c. These pains in some cases, were described as much more severe than those usually met with in yellow fever; the two diseases were sometimes confounded, or the one succeeded the other. Previous attacks of fever, did not exempt persons from an attack of the *Dengue*. We believe, but few if any cases proved fatal, as the affection passed off in a few hours, or at most a few days, when opposed by mild cathartics, tepid drinks, hot foot-baths, and the sulphate of Quinine. It disappeared with the yellow fever.

We propose to give below a few statistical facts, which may be of some interest to the physicians of this city, as well as citizens generally. We find on examining the "*Dead-Book*," kept by the Board of Health, that from the 1st of May 1848, (the day on which the present Board was organized,) up to the 14th of October, the total number of deaths of all diseases, in New Orleans, was 3332. Of these were interred by

certificates of physicians,	-	-	-	-	-	-	1551
From Charity Hospital,	-	-	-	-	-	-	725
" Private Hospitals,	-	-	-	-	-	-	250
Died at other places, and brought here for interment,	-	-	-	-	-	-	6
By citizens' certificates,	-	-	-	-	-	-	237
" Coroner's certificates,	-	-	-	-	-	-	116
" Commissaries' certificates,	-	-	-	-	-	-	206
" Midwives' certificates,	-	-	-	-	-	-	94
Uncertain,	-	-	-	-	-	-	147
Total,	-	-	-	-	-	-	3332

Of these, up to the 28th October, the number of deaths from yellow fever, was 716, of which number, more than one half died in the Hospitals of the city. From the foregoing statement it will be seen that not quite one half of those who died, came under the care of physicians in private practice. It may be asked, were the 206 cases, returned by our commissaries, suffered to languish and die without medical aid?

During the season, the Board of Health, with much foresight and great firmness, maintained that the fever was not epidemic. The careful and correct statements made public by the Board, contributed greatly, we conceive, to allay the apprehensions of the public mind, and perhaps, to limit the ravages of the disease. The following resolution was published by the Board of Health, September 4th, 1848:

"*Resolved*, That the official reports show that the cases (of fever) are as yet of a mild character, and that the disease has not increased to such an extent as to justify the Board in pronouncing it epidemic; still they caution those who are unacclimated, against imprudent exposure and the exciting causes of the fever.

"(SIGNED:) A. D. CROSSMAN, *President of Board of Health*.

"A. HESTER, *Secretary*."

Notwithstanding the above clear and explicit, and we may add, honest statement of the Board of Health, yet many maintained, and some went even so far as to publish to the world, contradictory opinions; (with what motives we leave it with them to determine.)

This was illiberal towards a highly respectable body of our fellow

citizens, who were appointed by our city councils to occupy a responsible position which they did not seek, and the labors of which they undertook to perform without fee or emolument. The incredulous should remember, that Gallileo was imprisoned because he declared the earth to be round; yet this did not alter the fact, or change the opinion of the philosopher.

Thus much we deemed it just to state in vindication of the motives of the members of the Board, in the discharge of their delicate duties during the past summer.

On the 2d of October, the Board of Health published the following notice: "The Board take great pleasure in announcing to their fellow citizens who are absent, and to persons who contemplate visiting New Orleans, that from this date no fear of yellow fever need be apprehended by the unacclimated for this season.

"(SIGNED:) A. D. CROSSMAN, *President of Board of Health.*

"A. HESTER, *Secretary.*"

A few deaths still continue to occur from yellow fever, chiefly old and neglected cases, relapses, etc. It will be noted, that as this disease declines, the usual autumnal diseases, as intermittent and remittent fevers, begin to appear; besides these, we have some cases of typhoid fever, diarrhoea, dysentery, and several deaths, weekly, from phthisis pulmonalis.

During the last two or three weeks, the deaths from tetanus and trismus have greatly increased, and we hear of these diseases in almost every part of the city and in the Hospitals, both private and public. The slightest wound, especially in the foot or hand, is likely to be followed by tetanic symptoms. This is not the place to speak of the treatment of this formidable and usually intractable disease; but we may remark that *chloroform* will relieve if it does not check those violent *clonic* and *tonic* spasms, which so speedily exhaust the poor patients; we speak advisedly on this point, and recommend it to the profession, as at least useful in checking the more violent symptoms of the disease, until the usual remedies can be brought to bear on the case. Notwithstanding the slight prevalence of the above disease, the city is quite healthy, and our citizens who left us early in the season in search of health and pleasure, as well as strangers, are rapidly returning to take their respective places among us. We invite all to return.

We cannot close these remarks without allusion to the *Asiatic Cholera*, which has recently visited Russia, Egypt, Arabia, and is now spreading death and destruction over the greater part of Europe.

This fearful epidemic is now raging in Berlin, Vienna, Hamburg, &c., and is reported to have made its appearance in Paris, London and Edinburgh and whithersoever it marches, in the language of *Horace*, "*terruit urbem, terruit gentes.*"

By this disease, the destroying Angel of the prophets must be typified, and although we cannot hope to stay his flight to our shores, yet it becomes us to prepare for his reception. Let the authorities of our cities, Boards of Health, &c., have the sanitary police of our towns increased, our streets, alleys, lots, etc., etc., cleansed, and thus make them fit temples, to carry out the figure, for the goddess Hygiæa to occupy, who alone can appease or avert the wrath of the Angel of death.

It is the universal opinion of the various commissions sent out to watch the progress of this epidemic, that the filthy and uncleanly parts of towns and cities are its chosen seats, and the first points to invite the disease. This fact should teach us to prepare for the enemy, to institute measures sufficiently stringent and effectual to abolish or remove all the *material* likely to constitute a focus—a hot-bed for the development and propagation of the epidemic. If “eternal vigilance is the price of liberty,” so may health be secured, under almost every circumstance, by careful and unremitted attention to the causes which are universally acknowledged to excite epidemic disease.

The next breeze that sweeps across the Atlantic, may bear upon its bosom the seeds of this appalling disease, and how much of woe, suffering and death will it not bring among us? It is the part of wisdom to anticipate evils before they overtake us, and if then they must come, heaven will arm us with courage to combat them.

The *Board of Sanatory Commissioners* of London, and some other European cities, state in their recent reports “that it is the combination of *humidity* with *impurity* of the atmosphere, which so powerfully predisposes to Cholera.” This important conclusion, to which the above commission arrived, after carefully investigating the subject, is eminently applicable to this city; for are we not constantly surrounded by, and living in a humid atmosphere! and do not our streets, gutters, alleys, vacant lots, etc., etc., abound in filth, debris, and every variety of animal and vegetable matter? Let this subject be looked to in time, for if neglected until the Cholera makes its appearance among us, we shall be too much occupied in burying the dead, &c., to give it any attention.

A. H.

HEALTH OF THE COUNTRY.

GRAND COTEAU, ST. LANDRY, October 12, 1848.

To report the health of this section, for the last summer and autumnal months, is but to record an exemption from any extensive or generally prevailing disease. The intermittents and remittents have extended through their usual circuit, but mild in character and less in frequency. The former scarcely call for the attention of the profession, so extensive of late has become the practice of the people to administer large quantities of quinine in their own families;—the latter only in their aggravated types demand our medical aid. The drug has become so common, its effects so apparent, and the risk in its use so little feared by most of the population, that every one gives and takes it indiscriminately in all fevers and in all quantities, and almost invariably with decided beneficial action. Thus the ordinary fevers are cut short at their beginning, many of them permanently cured; and it is only to stubborn, complex quotidians and tertians that we are required to render our services. The practice therefore in the country is becoming abbreviated to a considerable extent from that of ten years past, and doubtless with pecuniary benefit to the people at large, and I am inclined to believe with actual advantage to the standing, dignity and intelligence of the faculty. We are thrown hereby constantly among patients whose cases require

all our attention and devotion; and this serves not only to bind our minds and our whole mental powers to the study, the research and duties of our profession, but places the worthy and erudite practitioner high above the capacities and pretensions of the illegalized quack, and the bold and reckless charlatan.

The remittent fevers have been of a mild type, easily manageable by proper remedies. It has been truly remarked that diseases of this character have materially lessened in severity since a few years back, or that the profession have become more expert or *au fait* in their treatment. The latter perhaps is the true solution; and I attribute our largest success at the present time, to a fearless and prompt administration in every stage of bilious fever, with proper combination, to the sulphate of quinine. We give it as a febrifuge and anti-periodic, in the paroxysm as in the intermission, and as the case may indicate, before and after alteratives and evacnants. The sulp. quinine indeed seems to control the morbid influences that constitute fever, equalizes as by a specific action the circulation of the fluids, destroys the chain of atomic associations in the economy, and gives a vivifying and tonic impulse to the *vis medicatrix nature* in establishing the vigorous powers of health.

A few cases of congestive fever have appeared in my practice during the summer, none of which has proved fatal. The diffusive stimulants with alteratives and quinine, aided by external applications of mustard, capsicum, frictions &c., very soon dispersed the local turgescence and restored the circulation to its natural channels. These few cases have convalesced more rapidly and more permanently than those of the common remittent. This fact may be accounted for from the patients having perhaps used more caution, and subjected themselves to less exposure, after having arisen from a severe disease, than such as deemed themselves afflicted by an ordinary sickness.

The College of Saint Charles, and the Academy for young-ladies attached to the Convent of the Sacred Heart, have had an almost entire exemption from all classes of diseases. The order of these houses, the regularity of diet and exercise, correctly balanced,—the stated and fixed periods of sleep and waking, from which there is no deviation,—the extraordinary *surveillance* of the institutions that ensures the utmost cleanliness over the whole premises, render any domestic cause for disease next to impossible. Hence we have seldom to notice more than an accidental case of *severe* malar^y in any given scholastic session.

E. M. M.

CHARITY HOSPITAL.

Since our last issue of the Journal, several interesting surgical cases have been admitted into this Institution—among this number we may mention a young German who received, sometime since, a penetrating wound in the abdomen, entering the centre of the umbilicus, and causing a protrusion of a large section of *omentum*. Efforts were made by Dr. Compton, under whose charge the case fell, to reduce it, but this was found impracticable until the subject was brought under the influence of *chloroform*, when the protruded omentum was readily returned. This

was about two weeks past, since which time the patient has been doing well.

Another interesting case of penetrating wound of the abdomen took place some three or four weeks since. A young man, we believe an Irishman—was stabbed in the abdomen; the knife entered about half way between the umbilicus and the crest of the ilium—penetrating, as was supposed, the descending colon.

It was manifest that the gut had been wounded, because fecal matter continued to be discharged from the wound for many days; but under the influence of opiates, anti-phlogistics, and repose the fecal matter gradually ceased to be discharged, the wound ultimately healed, under the judicious management of Dr. Nott, in whose care he was, and a few days since, the patient was discharged well, the natural course of the bowels having been perfectly re-established.

The above is a very instructive case, and shows how competent nature must be to repair serious lesions, if allowed to take her own wise course—when aided by a judicious surgeon. Besides these, many other curious cases have been received into the Charity Hospital. But we cannot make room for them in the present number.

The following one, however, is of such practical value as to deserve to be detailed in full:

Service of Dr. COMPTON. Successful operation for deformity of thigh from badly united fracture.

George Darcy, an exceedingly intelligent Irish lad, aged 18 years, was admitted into the Charity Hospital on the 6th of June last, and came under the charge of Dr. T. W. Compton. He gave the following account of himself—being a cabin boy on the ship *Selim*, going from Whitehaven to Charleston, May 1846, was ordered aloft—on reaching the main top, lost his hold and fell to the deck, the distance of one hundred feet. By this fall one femur, the bones of both arms, those of one forearm, the frontal bone at the inner margin of the right brow were fractured, and as a consequence of the last injury total blindness of the right eye ensued without change in the appearance of this organ. He remained in a state of insensibility for six weeks. The evidences of this complication of injuries are still apparent on his person.

On his arrival in Charleston he was sent to the Marine Hospital where he remained nearly twelve months—discharged on crutches—went to Boston and entered the Hospital, with bones of right arm exfoliating—discharged at the end of five weeks—went to Dublin and was attacked with Typhus Fever. Abscesses formed in injured thigh, extending from knee to hip-joint. He was here advised to have the limb amputated at hip-joint—refused and was discharged after having remained 3 months—went to Liverpool, whence he sailed for New Orleans. Some 4 days after his arrival, the thigh being painful, he entered the Charity Hospital 6th June, 1848.

The above history is necessarily imperfect and, though unimportant to the point to be illustrated, is a curious and striking example of extraordinary tenacity of life.

The femur had been fractured about its middle and the fragments had

so united as to produce very great deformity—the upper fragment overlapped the lower some two inches and so great was the anterior angular derangement, that when the patient stood up on the sound limb, the knee of the injured leg projected forwards and outwards (as in dislocation into foramen ovale) and the heel rested on the sound leg above the inner ankle $4\frac{1}{2}$ inches from the floor.

The lad was very anxious to have the deformity corrected and willing to submit to any operation which offered a chance of success.

Dr. Compton after due reflection upon all the circumstances decided upon the following operation:—He made an incision about eight inches long, commencing four inches above the projecting angle and passing downwards over it along the middle line of the thigh in front—the projecting end of the upper fragment was just below the skin and had caused much irritation—the incision was carried through its whole length, directly down to the bone, and the soft parts were separated from it on each side. The Doctor had intended to divide the bone at the point of union with a chain saw, but the deposition of callous and cartilaginous matter was so great as to render it difficult to use this instrument. The soft parts were therefore drawn aside and the projecting end of the upper fragment of the femur which had overlapped the lower was first removed with a small saw—a common amputating saw was then used till the main bone was more than half divided and the division was then completed with Hay's saw.

The limb was then brought into a right line and dressed with Liston's splint for three or four days, when it was placed on a double inclined plane on which it remained until the union was complete.

A good deal of inflammation and suppuration ensued and continued for several weeks, the extensive supuration may partly be attributed to the exposure of the cancellated structure of the bone where the projecting end of the upper fragment was removed. Such local and general treatment was employed by the Doctor from time to time as the symptoms called for and a gradual amendment of all the symptoms took place.

The operation has now been performed three months and the patient is up on crutches with general health in a satisfactory condition—the bones are firmly united and the soft parts perfectly sound. The limb too is perfectly straight, and there remains a shortening of not more than an inch and a half—which may be easily corrected by a high-heeled shoe.

The great practical value of this case must be apparent to every surgeon, and Dr. Compton deserves much credit for both the conception and execution of the operation as well as for the skill and tact with which he has conducted a case of so much complication to a successful issue.

HOSPITAL REPORTS.

CHARITY HOSPITAL.

June 1848.

Admissions :	Males,	-	-	-	-	508,	}	656
"	Females,	-	-	-	-	148,		
Discharges :	Males,	-	-	-	-	451,	}	574
"	Females,	-	-	-	-	123,		
Deaths :	Males,	-	-	-	-	62,	}	72
"	Females,	-	-	-	-	10,		

July 1848.

Admissions :	Males,	-	-	-	-	768,	}	915
"	Females,	-	-	-	-	147,		
Discharges :	Males,	-	-	-	-	614,	}	761
"	Females,	-	-	-	-	147,		
Deaths :	Males,	-	-	-	-	73,	}	89
"	Females,	-	-	-	-	16,		

August 1848.

Admissions :	Males,	-	-	-	-	1196,	}	1411
"	Females,	-	-	-	-	215,		
Discharges :	Males,	-	-	-	-	994,	}	1161
"	Females,	-	-	-	-	167,		
Deaths :	Males,	-	-	-	-	176,	}	196
"	Females,	-	-	-	-	20,		

September 1848.

Admissions :	Males,	-	-	-	-	1105,	}	1353
"	Females,	-	-	-	-	248,		
Discharges :	Males,	-	-	-	-	878,	}	1111
"	Females,	-	-	-	-	236,		
Deaths :	Males,	-	-	-	-	230,	}	253
"	Females,	-	-	-	-	23,		

HOSPITAL RERORTS.—(CONTINUED.)

By DR. BRICKELL.

MESSRS. EDITORS.—I continue my report of experiments with quinine. Below are notes on ten cases wherein large doses of this medicine have been administered, uncombined with opium. D. B.

CASE 1.

June 5th, 1848. W.W. Wright, native of England, æt. 42 years ; has resided in this country twenty-four years—clerk in a counting-house here—entered ward 13, Charity Hospital, yesterday. Has been sick with inter. fever (at intervals of a few weeks) during sixteen months past ; "tertian" type generally, though occasionally "quotidian ;" has taken quinine, in "broken doses," for a long time, but with no beneficial effect whatever. For three weeks past he has taken a paroxysm regularly every other day, at 9 A. M. ; expects a paroxysm to-morrow ; tongue is coated white ; spleen somewhat enlarged ; appetite very good ; took purgative yesterday, which operated freely.

Treat.—Quin. sulph. grs. xxx., to be taken at 7 A. M. to-morrow ; low diet.

6th, 9 A. M. Patient was mistaken in the time of recurrence of the paroxysm by two hours ; consequently he was seized with a chill a few minutes after taking the medicine this morning. I find him in the "hot stage."

Treat.—Warm drinks, &c. for the day ; same quantity of quinine as before to be taken at 5 A. M., on the 8th.

8th, 9 A. M. No paroxysm this morning. Says he feels much better than he has done for some time past ; thinks the last paroxysm was much mitigated by the quinine, notwithstanding it was taken so late.

Treat.—Port wine and good diet ; quin. sulph. grs. xv., to be taken at 5 A. M. of the 10th.

10th, 9 A. M. No paroxysm ; improving very rapidly ; looks and feels one hundred per cent. better.

Treat.—Port wine and full diet.

12th. Discharged him *well* ; promises to inform me if the disease should return.

CASE 2.

June 7th, 1848. Andrew Lyings, native of Ireland, æt. 38 years ; has resided in this country one year ; laborer in the city ; entered ward 11, Charity Hospital, yesterday. Has been sick with Inter. fever one month ; "tertian" type ; paroxysm recurs regularly at 5 P. M. Patient is quite pale ; tongue coated white ; complete anorexia ; took purgative yesterday.

Treat.—Quin. sulph. grs. xxx., take at 3 P. M. ; low diet.

8th, 9 A. M. Took medicine as directed yesterday, but had a severe chill, &c., at the usual hour ; hot fever nearly all night ; did not perspire ; skin cool and moist this morning ; tongue still coated ; anorexia.

Treat.—Quin. sulph., as before, to be taken at 3 P. M. of the 9th ; ol. ricin. f. $\frac{3}{4}$ j., take at once.

10th, 9 A. M. No paroxysm yesterday ; feels much better ; countenance cheerful ; appetite improving.

Treat.—Port wine and nourishing diet ; quin. sulph. grs. xv., to be taken at 3 P. M. on the 11th.

12th, 9 A. M. No paroxysm yesterday ; marked improvement.

Treat.—Port wine and full diet.

13th. Desires to leave the house ; discharged him.

CASE 3.

June 14th, 1848. John Alexander, native of Pennsylvania, æt. 33 years ; quite robust—boat-hand—has been in the city two weeks ; entered ward 13, Charity Hospital, yesterday ; has had inter. fever for five days—"tertian" type—paroxysm recurs regularly at 7 A. M. ; generally very severe ; tongue is dry and brownish at the centre, with very red edges ; great thirst ; skin now cool and dry ; expects paroxysm to-morrow.

Treat.—Ol. ricin. f. $\frac{3}{4}$ j.; take at once; lemonade and low diet; quin sulph. grs. xxx., to be taken at 5 A. M. to-morrow.

15th, 9 A. M. Nurse neglected to administer the medicine as directed; consequently the paroxysm recurred as usual; patient is now in the "hot stage."

Treat.—Administered the quinine of yesterday; ordered mixt. neutral. f. $\frac{3}{4}$ vj., table-spoonful every two hours.

16th, 9 A. M. Perspired very freely after taking the quinine yesterday; slept well last night; tongue moist and much paler; slight yellowness of the skin, and conjunctivæ has appeared since yesterday. Considerable prostration.

Treat.—Mixt. neutral.; low diet; quin. sulph. grs. xv., to be taken at 5 A. M. to-morrow.

17th, 9 A. M. No paroxysm this morning; improving gradually.

Treat.—Port wine and nourishing diet.

21st. Patient continued to improve, and this morning I discharged him well.

CASE 4.

June 17th, 1848. John McReeve, native of Ireland, æt. 35 years; has resided in this country four months; in the city three weeks—laborer—entered ward 13, Charity Hospital, yesterday. Has had inter. fever eight days—"quotidian" type—paroxysm recurs regularly at 10 P. M.; tongue is coated white; anorexia; took dose of oil yesterday; operated freely.

Treat.—Quin. sulph. grs. xxx., take at 8 P. M.; low diet.

18th, 9 A. M. Paroxysm recurred at the usual hour, notwithstanding he took the medicine as directed; does not think it was at all mitigated; present symptoms same as before mentioned.

Treat.—Repeated prescription of yesterday.

19th, 9 A. M. No paroxysm last night; perspired very freely after taking the medicine; feels infinitely better; slight "tinnitus aurium."

Treat.—Quin. sulph. grs. xv., take as before; half diet.

20th, 9 A. M. Missed paroxysm again; is improving rapidly.

21st. Discharged him.

CASE 5.

June 19th, 1848. John Brickelius, æt. 26 years, native of Germany; has resided in this country 2 years; in this city all the time—tailor—entered ward 11, Charity Hospital, this morning. Has had inter. fever with short intervals, during seven months—"Quotidian" type—paroxysm has recurred at 2 P. M. for eight days past; he is quite pale; tongue slightly coated; appetite pretty good; rather costive.

Treat.—Ol. ricin. f. $\frac{3}{4}$ i. take at once; quin. sulph. grs. xxx., take at 12 M.; half diet.

20th, 9 A. M. No paroxysm yesterday; says he feels very much better; appetite improving.

Treat.—Quin. sulph. grs. xv., take at 12 M.

21st. Patient is anxious to go to work, so I discharged him rather earlier than I would have wished. He promises to report himself should the disease return.

CASE 6.

June 23d, 1848. John Oliver, native of Ohio, æt. 33 years; in this city 14 days—steamboat-hand—quite robust; entered ward 13, Charity Hospital, yesterday. Was taken sick yesterday at 4 P. M.; had severe chill, followed by high fever, headache, thirst, &c.; perspired very freely; tongue is quite white this morning; skin cool; pulse natural; anorexia; no pain; costive.

Treat.—Ol. ricin. f. $\frac{3}{4}$ i., take at once; quin. sulph. grs. xxx., take at 12 M.; low diet; lemonade.

24th, 9 A. M. No paroxysm yesterday; oil operated freely; says he feels nearly well; tongue cleaning rapidly; some appetite.

Treat.—Quin. sulph. grs. xv., take as before.

27th. Discharged him quite well.

CASE 7.

July 20th, 1848. Patrick Daguin, native of Ireland, æt. 42 years—laborer; resided in this city 18 months; entered ward 13, Charity Hospital yesterday. Has been subject to frequent attacks of inter. fever during the last 15 months, from two to four weeks generally intervening between the attacks, which generally assumed the "tertian" form, and continued some ten or twelve days. Was seized with a severe paroxysm on the 14th instant, at 12 M.; has had paroxysms regularly every other day up to the present time, expects do. to-morrow; is quite pale; complete anorexia; rather costive. Upon entering the ward yesterday, he was ordered by the house surgeon quinine in broken doses, with the hope of averting the expected paroxysm; medicine failed to have the desired effect, however; complains considerable of "tinnitus aurium," and dull pain in the occipital region this morning, the probable effects of the quinine, of which he took grs. xv. in solution.

Treat.—Ol. ricin. f. $\frac{3}{4}$ i. take at once; quin. sulph. grs. xxx., to be taken at 10 A. M. to-morrow; low diet.

22d, 9 A. M. No paroxysm yesterday; looks much better; little appetite.

Treat.—Port wine and half diet; quin. sulph. grs. xv., take at 10 A. M. to-morrow.

24th, 9 A. M. No paroxysm yesterday; improving rapidly.

Treat.—Ol. ricin. f. $\frac{3}{4}$ j., take at once; quin. sulph. grs. xv., take to-morrow at 10 A. M.

26th, 9 A. M. No paroxysm yesterday; appetite good.

Treat.—Port wine and good diet.

27th. Discharged him well.

CASE 8.

July 25th, 1848. Bernhard Winkomp, native of Germany, æt. 25 years; has resided in this country nearly three years; in this city all

the time—laborer—entered ward 11, Charity Hospital, yesterday. Was taken sick with inter. fever eight days ago—"tertian" type—paroxysms have recurred regularly ever since at 1 P. M.; expects one to-day; tongue is coated white; conjunctivæ yellowish; very little appetite; bowels in tolerable condition.

Treat.—Quin. sulph. grs. xxx., take at 11 A. M.; low diet.

26th, 9 A. M. Paroxysm recurred at 9½ A. M. (three hours and a half earlier than usual) consequently the quinine was administered in the "hot stage;" patient says he perspired more freely than heretofore; thinks the fever was somewhat mitigated by the quinine.

Treat.—Quin. sulph. grs. xxx., take at 6 A. M. to-morrow.

28th, 9 A. M. No paroxysm yesterday; very much improved in appearance; says he feels well; appetite good.

29th. Patient insists on being discharged.

CASE 9.

July 25th, 1848. John Ricker, native of Germany, æt. 26 years, has resided in this country 1½ years; citizen of Lafayette—baker—entered ward 11, Charity Hospital, yesterday. Has had inter. fever eight days—"tertian" type—paroxysms recur at night, from 12 to 3 o'clock last night; tongue coated white; thirst; complete anorexia; took dose of oil yesterday; operated freely.

Treat.—Neutral mixture for to-day; low diet; quin. sulph. grs. xxx., take at 10 P. M. of the 26th.

27th, 9 A. M. No paroxysm last night; feels well; good appetite.

Treat.—Port wine and good diet.

28th. Desires to be discharged.

CASE 10.

July 28th, 1848. Wm. Slack, native of Ireland, æt. 18 years; has resided in this country one year; in this city two months—laborer—entered ward 11 Charity Hospital, yesterday. Has had inter. fever for three days past; has had two or three attacks of a few days duration, each, during the past month—"quotidian" type—paroxysms recur at 3½ P. M.; he is quite pale; tongue coated and pale; very little appetite; took dose of oil yesterday; operated well.

Treat.—Quin. sulph. grs. xxx., take at 1 P. M.; low diet.

29th, 9 A. M. No paroxysm yesterday, much approved in appearance.

Treat.—Quin. Sulph. grs. xv., take at 1 P. M.; half diet.

30th, 9 A. M. No paroxysm; improving rapidly. Port wine and full diet.

31st. Discharged him.

Interments in the City of New Orleans from the 19th of August, 1848, and ending on the 21st October, 1848, being a period of ten weeks.

Abscess hepatic, 1; Accidental 1; Accouchement, 5; Anasarca, 1; Anemia, 2; Angina malig., 2; Aorta, stricture of, 1; Apoplexy, 15; Arm, fracture of, 1; Arteries, ossification of, 1; Ascites, 3; do. chronic, 1; Asphyxia, 1; Asthma, 1; Brain, congestion of, 17; do. softening of, 2; Bronchitis, 3; do. chronic, 1; Births, premature, 4; Burn, 3; Cancer, 2; Catarrh, 2; do. pulmonary, 1; Cerebritis, 19; Cholera, 1; do. infantum, 4; Colica, 1; Colitis, 2; Congestion, 1; Consumption, 109; Convulsions, 46; Cramps, 3; Croup, 1; Cynanche Trachealis, 1; Debility, 31; Del. Tremens, 8; Dentition, 15; Diarrhœa, 26; do. chronic, 15; Disease, Chronic, 1; do. inflammatory, 1; Dropsy, 20; Drowned, 33; Dysentery, 45; do. chronic, 35; Encephalitis, 4; Enteritis, 24; do. chronic, 1; Entero-colitis, chronic, 1; Epilepsy, 2; Erysipelas, 2; Fever, 7; do. adynamic, 2; do. bilious, 11; do. congestive, 26; do. intermittent, 8; do. malignant, 5; do. nervous, 1; perniciouss, 11; do. do. intermittent 1; typhus, 25; do. remittent, 2; do. scarlet, 3; do. typhoid, 13; do. do. intermittent, 2; do. yellow 635; Gangrene, 9; Gastritis, 7; do. chronic, 1; Gastro-duodenitis, 1; Gastro-enteritis, 11; do. do. chronic, 3; Head, disease of, 1; do. contusion of, 1; do. wound of, 2; Heart, aneurism of, 1; do. disease of, 3; do. hypertrophy of, 2; Hæmoptisis, 1; Hemorrhage, 4; Hepatitis, 5; do. acute, 1; do. chronic, 1; Hernia, 1; Hydrocephalus, 2; Insanity 1; Intemperance, 11; Intestines, inflammation of, 3; do. perforation of, 1; Jaundice, 2; Labor, premature, 2; Laryngitis, 2; Leg, compound fracture of, 1; Marasmus, 4; Measles, 3; Meningitis, 6; Old age, 9; Paralysis, 6; Parotitis, 1; Pertussis, 8; Phrenitis, 1; Poisoned by Laudanum 2; Rheumatism 1; Ribs, fracture of, 1; Scalded (by steamboat explosion), 7; Scorbutis, 3; Scrofula, 1; Skull, fracture of, 1; Small Pox, 2; Spine, injury of, 1; Still-Born, 46; Strangulation, 1; Suddenly, 7; Sun Stroke, 6; Syphilis, 1; Tabes Mesenterica, 1; Tetanus, 15; Traumatic Tetanus, 1; Trismus Nescentium, 36; Tumor, 4; Typhus Icterodes, 2; Uncertain 93; Uterus, abcess of, 1; Verminose, 3; Vertebra, dislocation of, 1; Womb, disease of, 1; Wound, gun-shot, 1; do. penetrating, 1. Total 1662.

Of which 307 were under 10 years; 1438 were white, and 224 colored.

(From Reports of Board of Health.)

A. HESTER, Sec'y.

DR. B. DOWLER AND THE ACADEMY OF NATURAL SCIENCES, PHILADELPHIA.

We are gratified to learn that our confrère and fellow-citizen, Dr. Bennett Dowler, has received from this learned and scientific academy the honorable title of *Corresponding Member*. We believe he is the first physician of the South who has been thus honored; and his laborious researches into natural science and their kindred subjects, fully entitle him to this distinction. We congratulate Dr. D. and trust he may live to gain yet higher honors in the field of science. A. H.

NECROLOGY.

DIED at his residence in this city, October, 1848, W. M. CARPENTER, M. D., Professor of Materia Medica in the Medical Department of the University of Louisiana. For several months anterior to his death, Dr. C.'s health began to decline, and although naturally delicate, yet he continued up to within a few months of his death, to apply his mind to the various studies that engaged his attention. By diligent application and untiring zeal Dr. Carpenter had acquired, at the early age of 37 years, a large amount of valuable information on a variety of scientific subjects, especially on Botany, Geology and Practical Medicine.

For some time Dr. C. was connected with this Journal as one of its editors, and his contributions to its pages give abundant evidence of a mind well-stored with useful and scientific knowledge. A. H.

ABSTRACT OF A METEOROLOGICAL JOURNAL FOR 1848.

By D. T. LILLIE, AT THE CITY OF NEW ORLEANS.

Latitude, 29 deg. 57 min.; Longitude, 90 deg. 07 min. west of Greenwich.

WEEKLY. — 1848.	THERMOMETER.			BAROMETER.			COURSE OF WIND.	FORCE OF WIND, Ratio 1 to 10.	Rainy Days.	Quantity of Rain. — Inches.
	Max.	Min.	Range.	Max.	Min.	Range.				
Sept. - 2	91.0	78.0	13.0	30.20	29.90	0.30	N.W.	2 $\frac{3}{4}$	2	0.125
" - 9	87.5	75.0	12.5	30.18	30.00	0.18	S.W.	3 $\frac{1}{4}$	2	0.880
" - 16	89.5	77.0	12.5	30.19	29.95	0.24	S.E.	3	1	0.850
" - 23	86.0	65.0	21.0	30.30	29.90	0.40	N.E.	3 $\frac{3}{4}$	1	0.220
" - 30	82.5	60.5	22.0	30.22	29.90	0.32	N.W.	3 $\frac{1}{2}$	0	0.000
Oct. - 7	84.0	58.5	25.5	30.50	30.06	0.44	N.W.	3 $\frac{1}{2}$	0	0.000
" - 14	84.5	70.0	14.5	30.35	29.94	0.41	N.	3	1	0.595
" - 21	83.0	59.0	24.0	30.43	30.04	0.39	W.	3	1	0.285
" - 28	80.0	67.0	13.0	30.32	30.00	0.32	S.W.	3 $\frac{1}{4}$	3	1.350

REMARKS.—The Thermometer used for these observations is not attached to the Barometer, but is a self-registering one, and is placed in a fair exposure. Regular hours of observation, 8 A.M., 2 P.M. and 8 P.M.

The Barometer is located at an elevation of 19 feet above the level of the ocean, and is suspended clear of the wall of the building.

The Rain Gauge is graduated to the thousandth part of an inch, and the receiver is elevated 40 feet from the ground.

It will be perceived, by reference to the rain column, that but a small quantity of Rain has fallen during the last 9 weeks, say about 4 $\frac{1}{2}$ inches. During the corresponding period last year, there fell upwards of 13 inches, and in the same period of time, in 1846, 7 inches.

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OF

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THE NEW ORLEANS
MEDICAL AND SURGICAL JOURNAL.

JANUARY, 1849.

Part First.

ORIGINAL COMMUNICATIONS.

- 1.—*Reflections on the Nature of Instinct and Reason, and on the Physiology of Plants and Animals.* By WM. P. HORT, M. D. of New Orleans.

On this, as on most all subjects, a difference of opinion exists.

In Enfield's 'History of Philosophy' we are informed that Ananagorus, an eminent Greek philosopher of the ionic school, was the first amongst the Greeks who conceived Mind as detached from Matter, and as acting upon it with intelligence and design in the formation of the Universe. Before his time Mind and Matter were confounded.

This doctrine of Ananagorus soon led to the belief that the human mind, or reason, was an emanation from the Divine Mind, and consequently something very different from the Instinct observed in animals.

I am induced to believe that this has been the prevailing opinion from the time when Ananagorus flourished, to the present moment.

Many persons however have been, and still are, of opinion, that the instinct of animals and the reason of man are one and the same thing, differing only in degree, according to the greater or less perfection of organization. This is evidently the opinion of the Author of the *Vestiges of Creation* as expressed in the Chapter entitled "Hypothesis of the developement of the vegetable and animal Kingdoms." He maintains that man, the present head of the animal creation, is descended from the purest and first on the scale—countless ages having elapsed, during the slow but regular process of the gradual developement.

Instinct is generally defined to be that impulse, resulting from the peculiar nature of an animal, which prompts it to do certain things, without being directed in so acting, by reflection, or by the reasoning powers.

It may also be defined as the impulse which directs an animal to seek pleasure, and avoid pain.

This last definition is equally applicable to human reason. Epicurus says "the passions and affections may be reduced to two—pleasure and pain." And the same may be said of all intellectual action.

Metaphysicians define reason to be the art of comparing ideas and drawing conclusions. According to them, it involves the existence of sensation, perception, memory, association, judgment, motive and volition. If animals therefore possess these faculties of the mind, it would be fair to infer that they are capable of reasoning.

In the further elucidation of this subject, I propose to advert briefly

1. To the evidences of instinct or reason in animals.
2. To the instincts and habits of plants.
3. To the physiology of animals and plants, and a comparative view of the same.
4. To the bearing of Phrenology on the subject.
5. To the nature of Mind.
6. To the nature of vitality.
7. To the physical history of man; and
8. To the classification of the material world and its inhabitants; and conclude with an attempt to show wherein consists the true difference between Instinct and Reason.

A volume might be filled with curious and entertaining anecdotes of the sagacity of animals. A few cases however, well selected, will answer every purpose.

The squirrel in its migrations in search of food, has been known, on arriving at the bank of a river, to select a piece of bark for the purpose of crossing; this is launched with a skilful perception of the force and direction of the current. And should the wind be favorable, it raises its bushy tail to catch the breeze and be assisted in its voyage.

In this case there would appear to be perception of difficulties to be surmounted, and a knowledge of the means of obviating them. There is also calculation as to the current and the wind, and the capacity of the piece of bark to bear its weight.

We are informed by Huber, a French naturalist, that there are three classes in a community of Ants—the females, the workers and the military: the first engaged in domestic duties; the second in forming the encampment, the hillock, the excavations and the galleries; it is also their duty to provide the necessary supply of food. The third class perform regular military duties; they keep a vigilant guard of videttes all around the encampment, and repel to the best of their ability, all attacks of enemies. These various duties are performed with as much diligence, perseverance, regularity and order, as can be seen in the best regulated community of human beings. If a noise or disturbance be made in the neighborhood of an ant-hill, the nearest sentinel immediately manifests anxiety and alarm; he runs to and fro in every direction to discover the cause of the alarm, then proceeds to the interior of the encampment, and speedily returns with a strong body of fighting ants, who never hesitate to attack an enemy with great courage and resolution.

Different communities of Ants are sometimes engaged in war, which

generally ends in the extermination of one or the other party. After a pitched battle, they invariably carry off their dead and wounded.

If the intelligence manifested by ants, is by some believed to imply the exercise of reason, the same inference may be drawn from the ingenuity of the Spider. After manifesting admirable skill in arranging its web, a spider waits patiently for its prey. Should a fly or some small inoffensive insect become entangled in the meshes of the web, it is immediately seized and despatched; but should the intruder be a wasp or any strong insect provided with an offensive or defensive weapon, the spider, aware of danger, approaches with the utmost caution to reconnoitre the unceremonious visitor. It then remains quiet, calmly looking on, until ineffectual efforts to disengage itself and escape have weakened the physical power of the victim. Then it weaves its delicate threads around the wings of the insect, and having secured them, proceeds to bind the legs in the same manner. This operation completed, the spider retires for a short time to watch the result; and when fully satisfied that the prisoner is completely disarmed, it proceeds to accomplish the work of death. The next effort is to raise its prey, in a perpendicular line, to its retreat, and if it fail in this attempt, it will form two inclined planes; and availing itself of this mechanical power, finally succeeds.

In this case we see displayed the skill, caution and intelligence of an able and experienced general, in attacking and destroying an enemy. And what must we think of the subsequent successful application of a mechanical power?

The beaver in constructing a dam is not surpassed by any millwright. Large trees are felled, thrown into the water, and properly adjusted by this interesting and valuable animal. Clay and mud, thoroughly tempered, are next used to raise the bank or dam sufficiently high to make it strong enough to resist the force of the current, and thereby form a large pond adapted to its wants and purposes.

The elephant is remarkable for its intelligence. Amongst many instances of its remarkable sagacity, the following have been recorded. A walnut was thrown to one of these noble animals in some menagerie in Europe, which lodged just beyond the reach of its proboscis. After various ineffectual efforts to reach it, the animal appeared to reflect for some time. It then raised its head and blew forcibly against the wall in a direction which caused a re-action and brought the nut within its reach. In this instance, the angles of incidence and reflection must have been accurately calculated, and I doubt if any better evidence to establish the fact of animals being capable of reasoning could be adduced.

The war-elephant of Hindostan, it is well known, is trained to perform the grand salam, which he does by falling on the first joint of the fore-leg at a certain signal. On a journey or march between Ceylon and Madras a large war-elephant refused to perform this ceremony, and seemed to be terribly enraged. The keeper was ordered to explain the cause of it, but, as he appeared, the elephant advanced towards him and with one stroke of his trunk laid him dead at his feet. He then resumed his position in the line of march and performed the grand salam.

This circumstance created considerable alarm, and was altogether unaccountable, until the wife of the deceased stated that she had always

dreaded something of the kind, as her husband was in the habit of robbing the animal of his rations of rice after they were served up to him in his crib. Now it would seem that this animal not only reasoned, but was in fact conscious of those fundamental principles of law, which Blackstone would call, "the rights of persons and the rights of things."

On another occasion, an elephant slew his keeper in a furious passion, in consequence of very harsh and cruel treatment which he had just received from him—The man's wife hearing of what had occurred, repaired to the spot accompanied by her young children, and in a paroxysm of rage and despair threw them between the fore-legs of the enraged elephant. In an instant his fierce anger gave way to repentant sorrow and compassionate feeling; he caressed the young children with the utmost tenderness, and seemed to be desponding and dejected for some time afterwards. In this case, like a judge of a criminal court, he seemed to have well discriminated between the innocent and the guilty, and to have been mindful of mercy almost at the moment that he was sternly administering justice.

There is an amusing story of a poor shoe-black who resided in the City of Paris, and who seated himself daily on one of the bridges over the Seine. He had trained his dog to procure work for him, who effected the object by plunging his feet in mud and water and soiling, as if accidentally, the shoes and boots of foot passengers. Of course the presence of the shoe-black was very acceptable, and his services were immediately in requisition.

A dog has been taught to know the value of money. His owner frequently took him to market when he was very hungry; the butcher who was in the secret, would show him meat and tempt him severely, but refuse to give it up until the owner of the dog produced a small piece of money, which he taught the dog to push towards the butcher with his paw; this being often repeated, a permanent impression of cause and effect in the case of beef and money was made on the dog; subsequently he often went to market by himself, whenever his owner, or some other person, aware of the fact, would give him a piece of money, and he never failed to return with a due allowance of meat.

It is said that a gentleman who went from Edinburg to London on a visit, had a fine dog with him, which was attacked and severely beaten by three or four English dogs. After this circumstance, his master kept him in confinement, but he seemed to be completely changed; he unheeded any acts of kindness, became sullen and morose, and appeared to be quite disheartened and discontented. One day he made his escape, evaded his master, went home to Edinburg, and in a short time returned to London with three other Scotch dogs. He then searched out his former cowardly as sailants, and with his reinforcement revenged his wrongs, by attacking the English dogs and beating them severely in their turn.

The nest of a martin was in the absence of the proprietor occupied by a sparrow. The martin on its return made fruitless efforts to dislodge the intruder. It then flew away, and in a short time returned with other martins and renewed the attack which again proved unavailing. A second retreat was made, and after holding a very noisy council of war, the discomfited birds procured stiff mud with which they

blocked up the aperture of the nest ; in that way destroying their enemy and gratifying their revenge.

Most all animals give evidence of memory, which in some instances is remarkable. Homer represents the old dog of Ulysses 'as recognizing his master after an absence from Ithaca of more than ten years.' And who has forgotten the sudden exclamation of old dame Wilson "the dog kens ye," when Morton returned to Scotland and the home of his childhood, after an equally long absence. In such illustrations Homer and Scott are always true to nature.

Horses have often given proof of retentive memory. Many years ago I was nearly thrown from a horse who was frightened by a large cotton basket which some careless negro had left in the bushes by the road side. About twelve months afterwards, the same horse started with me at the very spot he had previously seen the basket, which however was no longer there. During nearly the whole interval of time he had been three hundred miles off in another State.

If the existence of reason in animals cannot be proved by such anecdotes as the foregoing, further illustrations would equally fail to establish the fact.

Let us now take up one of the strongest cases, and see how far the definition of reason assumed in the premises is applicable to it. The first link in the chain was sensation, not an intellectual faculty, but a simple principle of vitality, common to plants as well as to animals, and absolutely necessary for the exercise of intellectual powers. Perception cannot exist without sensation, but the latter must always precede the former.

In the case of the spider then, as soon as an impression is made on the delicate threads of the web by some insect, the sensation is conveyed to the spider, which perceives it ; memory and association apparently aid in the discrimination of the object ; judgment determines if it may be approached with impunity or with caution, and regulates the whole course of operations from the first perception of its prey, until it is finally secured in the retreat of the spider. There is also action in the case, which may be referred to volition, and thus we have an assemblage of nearly all those intellectual faculties which are evidences of reason according to the definition of metaphysicians.

Having now briefly adverted to the remarkable instincts of animals, which by some are considered sufficient evidence of the existence of intellectual power or reason, I propose to apply the same mode of argument to plants.

The sensitive plant on receiving the slightest touch, instinctively shrinks and closes its leaves ; the stems droop, and the whole plant presents the appearance of death, like the hedge-hog and the lady-bug, who under the same circumstances will roll themselves up into a ball, feigning to be dead. The plant then manifests sensation and action ; we can also infer the agency of perception, volition, and motive. A knowledge of impending injury appears to exist, as well as the exercise of judgment in affecting the semblance of death as the best means of escape.

Treating the question in a strictly logical sense, we might as well infer the existence of reason in this case, as in any of the instances of animal sagacity.

Some plants when touched emit a poison for their defence, as the *Rhus Radicans*, the *Urtica Urens* &c; so will the Polecat and other animals and insects, who are provided by nature with a similar means of defence. Many shrubs and trees are armed with sharp thorns which cause as painful a wound as the sting of a bee or wasp; all of which appear to be designed by nature as weapons of defence for both the animal and the plant.

The sunflower and the cotton plant present their leaves and flowers to the rising sun to enjoy the full benefit derived from light and heat. As the sun rises in the heavens, the position of the leaves and flower is changed to correspond with that of the sun; and when it arrives at the zenith the leaves and flowers are level or horizontal; again when the sun descends in the West, they still follow its movements, and at sunset are in a position the very reverse of what they were at sunrise.

Most plants to which light and heat are essential, expand their flowers in the day-time and close them at night. Guabert, a botanist, in a recent sitting of the French Academy of Sciences, attributed the remarkable mortality of the trees in the Parisian Boulevards to the nightly illumination with Gas, since trees (and he might have added most plants—H.) as well as living creatures need repose at night.

Some plants only expand their flowers at a certain hour in the afternoon; while the night blooming *Cereus* and others of the same species expand their magnificent flowers at midnight, and close them before the dawn of day; showing that there are plants as well as animals that have been ordained for night, and are accordingly active only during those hours.

It is well known that plants confined in a cellar or dark room will grow in the direction of the spot where light may enter; and the roots of the Indian corn or Maize, planted on the edge of a hill-side, have been known in a time of long continued drouth, to extend thirty feet to water at the bottom of the hill, while all growth was suspended on the opposite side.

It is common amongst botanists to speak of the instincts of plants, as naturalists speak of the instincts of animals; and there can be little doubt of the fact, for both imply a certain degree of intelligence, which, however modified, may be referred to the same or similar causes.

The *Dionea Muscipula* (or Venus' fly trap) is an interesting plant found in some parts of the Southern States in the pine woods near the seaboard. It expands its strangely organized radical leaves, forming something which resembles a rat trap; on the inner surface are distributed highly sensitive and delicate points which project slightly. An insect may enter the trap with impunity so long as it happens not to touch one of the sensitive points, but the moment one is touched, the trap closes and destroys the insect. It is possible that this plant derives a portion of its nutrition in this way, as I have observed that for two or three days after a large fly or bug has been secured, the trap remains closed, and when it opens, all the fluids of the insect appear to have been absorbed, as nothing remains but a dry carcass, which a very light breeze will remove. Let us now see how this case will compare with one of the instances of animal instinct or intelligence. In the delicate point touched resembling a nervous filament, there is evidently sensa-

tion, of which there is perception; judgment is manifested in skilfully and rapidly closing the trap on something that may be beneficial or injurious to the plant; motive, volition and action may also be inferred—shall we therefore conclude that this plant reasons?

This analogy is further sustained by the fact that the organization of some plants is more perfect than that of some animals; and almost as perfect as that of any animal, with the exception of the power of locomotion, and consequent superior facility of obtaining food.

Plants require nutriment as well as animals, also air, light, heat, electricity and water, they have a vascular system in which a regular circulation is carried on; they have secreting, excreting and absorbing organs of their fluids.

Do some animals hibernate? So do plants. Are plants incapable of moving from the spot where they grow? So are some animals. In fact there are some living or organized substances which naturalists are at a loss to refer to the animal or vegetable kingdom. Plants as well as animals are capable of reproducing or propagating their species.

What do we find in the animal economy performed by the digestive and assimilating organs, more wonderful than what we see in the organs and functions of plants?

According to Liebig, Matteucci, and other eminent chemists of the present age who have devoted much attention to animal chemistry, animal heat is the result of chemical action, which is a process of slow combustion continually going on in all parts of the body, whereby carbonic acid is generated. So, according to Professor Matteucci, the heat developed in vegetables by germination is a phenomenon of chemical action, occasioned by the combination of oxygen with the carbon of the germinating grain. It is known, that in germination there is an absorption of oxygen and a disengagement of Carbonic acid; that the *diastase* converts the starch into dextrine and sugar, which afterwards disappear as carbonic acid. It is curious that in plants, as in animals, it is starch and sugar which, by combustion, disengage the heat peculiar to these bodies. It is also thus that we must explain the heat which accompanies the fecundation of plants, and it is for this reason that we see, in sugar-cane, beet-root, and carrot, the sugar disappear after flowering and fructification. (Conclusion of the Professor's 7th lecture, on sanguification—Nutrition—Animal heat.) Plants, animals, and man are alike liable to various diseases, and even epidemics.*

The various kinds of food taken by an animal are digested in the stomach, and reduced to a fluid somewhat of a homogeneous nature, called chyme or chyle; this fluid finds its way into the blood vessels, where it becomes mixed with, and lost in the mass of blood; the blood is distributed to the secreting organs, whence every part is supplied with its appropriate substance, which completes the assimilating process. What is not necessary for the nutrition of the animal is absorbed and exhaled, and in various ways passed out of the system.

Plants receive nutriment through their roots and also sometimes by absorption by means of the leaves. The various substances taken up

* Epidemics prevail among the lower animals and amongst plants, and are governed by the same laws; and so far as the subject has been investigated, they arise from the same causes, to wit, parasites.

by a plant probably undergo some process analagous to digestion in passing through the roots, when the sap is formed, which may be compared to the blood of animals; this sap finds its way to every part of the plant, and from it are formed the leaves, buds, blossoms and fruit, and are secreted or eliminated the various diversified productions of the plant or tree. Thus from the sap in the pine tree which grows in barren sandy soil near the southern sea board, are produced the resin and the turpentine. From the sap in the peach tree are formed the fruit, which is wholesome and nutritious, and the prussic acid which may be extracted from the leaves and kernels. Whatever is not required by the plant may be exhaled by the leaves, but is generally returned to the earth by the descending sap, and from this cause, as well as from exhaustion, the soil may become noxious to the plant. As a general rule carbon is abundantly absorbed by plants and oxygen is given out.

Now although the blood of animals and the sap of plants contain all the elements of the various animal and vegetable growths and productions, yet no chemist could predict from an examination of the blood or the sap, what would be the results of combinations under the influence of vital laws; prussic acid cannot be detected in the sap of the peach trees, nor resin or turpentine in that of the pine.

At one time, every thing in the economy of plants was referred in the schools of philosophy to the principles of hydraulics.

Next we have the theory of Bernad in St. Pierre—that plants are animated by different families of very minute animalcules which produce by their labors all the various and curious results that we notice in plants: That a tree or plant flourishes so long as these families or colonies continue their labors; but that it perishes so soon as any adverse circumstance renders it unfit for their abode, and compels them to abandon it.

Exceedingly minute animalcules may be seen by the aid of a powerful microscope moving about in the sap, and secreted matter of trees and plants, precisely as they are seen in all the fluids of animals. But the parasites of animals abound in a diseased state of the body, while in the position of vigorous health, they are comparatively scarce. The analogy therefore does not hold good—for this fact proves the very reverse of St. Pierre's theory.

Rejecting all such theories, and all explanations according to mechanical, or chemical laws, of the phenomena of life, I proceed to remark that what is inferred from the organization and functions of animals, may with equal propriety be inferred from the organization &c. of plants; and if the wonderful habits and actions of animals, adapted to their support, and preservation from suffering and death, imply intelligence, then should the habits and actions of plants, exerted for the same ends, imply intelligence—not equal to that manifested by some animals, but the same in principle, differing only in degree.

Again, if it be true that mind, as some affirm, is a property of organized molecular matter, the ratio of the same being determined by the greater or less perfection of organization and cerebral developement, it follows that animals and plants must possess mind as well as man, and that they reason by means of organization and function in the same way that man reasons, although always in proportion to their peculiar organization.

Some persons may be misled by supposing that cerebral developement is something entirely different from lower forms of organization and function; and may say that the analogies presented do not hold good, because plants, some animals and insects have no brain. But no such difference actually exists; the brain of man is only a higher degree of organization than the brain of an Ape, a Dog, or an Elephant, and their brains bear a similar relation to the smallest amount of brain or cerebral matter that we can perceive in an inferior animal or in an insect. The substance of the brain is the same as that of the nerves, whether the amount be greater or less, or the organization higher or lower.

With respect to plants, as in the case of the sensitive plant, Venus, fly trap and several others, we have the strongest presumptive evidence of the presence of nerves, of something so much resembling nerves in its effects, as to make it more easy to admit the identity than to demonstrate the difference. Besides, from the laws of physiology, it is difficult to conceive of faculties and functions, as sensation, perception, secretion, absorption, &c., without supposing the existence of nerves and plants possess and exhibit these faculties and functions.

As intimately connected with the subject, I shall next advert to certain opinions entertained by some of the disciples of Gall and Spurzheim, who maintain that mind is the result of organization; a mere property of matter, as color, smell and form are said to be properties of matter.

The power of the mind according to this doctrine, is greater or less in proportion to the harmony of the cerebral developement, the perfection of the nervous system, and the regularity of functions; from which it follows that mind ceases to exist when death occurs; and the instinct of a monad differs from the intellect of the most profound philosopher, or an enlightened statesman, only in degree.

From such an inference Mr. Combe of Edinburg, has ably and logically rescued Phrenology. According to this writer, the brain is the material organ of the mind, the medium, through which, by means of the organs of sense, it holds intercourse with the external objects in the world; which is susceptible of an existence independent of the body, and which will survive death and the grave.

It must be admitted that many eminent philosophers gifted with the strongest reasoning powers, have doubted and combatted this opinion.

And here it is well to observe that there is a great difference between believing what is simply *beyond* the comprehension of human intellect, and what is evidently *contrary* to reason. In every department of science, we see the results of certain laws; as of the law of chemical affinity in the laboratory; the laws of vegetation in the field and the forest; the laws of crystalization and of a attraction; yet everywhere we encounter a limit which the human mind cannot transcend; this is no reason however why we should doubt the existence of such laws; and certainly no intelligent man has ever doubted on the subject. The case is very different however when we read of spirit or soul, as something essentially distinct from matter. Reflection throws no light on this proposition; for the longer we reflect, the more we must become convinced that it is contrary to reason; and this is probably the cause why a great and important truth believed by a vast majority of mankind in all ages and in

all countries, has been rejected by so many distinguished philosophers?

If the human soul be not material, of what does it consist. It can have no actual existence; for a thing or a substance either exists, or it does not exist; if it exists, it must be an entity, and occupy some portion of space; and whatever occupies a portion of space, must be matter. Not necessarily such matter as this earth is composed of, but a description of refined matter of which we can form but a very vague conception; for there is no assignable limit to terms of matter. To illustrate this position, I cannot do better than quote from a pamphlet on the constitution of matter by Prof. Riddell: "To assign a last term of matter on the scale of minuteness, would be to set limits to a subject which wears every aspect of infinity. Whatever we call great or small, cannot be absolutely great or small only relatively so. And if there be an assignable ultimate atom of matter, no reason can be given why it should have any special limit of dimensions. Admit its existence, and then comes the natural inquiry as to its theoretical divisibility; which affirmatively forcing itself upon our conviction, we are again compelled to assent that there cannot be assigned an ultimate term of matter. Should it be alledged that it is unphilosophical to invoke the agency of matter so attenuated as to elude our direct observations, I would reply that I think the existence of several material media, differing from each other almost immeasurably in respect to degrees of attenuation, or the relative sizes of their component atoms, may be logically deduced from known phenomena of nature. The impulses causing sound, travel in the ponderable, and comparatively gross medium air, at the rate of 1142 feet per second. The impulses causing light travel in the imponderable and refined medium existing between us and the sun, at the rate of 192,500 miles per second. We infer the luminiferous medium is different from, and far more refined than air, from its apparent want of weight, and the greater velocity of impulses it transmits. For it may be conclusively shown, that the less dense and the more refined the material medium, in respect to the smallness of its component atoms, the greater the velocity with which it transmits impulsively any given momentum. Now as the influence causing gravity, has a velocity according to La Place, at least 100 millions times greater than light, it is philosophical to infer that a still more refined medium than the luminiferous, is the instrument of its transmission." (pp. 2. 3.)

If the foregoing chain of reasoning be strictly logical, then the soul of man must be material as well as his body; not composed of the same comparatively gross material atoms, but of infinitely more attenuated and refined terms of matter, which we may, by the light of nature alone believe, though, which we cannot mathematically or philosophically demonstrate, to be capable of a continuous associated existence after death and its separation from the body; retaining its personal identity; visible to and cognizable by, kindred matter in similar circumstances, to which I would apply the appellation *spiritualized matter*; but of which we shall, in the present state of existence, probably form no definite or clear conception."

There may be here something beyond the grasp of intellect, but certainly nothing at variance with reason.

Let this view of the subject be rejected, and what remains as regards the human soul but the vague and unsatisfactory ideal philosophy of

Bishop Berkly who denied the existence of matter under all conditions and circumstances— or else we must adopt that gloomy belief so repulsive to the social feelings of the heart, and the common instinct and general faith of mankind — that death is the end of our being, and consigns both body and soul to the grave, there in due process of time to mingle in the general mass of matter, of which this earth is composed.

Is mind then a mere property of matter peculiarly organised, or does it consist of distinct and very different terms of highly refined matter, mysteriously connected with the body for a limited portion of time—and governed by its own specific laws, modified somewhat during the continuance of its temporary union with the body?

A belief in the immortality of the soul has prevailed in all ages and in all countries of which we have any knowledge ; yet this is not absolute proof of the truth of the doctrine ; it amounts to nothing more than strong presumptive evidence. Reason, however may greatly strengthen the probability of this truth, if its exercise will not lead to certain conclusions.

The mind of man, composed of spiritualized matter, is often clearer and stronger as the period of its separation from the body draws near. This is sometimes the case in violent and rapid diseases, as well as in those which are chronic — such as consumption in its various forms ; also after fatal injury to the spine, where important functions may be destroyed, and *sensorial* life cease in many parts of the body.

The following case, communicated to me by persons of undoubted veracity, will prove interesting by way of illustration, while at the same time it confirms the result of my personal experience and observation on various occasions.

An elderly gentleman, an elder in a Presbyterian church near Newark in New Jersey, had throughout life experienced great difficulty in giving utterance to his thoughts. In public, he never attempted to speak ; and by his family, friends and neighbours was always considered as a man of remarkably few words. This did not so much arise from diffidence as from inability.

In harvesting his crop, he fell from the top of a stack of oats, and being a heavy man broke his neck. He was carried home, and a surgeon was sent for. The medical attendant soon arrived and after a short examination, in reply to a question put by the patient, he stated that he could not possibly live more than from twenty to twenty-four hours, but might die much sooner. The extremities were insensible to pain and paralysed — and some of the most important functions of the body were either entirely and for ever suspended, or so impaired as to be of no further use. The only way in which life could be prolonged for the time stated by the Doctor, was the communication kept up between the heart, lungs and brain by means of the parvagus and great sympathetic nerves, which come directly from the brain. In this situation, he requested the attendance of his lawyer, and to the surprise of all who were present, dictated his will without a moments hesitation in clear language evincing considerable strength of mind. That business over, he desired that every member of his family, and as many of his friends and neighbours as could be collected, should assemble in his room, when he addressed them for two hours in a strain of eloquence which they had never heard surpassed. His theme was religion — his object to convince all his

hearers of its truth — himself, a dying man, he cited as a proof of its ability to comfort and sustain the soul in that trying hour of approaching dissolution. After having discharged this duty, he took affectionate leave of all, and desired to be no more spoken to or disturbed; the remaining hours he passed apparently in silent prayer — and at the last moment in the very act of death, when he could no longer see nor hear, he exclaimed that his mind was clearer than ever, and that all was right.

I have over and over again had the most positive assurance of consciousness being retained to the last moment of life, or as it may be more clearly expressed, to the instant of separation occurring between the spiritualized matter of the mind, and the brain, its comparatively gross material organ.

It may be asked, why is this not always the case.—Because in many diseases the brain is so impaired, as to be no longer subservient to the mind, whose means of communication are therefore cut off; and in other instances its motions may be so much disturbed temporarily by the diseased action of the brain, as to present singular phenomena, as delirium in fever, and madness in its various phases—which, to many persons, must be incomprehensible on the supposition that mind has an existence distinct from that of the body, and yet is not material in its nature. For matter can only act on matter.*

Molecular matter, of which this earth is composed, is governed uniformly by certain and peculiar laws, which are well known to all who study them.

If mind be a mere property of such matter, it would necessarily be controlled by those laws. If however the spiritualised matter of mind differs from ordinary molecular matter, then are we prepared for the exhibition of new and different laws.

One of the fixed laws of common matter is that certain causes invariably produce certain effects, as seen in the analysis and synthesis of substances in the laboratory.

If an impression be made on wax with a seal, when time has obliterated that impression, it will never be revived unless the cause operates again, and the seal is re-applied. The elements of which air and water are composed, oxygen and nitrogen in one case, and oxygen and hydrogen in the other, if separated, will never again form air or water, without the operation of the original cause which produced the combination.

But an idea in the mind may be lost for years after the cause that produced it has ceased to operate, and will suddenly and unexpectedly re-appear thousands of miles from the place where it was first received; not only without the operation of the original cause, but without any apparent cause at all—not even that of association or suggestion.

There is a singular and well authenticated case on record, of the alternate loss and recovery of memory, which cannot be accounted for, if the operations of the mind are governed by the laws of common matter.

A lady in London who had received a very moderate education, lost, after a fit of sickness, the memory of all knowledge she had previously

* This doctrine is clearly taught in the ancient Jewish scripture. Elijah is represented as restoring to life the dead son of a poor widow: the language used is explicit “and the soul of the child came into him again.” where upon he was presented *alive* to his mother.

acquired. She received a second and much better education; but what is most singular, after experiencing a second attack of sickness, all that she had last learned was totally forgotten, and the previous knowledge that she had lost was regained. This singular phenomenon was several times repeated; she recollecting alternately the knowledge previously, and that subsequently acquired.

Dreams lead to the conclusion that the matter of mind is governed by very different laws, from those physical laws with which we are familiar. The latter are always uniform in their action, in a well regulated mind; there may be something approaching to uniformity of action, when a man is awake, but there is usually nothing of the kind when he is asleep and dreaming.

Then, as a general rule, reason does not act at all; imagination and memory are active, and the strangest and most incongruous ideas crowd the mind without any analogy, association, or order. In the morning reason resumes its empire and order is again restored.

There is nothing more curious and inexplicable than the phenomena of dreams. Sometimes they can be traced to the last subject that occupied the mind before going to sleep, but mixed up in great confusion with other ideas; at other times, dreams have no reference whatever to anything that has recently engaged our attention, but appear to be made up of disconnected fragments of romances and legends distorted by the wildest imagination.

And there are occasions, exceptions to the general rule, after the mind has been much strained, as after an equal and doubtful game of chess, or the investigation of a very difficult mathematical problem, or a close and interesting discussion of an abstract philosophical question, when the train of reasoning will go on scarcely interrupted throughout the night.

After playing a match at chess many years ago with a very equal player, I found myself in a position at a late hour of the night, from which I could see no way of extricating myself; being weary and sleepy, I requested that the game might be postponed till the next evening.

During the night I dreamed of nothing but that game of chess, and what is very remarkable, played it out successfully while asleep. Recollecting the moves in the morning, I made a memorandum of them, and the next evening beat my opponent, to his great surprise.

The general character of a man, or rather the tone of his mind, has much to do with the nature of his dreams. If habitually of a cheerful disposition, his dreams will be agreeable, unless disturbed by indigestion. On the contrary, if the turn of mind be gloomy, the dreams will probably be of that description, which we find at the opening of Young's *Night Thoughts*:

"I wake, emerging from a sea of dreams
Tumultuous; where my wrecked, desponding thought,
From wave to wave of fancied misery,
At random drove, her helm of reason lost."

Or as we read in Shakespeare, written at an earlier period, where he makes Hamlet exclaim,

"To die, to sleep—perchance to dream,
Aye, there's the rub."

I offer no explanation of these strange phenomena, as it is a subject

foreign to my purpose at the present time. They have been introduced to show how little resemblance there is between the laws and motions of spiritualised matter of mind, and of the molecular matter of the earth. Indeed Socrates considered the phenomena of dreaming as proof of the immortality of the soul, and consequently of its being something distinct in its nature from the body.

There are, and can be, no innate ideas of external objects in the mind. All ideas of such objects, and everything which makes up the sum of human knowledge, are obtained by means of the organs of sense; yet after an impression has been made upon the mind, and perception has taken place, and memory has acted, ordinary physical laws cease to operate, and the matter of mind acts independently by its own peculiar laws.

Yet there are persons who combat this opinion so clearly expressed by Mr. Locke in his "Essay on the Human Understanding."*

The best decision in this instance is the practical one—Let any person point out any fact with which he is acquainted or advert to knowledge on any subject, of which he may be possessed, and if he reflects at all, he will not fail to discover that every idea that he has, or ever had, concerning any thing, has been received through one of the organs of sense. Every fact in science, and every truth in nature, is independent of the human mind, and is not in the smallest degree affected by man's knowledge or ignorance of their existence.

The organs of sense might all be impaired or destroyed, yet mind could act as vigorously as ever on all knowledge previously acquired. And moreover, although the distinctness of the ideas originally acquired, depends on the proper organization and developement of the nerves of the senses, it does not follow if the organs of sight, or hearing or smell are weak, that the faculties of the mind will be weak in proportion, which would be the case if mind were the mere result of physical organization.

We often see the mind remarkably active with one or more of the organs of sense entirely defective. Of this there is a striking illustration in the case of those unfortunate persons who are born deaf and dumb. A system of education adapted to their peculiar condition, has been contrived by the ingenuity of human reason, capable of rousing their minds to action, and of imparting to them all kinds of useful and scientific knowledge.

Their minds with such defective organization as regards some of the nerves of sense, are in all respects as strong as the minds of those whose organization is complete. I have sometimes thought that their reasoning powers when fully developed were stronger on an average than those of persons provided by nature with all the organs of sense, and consequent means of acquiring knowledge. The great excellence of the

* John Locke, the author of the "Essay on the Human Understanding," published in 1690, admits that "it might have pleased God to bestow a power of thinking on matter."

For this philosophical and reasonable admission, he has been severely criticised and censured. But to what does, or can this power of thinking belong, if it does not appertain to some term or terms of highly spiritualized matter? St. Paul plainly tells us, that "there is a natural body, and there is a spiritual body."

poems of Homer and Milton has often been ascribed to both having written when deprived of sight.

There was a Frenchman in this City six or seven years ago, who was born deaf and dumb; he was a very sensible and well educated man, he could converse on almost any subject by means of the usual signs, or by writing; he understood the English, French, and Spanish languages, and he never for a moment appeared to be at a loss for a reply to any question designed to perplex him.

If the form of the brain determined the power of the mind, and the propensities and character of a man according to the fixed operations of what we call physical laws, the result would be as certain as the laws of gravitation or vegetation. But this is not the case.

The most experienced phrenologists are often deceived. They can accurately enough estimate the form of the brain, and consequent presence and force of mental faculties. They can calculate with exactness the relative proportions of the anterior and posterior brain, and of the region of the moral faculties. They can point out the natural tendencies of the man, and the *probability* of his acting so and so. But beyond this they cannot go. Reason, education and religion often triumph over the worst moral indications, and diligence and persevering application over an unpromising cerebral developement. The man highly gifted by nature, may, like a rich uncultivated soil, produce little else but rank and poisonous weeds; while very moderate mental power diligently improved, like a poor soil well cultivated, be made productive of much that is good and useful.

Even the very temperaments on which character so much depends, may in the course of time be entirely changed.

Rapid as are the impulses of matter in certain known media, to which allusion has already been made, the rapidity of the motions of mind must far exceed them all. This is a clear inference from the fact that the velocity of impulses or of motions, depends on the degree of refinement of the term of matter, through which those impulses are transmitted, or in which those motions occur.

The medium transmitting the impulses of gravitation, which according to Laplace admits of impulses at least 100 millions of times more rapid than those which cause light, produce only physical or mechanical phenomena. How much more refined then must be the matter of mind, which is endowed with high intelligence, and capable of memory, association, imagination, thought, and deep trains of reasoning. In fact, it may be inferred, that the rapidity of its motions must in an infinite ratio exceed those, of which any term of matter producing merely physical or mechanical results, may be susceptible.

How is it possible to reconcile the foregoing facts and remarks with the idea that mind is only a property of molecular matter peculiarly organized—or that character necessarily depends on the form of the brain and the developement and harmony of functions?

I therefore conclude, 1st, that mind is not a property of molecular matter; since it is impossible for the greatest rapidity of motion of which the atoms of molecular matter are known to be capable, to give rise to the infinitely more rapid motions of the atoms of the mind when it is actively engaged. This will appear perfectly plain, if the rapidity of the

impulses producing gravitation is compared with the most rapid impulses in molecular matter.

2ndly. That mind, if an entity, which it must be to have an actual existence, undoubtedly consists of some form of matter infinitely refined in comparison with what we usually recognise as matter.

3rdly. That character is not necessarily dependent on the form or organisation of the brain; but that it is developed and controlled by education, religion, moral influences, by the general circumstances in which it may be placed, and the prevailing influences to which it may be exposed. To believe otherwise, would be to destroy the free agency and dignity of man, and make him any thing but what he really is, a moral and accountable being, and a fit subject for both human and divine laws.

Having disposed of the foregoing questions, and briefly adverted to the analogy existing between animals and plants, as seen in their instincts, organisation and functions, I shall now proceed to make some remarks on the subject of vitality.

Molecular matter is usually divided into elementary matter, the atoms of which are supposed to be homogeneous — as the metals and gases, and all substances that Chemists are at present unable to decompose: And compound matter, which is supposed to consist of heterogeneous atoms. The atoms of the former being held together by cohesive attraction, and those of the latter by chemical laws, generally called affinities.

With a view of not being misunderstood, I use the ordinary language of the schools, although I am impressed with the conviction that all attraction, including chemical affinity, results from motions in certain terms of matter, agreeably to the suggestions advanced by Professor Riddell in a memoir on the constitution of matter. (vide *N. O. Med. Journal* — March 1846.)

In the departments of natural science, all that relates to molecular matter, as Mineralogy, Geology, Chemistry, Crystalization, Mechanics etc., affords no appearance of what is called vitality: But on entering the field of Botany, we are immediately struck with the developement of a new phenomenon; we see a living plant, with organization, and functions of absorption, secretion and exhalation, totally different from any thing that we can observe in the form of molecular matter not endowed with vitality.

The question then naturally arises, — What is life or vitality? What is this new principle which characterises plants and animals? Is it the result of a certain organization of molecular matter — or of electricity, — or of chemical action, as some maintain — or is it, as Richerand observes “a principle superadded to matter”?

The consideration of vitality as something independent of organised matter, or as being the effect or result of the same, is entirely distinct from the question of the origin of the living things which are found on the surface of this earth. Geology clearly refers to an origin of plants, inferior animals, and man; it settles that point, and demonstrates the different Eras when plants and animals made their first appearance. Was this the result of creative power, or of electro-chemical action? Man was the last of living entities in making his appearance on this earth, and if the changes which are imperceptibly taking place on the surface

of the earth are sufficient to induce us to suspect the production of new living entities from time to time, we should rather expect, according to the law of progress or theory of developement, to see some improvement on man, rather than obscure forms of insect life. To establish any such result, incontrovertible facts are indispensably necessary; and if established, the question again arises — Is it the result of creative energy, or a mere chance effect of chemical action.

In an article on the causes of disease which I published in the N.O. Med. Journal in January 1846, the following remarks appear. “The mode in which infusory animalcules are produced and multiplied is involved in much obscurity. Many distinguished Naturalists, adopting the views of Buffon, have regarded them as the product of an inherent power belonging to a certain class of material particles, which, in circumstances favorable to its operation, tends to form those minute organizations; and in this manner, they explain how the same organic matter which had composed former living aggregates, on the dissolution of their union, reappear under new forms of life, and gives rise to the phenomenon of innumerable animalcules starting into being, and commencing a new, but fleeting career of existence. Yet the analogy of every other department of the animal and vegetable kingdoms is directly opposed to the supposition that any living being can arise without its having been originally derived from an individual of the same species as itself, and of which it once formed a part. The difficulty, which the hypothesis of the spontaneous production of infusory animalcules professes to remove, consists in our inability to trace the pre-existence of the germs in the fluid where these animalcules are found to arise, and to follow the operations of nature in these regions of infinite minuteness.” It would be unphilosophical to array, what is at best but an hypothesis, in relation to an obscure class of animalcules, as evidence against the universal analogies of the vegetable and animal kingdoms. But we are told that the experiments of Mr. Crosse are something more than an hypothesis, that they amount to absolute demonstration.

They are described as follows in the “Vestiges of Creation” :

“We can even conceive that man, in his many doings upon the surface of the earth, may occasionally, without his being aware of it, or otherwise, act as an instrument in preparing the association of conditions under which the creative laws work; and perhaps some instances of his having acted as such an instrument have actually occurred in our own time.

I allude, of course, to the experiments conducted a few years ago by Mr. Crosse which seemed to result in the production of a heretofore unknown species of insect in considerable numbers. ***** Mr. Crosse was pursuing some experiments in crystallization, causing a powerful voltaic battery to operate upon a saturated solution of silicate of potash, when the insects unexpectedly made their appearance. He afterwards tried nitrate of copper which is a deadly poison, and from that fluid also did live insects emerge. Discouraged by the reception of his experiments, Mr. Crosse soon discontinued them; but they were some years afterwards pursued by Mr. Weekes of Sandwich, with precisely the same results. This gentleman besides trying the first of the above substances, employed ferrocyanuret of potassium on account of its contain-

ning a larger proportion of carbon, the principal element of organic bodies ; and from this substance the insects were produced *in increased numbers*. A few weeks sufficed for this experiment, with the powerful battery of Mr. Crosse ; but the first attempts of Mr. Weekes required about eleven months, a ground of presumption in itself that the electricity was chiefly concerned in the phenomenon. The changes undergone by the fluid operated upon, were in both cases remarkable, and nearly alike. In Mr. Weekes' apparatus, the silicate of potash became first turbid, then of a milky appearance ; round the negative wire of the battery, dipped into the fluid, there gathered a quantity of *gelatinous matter*, a part of the process which is very striking, when we remember that gelatin is one of the *proximate principles*, or first compounds, out of which animal bodies are formed, though, of course, we should require further proofs to satisfy us that the matter here concerned was gelatin. From the matter, whatever was its nature, Mr. Weekes observed one of the insects in the very act of emerging, immediately after which it ascended to the surface of the fluid, and sought concealment in an obscure corner of the apparatus. The insects produced by both experimentalists seem to have been the same ; a species of *Acarus*, minute and semi-transparent, and furnished with long bristles, which can only be seen by the aid of the microscope. It is worthy of remark, that some of those insects, soon after their existence had commenced, were found to be likely to extend their species. They were sometimes observed to go back to the fluid to feed, and occasionally they devoured each other.****
***** For the presumption that an act of aboriginal creation did take place, there is this to be said, that in Mr. Weekes experiments, every care that ingenuity could devise was taken to exclude the possibility of a developement of the insect from ova. The wood of the frame was baked in a powerful heat ; a bell shaped glass covered the apparatus, and from this the atmosphere was excluded by the fumes constantly rising from the liquid, for the emission of which there was an aperture so arranged at the top of the glass, that only these fumes could pass. The water was distilled, and the substance of the silicate had been subjected to white heat. Thus every source of fallacy seemed to be shut up. (pp. 138. 139. 141.)

I have quoted at length on the subject of these experiments because they are of paramount importance in a philosophical point of view as compared with mere opinions, and hypotheses.

Are they then sufficient to establish the fact, contrary to what has been considered a general rule as regards living things, that cases of spontaneous existence have occurred under the operation of chemical laws and without the influence of any preceding germ of vitality ? The author of the "Vestiges" is cautious and judicious in his remarks on the subject ; he observes that the experiments *seemed* to result etc. ; again, he requires further proof that what appeared to be gelatinous matter was actually gelatin. And on the supposition that spontaneous existence was brought about by chemical agency, he remarks "The experimentalist could never be considered as the author of the existence of these creatures, except by the most unreasoning ignorance. The utmost that can be claimed for, or imputed to him is, that he arranged the natural conditions under which the true creative energy — that flowing from the

primordial appointment of the Divine Author of all things, — was pleased to work in that instance.” — I see no objection to this opinion, provided there is indubitable proof that what is alledged actually took place, for Geology teaches us that the present formation or structure of the earth, was the work of time slowly carried on during an indefinite succession of ages.

In the course of time, molecular matter was so arranged as to be calculated for the development of sea plants, corals etc., which period is designated as the Transition Era.

At the subsequent period fishes became abundant during the Era of the Old Red Sandstone. A subsequent important Era is that of the Carboniferous formation, when secondary rocks appeared, adapted to the production of land plants.

In the Era of the New Red Sandstone, reptiles make their appearance; and traces of birds are observed. The mammalia are first seen in the Era of the Oolite. Passing over the Era of the Cretaceous formation, we arrive at the Tertiary, when Mammalia become abundant. Thus would it appear that creative power was displayed in the production of the various forms of vegetable and animal life, as the condition of the earth was adapted to their peculiar mode of existence; and creative power may still be exercised when a certain favorable arrangement of circumstances exists, for aught we know to the contrary.

The insect produced in the case before us is said to have been an *Acarus* of a “heretofore unknown species.” The family of the *Acari* has been long known to naturalists. It is spoken of by Aristotle. There may still be varieties of the family not yet discovered, derived from a common source, but modified by unknown circumstances, just as we discover occasionally a variety in a family of plants. So far then there is no evidence of spontaneous existence.

I pass over the experiments made by Mr. Crosse because it does not appear that he adopted any precaution to exclude the atmosphere. It is singular that he should have been so discouraged by the reception of his experiments, as to discontinue them. On the contrary it should have stimulated him to persevere and to prove beyond doubt and cavil, even to the most skeptical, the truth of what he affirmed. The experiments were subsequently pursued by Mr. Weekes, who adopted as we are told “every care that ingenuity could devise to exclude the possibility of a development of the insects from Ova.—” Baking the frame work in a powerful heat, distilling the water, and subjecting the silicate to a white heat, would appear to me to avail nothing, so long as there was an aperture at the top of the bell glass. That glass must necessarily have contained atmospheric air; and we are not told that any attempt was made to free it from air by means of an air pump; but the atmosphere is supposed to have been excluded by the fumes constantly arising from the liquid. The solutions used by Mr. Crosse were silicate of potash and nitrate of copper. Mr. Weekes tried the same, and besides, the ferro-cyanuret of potassium.

I am not aware that fumes would arise from a solution of silicate of potash or from a solution of ferro-cyanuret of potassium, or from nitrate of copper in solution, without the application of heat, and we are not informed that heat was applied.

Admitting however that fumes were so abundant as in the opinion of Mr. Weekes to exclude by their passage through the aperture at the top, all other substances, what had become of the air already confined in the glass? Why should the phenomena of endosmosis and exosmosis be overlooked in this experiment? Animal membranes, whether living or dead, according to the researches of Matteucci, Dutochet, Cima, and other eminent physiologists, are permeated from without and from within at the same time, by even viscid fluids, as solutions of gum arabic, and syrup. The experiments of Professor Riddell, and cases cited from various authors, contained in a treatise written by him in 1836, prove that germs of vitality which elude the power of the human eye and even of the microscope, exist in the atmosphere. Mascati and Boussingault demonstrated that organic matter exists in extremely small quantities in the noxious air that hovers over marshes. From a globular glass filled with ice suspended in the air, over the rice grounds of Tuscany, the dew which had been abundantly deposited on the surface, was collected, and at first appeared to be clear limpid water. There was soon, however, an appearance of little flakes, possessed of properties peculiar to animalized matters, and finally at the end of some days the liquid putrified completely. The Professor further informs us, that in 1834, Boussingault reported some striking experiments tried by him at Cartago in South America. In the middle of a swampy meadow, in every instance, carbonaceous matter was detected in the dew, by the addition of Sulphuric Acid. He remarks: "The results prove very clearly that in marshy places during the precipitation of dew, there is an *organic matter* deposited with it."

Professor Riddell's experiments on the atmosphere in a pest-house in Cincinnati, where cases of small pox abounded, all tend to the same conclusion, and are equally decisive. (see N. O. Med. and Surg. Journal for Jany. 1846. p 466.)

As these germs of vitality are known to exist in the atmosphere invisible to the naked eye, and even the microscope; and we know of no limit to their extreme minuteness; and as it has not been proven that atmospheric air was excluded in the experiments of Messrs. Crosse and Weekes, I am unwilling to admit that vitality was in those instances the result of a "*chemico electric operation*."

Experiments so novel and so startling, have no doubt been repeated over and over again by Chemists in all parts of Europe, yet no such result as that stated by those gentlemen to have been produced ten or twelve years ago, has been announced. Electricity, it is well known is favorable under certain modifications, to both vegetable and animal development. The carbon in the ferro-cyannuret of potassium, was another substance favorable to the development of animal life. I infer that there was a condition of things favorable to the production of the acarus from an invisible living germ which happened to be present.

If the rapid growth of the insect be objected to, I observe that in the space of six or eight hours, living germs invisible to the naked eye, deposited on a slight wound by a peculiar green fly which appeared in Florida in 1828, produced voracious maggots half an inch in length, whose ravages on the unfortunate animal that had become their victim,

were rapid and invariably fatal, unless arrested by some application which destroyed them.

It does not appear that Messrs. Crosse and Weekes, or the Author of the "*Vestiges*" drew any other inference than that a state of things was present, capable of bringing into existence living creatures. And the latter clearly refers to the creative energy of the Great First Cause, the production of the living insect under the operation of natural causes.

Mackintosh, in his "Electrical theory of the Universe," in the chapter entitled "The Circuit of Motion," observes p. 79. "we propose to show that the vital energy is to be ascribed to galvanic action."

After quoting largely from a paper by Dr. Ure, read before the Glasgow Literary Society, Dec. 10th. 1818, and published in the *Journal of Science and the Arts*, in which allusion is made to the well known experiments of Galvani on dead frogs, he refers to Volta, Aldini, Ritter, and Dr. Wilson Philip, to prove what is universally admitted,—that electricity exerts a wonderful influence in the human system; stimulating nervous action, and imparting great energy to animal life.

The experiments of Dr. Ure, on the body of a condemned murderer, who was executed in Glasgow on the 4th of November 1817, are then reviewed at length, and he concludes that they are sufficient to satisfy any reasonable mind, that electrical action is the great moving principle in the animal economy.

Again, "Nor are we compelled to stop here * * * * The recent discoveries of Mr. Crosse, prove that electrical power not only governs the living machine, but actually originates the animal functions." In another place he says, "This proves that it (the insect) is produced from the silex, and not from the acid." And at p. 86, he observes, "in short, every atom, and every aggregate of atoms, whether in the animal, vegetable, or mineral kingdom, or in the wide field of the Universe, is at all times, and in all places, subject to the powers of electricity, the great and universal primary mover of the physical world."

This language is as explicit as it is extraordinary. It distinctly affirms that electricity is *the great first cause*; so that vitality, perception, memory, imagination, reasoning, judgment, volition, and action resulting therefrom, are nothing more than electrical phenomena.

It would be as difficult to demonstrate that electricity is an entity, as to prove that sound, light and heat are entities. The author himself contends that the latter are the effects of motions in certain terms of matter. And the very same reasoning which he employs in relation to sound, light and heat, is equally applicable to electricity. We know nothing of the existence of electricity, except as a phenomenon produced by motion. In the common machine, whether a glass cylinder, or a circular plate of glass be used, it is the friction of the glass with an amalgam, that developes electricity. If we employ the apparatus invented by Galvani and Volta, or any of its modifications as in the electric telegraph, then we see clearly that the peculiar form of electricity there evolved, is the result of chemical action, or friction. Electricity then is the result of a *preceding* motion—so that it cannot be the "primary mover of the physical world."

With respect to the identity of electricity and vitality, all the evidence

so far adduced utterly fails. The effects of electricity on the nervous system during life are well known. Without it, there is no action of the nerves, and a suspension of all the functions of life must follow. But although a paralysis, or painful affection of a limb may be relieved by electricity, and function, partially suspended, be restored, it is certain that when an individual is actually dead—however much electricity may for a short time excite the nervous system, and produce even violent muscular action, it never yet restored a human being to life, and I believe never will. This however it should do, if it were itself the vital principle, or the great first cause, and primary mover of the physical world, as maintained by Mackintosh.

It has often been argued that if electric shocks properly applied to the nerves of dead persons, can produce such extraordinary effects, as in the case of the murderer, cited by Dr. Ure,—electricity might, with additional knowledge, be so applied as actually to restore the dead to life. It is probable that such persons do not comprehend that there is *sensorial* death, which is death proper,—and a subsequent *animal* death. The animal life which appears to linger for a short time after death has actually occurred, is evinced in the contractile power of the muscles; the growth of the beard and the nails, the flow of a current of electric fluid from layers of muscles separated from the animal, and arranged somewhat in the form of a battery, and in the susceptibility of nervous action.—This apparent vitality is but of short duration, and cannot be prolonged beyond the regular or natural time, by any of the known appliances of art. And whether it can, or cannot be, satisfactorily explained, is a matter of small importance. It *may be* that some small portion of a term, or terms of spiritualized matter may linger with the molecular atoms for a short time, so as to produce under certain circumstances, a faint semblance of life. Again, the susceptibility of muscular fibres and the nerves may be temporarily preserved, by the action of animal electricity, which does not immediately leave the body after death. And it may be possible that it is owing to parasites which abound in every part of the human body, not yet dislodged, which possess a distinct, and independent vitality.

I do not find that the eminent professors of the Italian schools, as Cima, Bachetti, Matteucci, and others, who have devoted close attention to the physical phenomena of living bodies, entertain the opinion that vitality is the result of chemical action. Liebig at one time appeared to incline to that notion; but at present he may be classed with Roget, Müller, and in short all the most eminent Chemists and Physiologists of the French, German, and Italian schools.

It may be inferred from their works that the laws of chemistry when controlled by vitality, are no longer the laws of the laboratory. Under the influence of vital laws, there is no end to the various and singular combinations, of what are at present considered to be a few simple elements, as carbon, oxygen, hydrogen, and nitrogen—but when death occurs, and the operation of vital laws is suspended, the ordinary laws of chemistry resume their sway—decomposition soon takes place, and the usual combinations, consequent on the decomposition of animal matter, are formed.

I venture the hypothesis, that vitality is essentially connected with

spiritualized matter—or in other words, that it is implied by it. The union then of that refined matter with the organized molecular atoms produces the phenomena of life, which continue so long as the union lasts, and ceases when the separation by death takes place.*

It is not necessary here to enter into any speculations as to the independent vitality of germs while contained within the body of the female—such may be the case; or they may form for the time being, a portion of the parents' vitality.

If it be asked how will this hypothesis apply, to the lowest species of the human race who are excelled in intelligence by many animals—to animals, including insects, animalcules, and the most obscure forms of animal life—and to plants, and every form and variety of vegetable life.—I carry out the idea by supposing that in every instance there is some term or terms of matter, to the variety of which we can set no limit, adapted to the peculiar organization, and sufficiently refined to produce the vitality required. Thus a certain term, or terms of spiritualized matter endowed with reasoning powers, may produce vitality in man; other term or terms characterised by instinct may lead to the same result in animals, and still inferior terms may be adapted to the organization of plants—modifications in every instance being admitted, according to conditions or circumstances.

There is, I have reason to believe, something new in the foregoing hypothesis. Those who do not believe in a future state of existence, that death is not only the destruction of the body, but the annihilation of the intellectual soul, with its stores of knowledge, with all its cherished memories of the past, and glorious anticipation of the future, will of course reject it as unworthy the consideration of the reasoning powers of man.

But to all who believe that mind will survive the wreck of the body, and continue for ever a distinct entity—and admit this to be a truth—then the hypothesis, so far as man is concerned, must also become a truth logically deduced.

When death takes place, decomposition of the body, supposing it not to be averted by artificial means, must soon ensue; if any thing remains of what was once the human being, it is the mind or soul, of whose existence, separate from the body, we cannot form any conception apart from the idea of vitality. The previous union then of the mind or spiritualized matter, with the body, necessarily produced the phenomena of life or vitality. So far as the hypothesis regards animals and plants, I leave it to all to deduce their own inferences.

In Professor Matteucci's first lecture on the physical phenomena of living bodies, I find the following remark: "Animals and vegetables increase by *intussusception*, minerals by *juxta-position*; or in other words, in the first, growth takes place by internal *juxta-position*; in the latter, by external; and that because organized bodies conceal in their interior the dissolved elements of new formations, while on the contrary, these elements are external to inorganized bodies."

Here is evidently an analogy implied between the growth of animals

* Socrates taught that the soul gives life to the body by its union with it, although he believed that it was totally distinct from every thing material.

and vegetables, and the increase of minerals, which I think is not sustained by facts ; and which has a tendency to make us lose sight of the peculiarity of the vital laws.

It is true that minerals increase externally, as animals and plants grow internally, but here the similarity ends ; in the former case, a mass of aggregated atoms may be increased by the addition of similar atoms, and sometimes of different atoms ; but these undergo no change—silex is still silex ; and so of lime and alumine, and earths and rocks in general. But in the living plant or animal, decomposition is going on, reducing the various elements in the first instance to a homogeneous mass, from which, after it has been absorbed or carried into the sap or blood, and distributed throughout the plant or animal, secretion eliminates the various compound substances necessary for their growth, and the supply of natural waste. All these curious changes occur under the operation of peculiar laws, which cannot be recognized in the study of inorganic matter, either in the laboratory, or in the open field of nature.

Function is not the result of organization but of vitality. When the latter is extinct, the former no longer exists ; but organization may, under certain circumstances, remain for a long time unimpaired. Plants may be preserved without disorganization or decomposition for years. The bodies of human beings frozen to death in high Northern latitudes remain unchanged for years or perhaps centuries. One kind of the Egyptian mummies, in which the body was preserved entire, remained for ages in the great vaults under the Pyramids, with the organization unimpaired. We read also of the bodies of travellers who have perished in the African or Arabian deserts and have been overwhelmed with the drifting sand, which were found long after shrivelled and dry, but without any sign of decomposition or putrefaction.

Organization then may exist independently of function and vitality ; but when vitality acts upon a certain organization, function is the result ; it therefore appears to be a property of organized matter, developed by vitality. However, the peculiar feature of vitality, which so essentially distinguishes organized from inorganic matter, is the *internal power of propagation or generation*, which is possessed by the most obscure and minute, as well as by the largest and most perfect organizations.

Variations in the mode of propagation do not establish fair exceptions to this rule. The viviporous and oviporous modes of production are well known to be the most common. “ Amongst the infusoria, many of the globular monads and vorticellæ increase by spontaneous and equal division. The living globule will at first appear as if encircled by an equatorial band, which will continue to be drawn more and more tight, until a complete separation occurs, each portion being an independent monad, which in turn is bisected like its parent. In this manner a mysterious multiplication goes on indefinitely. The monasuva consists of four or five corpuscles in a cluster, by the spontaneous separation of which, the species is propagated. The volvox globator consists of a spherical membranous sac, filled with liquid, in which float many more diminutive globules like itself. These have precisely the same structure as the enveloping membrane, even to containing within them a series of still minuter spherules. Observers have seen the fifth generation in the same individual animalcule. The gonium pectorale has an

angular flattened body, containing sixteen corpuscles, which subsequently become distinct animalcules, like those in the volvox. (Riddel in Western Journal of Medical Sciences ; March 1834.)

For the further elucidation of this branch of the subject, I shall again refer to the article I published in the N. O. Med. Journal, January 1846.

"The discoveries of Erhrenberg relative to the organization of the rotifera, go far towards placing these diminutive beings on a level, both in structure and function, with the larger animals of whose history and economy we have a more certain knowledge. * * * * * In many of these animalcules, he has seen the ova excluded in the form of extremely minute globules, the twelve thousandth part of an inch in diameter. When these had grown to the size of the seventeen hundredth part of an inch, or seven times their original diameter, they were distinctly seen to excite currents, and to swallow food. The same diligent observer detected the young of the rotifera vulgaris, perfectly formed, moving in the interior of the parent animalcule, and excluded in a living state, thus constituting them viviparous animals, as the former were oviparous. Other species again imitate the hydra, in being what is termed gemmiparous, that is producing gemmules (like the budding of a plant,) which shoot forth from the side of the parent, and are soon provided with cilia, enabling them, when separated, to provide for their own subsistence, although they are of very diminutive size when thus cast off." (Roget's Ani. and Veg. Phy. Vol 2.) It appears then that even among the infusoria, which are the minutest beings known to Naturalists, and can only be seen and examined by the aid of a microscope, some are equal in structure and functions to the larger animals, while others, which deviate from the usual mode of production present no objection to the peculiar law, or characteristic of vitality, to which I have alluded. Before becoming independent beings, they formed part of the parent, from which they derived existence, nourishment and vitality.

If we consider the peculiar phenomena of the animal functions, resulting from vitality ; the contractility of muscular fibre ; the curious connections and sympathies of the nervous system ; in short the whole complicated machinery of the human body ; and the still higher endowments of mind, with its powers of perception, memory, association, reflection, reasoning,—which are so different from the phenomena of mere molecular matter as it is studied in the laboratory or in the mineral kingdom, I cannot conceive how vitality and mind should be considered as results, or properties of molecular matter in any form of combination, or mode of complicated organization. The subject does not, it is true, admit of mathematical demonstration ; but there are other kinds of evidence which may produce an equally strong connection in the human mind ; and such evidence I think is abundant, without adducing the authority of revelation.

Having now briefly explained my views of the nature of mind and of vitality, I shall next attempt a classification of the earth and its inhabitants. The earth consists of molecular matter, which, as I have already observed, may be divided into two classes, 1st, elementary matter and 2nd, the same in a state of combination. In the former, the atoms are supposed to be homogeneous, as in certain gases and in the metals. Chemists, at the present time, enumerate about sixty elementary substan-

ces. It is highly probable however that in the progress of chemical science, the number will be found to be much less, as nature delights in the simplicity of means, how productive soever of results such means may prove. Thus the combination of carbon, hydrogen, oxygen and nitrogen are found to be almost interminable; and we may presume that the metals have a common base, and even the gases. Be this as it may, a mass of elementary matter is held together by cohesive attraction, resulting from a certain arrangement, and the peculiar motions of the atoms of which it is composed.

Compound matter, consisting of two or more elementary substances, of which the atoms are heterogeneous or dissimilar, appears to be held together by a different law, called chemical affinity; yet I believe, as before stated, that it is the same law of attraction, modified in a manner which we do not exactly comprehend, by motions in certain terms of matter; for although all molecular matter constitutes but one term, it is no doubt permeated by other terms invisible to us, which may produce an infinity of results by exciting polaric motions.

The true difference however between compound and elementary matter is this; that dissimilar substances will unite to form a new substance in definit or equivalent proportions. Moreover, one substance though capable of combining with several others, may have a stronger affinity for one, than for all the rest.

In the next class, including all plants, we first meet with the phenomenon of vitality, characterized by a species of instinct; by growth, by functions, and by the capacity of reproduction. "The simplest form of reproduction may be seen in the cryptogamic plants. There is a parent cell, which having arrived at maturity, by the exercise of organic functions, bursts, and liberates its contained granules. These, thrown at once upon their own resources, and entirely dependent for their nutrition on the surrounding elements, develop themselves into new cells, which repeat the life of their original. Amongst the higher tribes of the cryptogamia, the reproductive cell does not burst, but the first cells of the new structure are developed within it and these gradually extend, by a similar process of multiplication, into that primary leaf-like expansion, which is the first formed structure in all plants. Here the little cell becomes directly a plant, the well formed living being." (Vestiges p 128.) Having already alluded to the instincts and functions of plants, I simply add that vitality is the new feature, which distinguishes this class, from the two foregoing classes; the former (functions &c) being dependent on the latter.

Vegetable and animal life are so blended together, that it is difficult in some cases to determine to which department the living entity belongs, as in the case of the sponge, the fungus, or the hydra viridis, which may be considered as connecting links between animal and vegetable life.

And the general similitude in organization, instinct, functions &c., is so striking, as has been already shown, that some hesitation may well exist as to their constituting one class, or forming two distinct classes. Moreover there is a fundamental form of organic being which is considered by some naturalists as the origin of all animal and vegetable life, it is the globule with a new globule forming within itself, by which it is in due time discharged, and which process is carried on in endless suc-

cession. The instinct of animals however is carried to a higher point than that of plants—having, in some instances of the highest organizations, every appearance of reason. Plants with few exceptions are fixtures; while animals, almost without exception, have the power of locomotion. The instincts of plants may be considered as blind; but the instincts of animals are aided by organs of sense such as man possesses. It may therefore be proper and expedient to consider animals as forming a distinct class.

If the vegetable and animal kingdoms are so blended together, that it is difficult to insulate them in distinct classes, we shall experience a similar embarrassment so far as organization is concerned, in separating mankind as a distinct class from the inferior animals; for there is in nature a scale gradually ascending in organization and function from the Infusoria, the lowest order of the Radiata, to the Bimana, (man) the highest order of the Mammalia and Vertebrata.

The same gradation may also be observed in the vegetable kingdom from the simplest lichen to the highest order of dicotyledonous trees.

Between the highest tribes of Simiæ (monkeys), as the Chimpanzee of Western Africa, and the lowest varieties of the human race, such as are found in some parts of Africa, and in the Andaman Islands in the Bay of Bengal, there is much less difference in appearance than what may be observed between races of men and animals. One manifests about as much intelligence and power of language as the other.

Again in Fletcher's rudiments of Physiology, it is stated as a fact, that as the brain of every tribe of animals appears to pass, during its developement, in succession through the types of all those below it, so the brain of man passes through the types of those of every tribe in the Creation.

It represents, accordingly, before the second month of utero-gestation, that of an avertebrated animal; at the second month, that of an osseous fish; at the third, that of a turtle; at the fourth, that of a bird; at the fifth, that of one of the rodentia (dormouse, squirrel etc.); at the sixth, that of one of the ruminantia (oxen, deer etc.); at the seventh that of one of the digitigrada (fox, wolf etc.); at the eight, that of one of the quadrumanu (monkeys etc.); till at length at the ninth, it compasses the brain of Man! It is hardly necessary to say, that all this is only an approximation to the truth; since neither is the brain of all osseous fishes, of all turtles, of all birds, nor of all the species of any of the above order of mammals, by any means precisely the same, nor does the brain of the human foetus at any time precisely resemble perhaps that of any individual whatever among the lower animals. Nevertheless it may be said to represent, at each of the above mentioned periods, the aggregate, as it were, of the brains of each of the tribes stated."

From the lowest of the African or Indian races, to the highest degree of intellect and developement of civilisation, there is a graduated scale, referring to perfection of the physical organization, color etc., extending through the Negro, Malay, American, and Mongolian nations, until it finally reaches the Caucasian. The form of the face, as well as color, is characteristic of these nations. In the Negro, we observe the great predominance of the lower jaw, the thick coarse lips, the flat nose, and the woolly hair. They are generally black and of ungainly forms. The

Mongolians have flattened features, high cheek bones, and great distance between the eyes, and are generally of a yellow color. The American Indian is in form and face intermediate between the Mongolian and Caucasian races, and from his color, is usually called the red man. In the Caucasian, we see the oval face, arched forehead, and the marked features; in short, the perfection of the race up to the present period. — From these diversities existing in the color and physical developement of the human race—many philosophers have supposed that there must have been distinct or separate origins. In a work however published by Dr. Pritchard entitled “*Researches into the Physical History of Man*”, it has I think been clearly demonstrated, that all the external peculiarities that are observed in the varieties of mankind, may be satisfactorily accounted for, on the supposition of a common origin. According to Buffon, unwholesome and badly prepared food tends directly to make the human race degenerate. The following remark of the Author of the “*Vestiges*” is I think fairly sustained. He says “It is fully established that a human family, tribe, or nation is liable, in the course of generations, to be either advanced from a mean form to a higher one, or degraded from a higher to a lower, by the influence of the physical conditions in which it lives. The coarse features and other structural peculiarities of the negro race, only continue while these people live amidst the circumstances usually associated with barbarism. In a more temperate clime and higher social state, the face and figure become greatly refined. The few African nations which possess any civilisation exhibit forms approaching the European; and when the same people in the United States have enjoyed a within door life for several generations, they assimilate to the whites, amongst whom they live. On the other hand, there are authentic instances of a people originally well formed and good looking being brought by imperfect diet and variety of physical hardships to a meaner form. It is remarkable that prominence of the jaws, a recession and diminution of the cranium, and an elongation and attenuation of the limbs, are peculiarities produced by these miserable conditions, for they indicate an unequivocal retrogression towards the type of the lower animals.” p. 164. And at page 195, the same Author states: “About two hundred years ago, a number of people were driven by a barbarous policy from the counties of Antrim and Down in Ireland, towards the sea-coast, where they have ever since been settled, but in unusually miserable circumstances, even for Ireland; and the consequence is, that they exhibit peculiar features of the most repulsive kind, projecting jaws with large open mouth, depressed noses, high cheek bones, and bow legs, together with an extremely diminutive stature.”

Thousands of persons in the Southern States, can testify to the correctness of the foregoing remarks as regards the African negro; and from the change which may be observed in the course of an ordinary life, it might easily be inferred what would ensue after the lapse of ages.

In a work published by the Marquis of Beccaria on “*Crimes and Punishments*,” and in Sismondi’s “*History of the Italian republics*” instances are given of the wonderful but disastrous change which may occur in men of the finest physical organization, and highly cultivated intellect, by long continued imprisonment in solitary confinement, say from fifteen to twenty years. They speak of State prisoners liberated by

some popular tumult or political revolution, whom even their own relations and friends could not recognise, so completely had their form and features degenerated and changed—who had lost the knowledge of language, and the power of articulation—and who more resembled drivelling idiots, than highly civilised and refined Italian nobles.

It is also well known that families who intermarry for a length of time, refusing any admixture of other blood with their own, through a false notion of aristocratic pride, degenerate both physically and intellectually. The Cretins, a people inhabiting a valley between Switzerland and Savoy, where, surrounded by mountains, they are shut out from the rest of the world, and have been intermarrying for centuries, now present the anomaly of a race or tribe of idiots. And these facts are sustained by analogies of the animal and vegetable kingdoms. Whence the superiority of our blooded horses, and other kinds of stock? It arises invariably from crossing the breed; from the great care and attention devoted to them, and from the quality of the food with which they are nourished. The finest horses in the United States have been derived from England, where the race has been improved century after century, by frequent crosses of the Arab and Norman breeds.—And so it is with horned cattle, and other kinds of domestic animals.

Every farmer knows, or ought to know, how fruit trees, and flowers, and grapes, and vegetables have been, and may be improved by judicious cultivation.—But cultivation alone will avail but little; the *seed* must be renewed, and the oftener the better. Whatever may be the nature of the disease in the cotton plant, called the rot, it is occasioned by continually planting the same seed. From observation I have reason to believe that if planters would send every year fifty or a hundred miles for fresh cotton seed, to plant their crops, they would never again see the rot in their fields. Plants and animals, as well as men, are affected by climate, elevation, and other physical agencies. In short no changes in the human race have been greater, than those, of which we know both plants and animals to be susceptible.

The common origin of mankind is also inferred from the identity which may be traced in languages; thus it has been calculated mathematically that if three words coincide in two different languages, it is ten to one they must be derived from some parent language. Six words give more than seventeen hundred to one; and eight near one hundred thousand, making the evidences in such cases but little short of absolute certainty (Dr. Young).

Humboldt makes out one hundred and seventy in common between the languages of the old and new continents;—satisfactory proof of the original connection of the American and other families.

In the learned and elaborate works of Gebellin, published in France some time during the last century, this identity of languages is presented on a very large scale, and so far as I am capable of judging, a fair and successful attempt is made to trace them all to one common primitive language.

If it has not been difficult to account for the physical changes already spoken of, which the human race have undergone, neither will difficulty be experienced in attempting to account for the diversity of color.

Heat has generally been considered as the agent which produces the

varieties of color amongst the nations of the earth; but there is far greater reason to suppose that light is the principal agent. This opinion is expressed in the "Vestiges". "The phenomenon, (color) in short, appears identical in character with the photographic process; not a result of the action of heat, as has been so long blunderingly supposed, but of light."

The chemical effects of light were noticed by the Alchymists. In 1556, the effect of the sun's rays in blackening a mixture of Chlorine and Silver, was observed. Subsequently Scheele, Petit and Priestly made some interesting discoveries; and since the introduction of the Daguerrotype and other photographic processes, the chemical agency of light has become a branch of science, for which the name of Actino-Chemistry has been proposed by Sir John Hershell.

A sunbeam passing over a sheet of iron will leave indications of its path. The chemical compound of gold, silver, copper, platinum, lead, nickel, are all under solar influence and liable to change.—Hydrogen and Chlorine may be kept together in the dark without uniting; but if exposed to sunshine, chemical union immediately ensues, and Muriatic acid is produced. Chloride of silver, which is white when precipitated from a solution in Nitric Acid, turns in a few minutes to a deep violet color, on exposure to the rays of the sun. Many similar facts might be noticed; indeed they are now so abundant, that perhaps there is nothing on the surface of the earth which is not liable to change under solar influence.

An interesting effect of light on electricity was stated by Mr. Hunt about four or five years ago at a meeting of the British Association. "A small galvanic arrangement was formed by putting a solution of the iodide of potash in a glass tube, one end of which was closed by a piece of skin; this tube was inserted in a solution of nitrate of silver, held in a cylindrical glass, the two fluids being connected with a piece of platina wire. Such an arrangement being kept in the dark for twelve hours, a beautiful crystallization of bright metallic silver takes place about the end of the wire which dips in the silver solution. If a similar arrangement is exposed to sunshine, no such change,—no symptom of crystallization takes place."

What would the vegetable kingdom be without light? Seed will germinate in the earth in darkness under the influences of heat and moisture; but the plant will droop and die without light. It is almost painful to witness the efforts, prompted by instinct, which plants, confined in a dark room or cellar, will make to approach a feeble ray of light struggling through a narrow aperture. All the beauty of coloring in leaves and flowers depends on light; for the richest verdure and most gorgeous flowers are ever to be seen in the inter-tropical regions, where a vertical sun pours down a flood of light. It is a curious fact, that the yellow rays which are the most luminous, will prevent germination; while the blue rays are very favorable to that process. It is well known that plants absorb atmospheric air, which is always more or less contaminated with carbonic acid, produced by the respiration of men and animals, and poured out abundantly by all burning bodies. During the sunshine, this carbonic acid is decomposed by the plant—the oxygen is given back to the air, while the carbon remains in the plant for the formation of the woody fibres. This action nearly ceases at night.

Whence it is easy to see how vastly important must be the influence of the sun's rays in the vegetable kingdom. The importance of the influence of the sun's light on all things of the surface of this earth can scarcely be estimated.—One thing is certain; without it, all animated nature would first droop and fade, and ere long perish, and become mingled with, and lost in, the general mass of molecular matter. All coloring then in the vegetable kingdom depends upon the action of the sun's ray's, by which also all modifications of color are brought about.

Passing from the vegetable to the animal creation, we find that the same remarks hold good. "Where the sunbeam sheds its genial influence, there life in all its myriad forms is found; where the sun's rays cannot penetrate, death holds his silent court. Without the actinic power, the entire surface would be an eternal blank, a chaos as at first."

The richest coloring of birds and animals, as is the case with plants, is seen in equatorial regions. The fair color of men who have removed from northern latitudes to reside in warm climates, will in course of time assume a much darker hue, where the surface is exposed to the action of light, while those parts of the body covered with clothes, where light is excluded, *but heat increased*, will retain their original fair color. Females who seldom go out until after the sun has set, and who are consequently never exposed to the intense light of the mid-day sun, experience no change in the color of the skin. The face, hands and neck are as fair as other parts of the body.

I have seen negro children born of the same parents, under the same circumstances, presenting no difference of color for years; but one having been sent to work on the plantation, and another taken into the house as a servant, a remarkable change of color was perceptible in a few years. The one working in the field became several shades darker, and the other, the house servant, several shades lighter.

The same fact has been often observed by southern planters, and is substantially affirmed by Dr. Pritchard. "We may further remark, that physical qualities of particular races of Africans are evidently related to their moral and social condition, and to the degree of barbarism or civilization, under which they exist." According to the same author the light color of the hair, eyes and skin of the inhabitants of the extreme north of Europe, gives way gradually to darker shades as we approach the northern tropic, until at the equator, black is the predominant color; and south of the tropic of Capricorn, color again becomes gradually lighter; this law being modified by elevation of land, and the degree of civilization. The limits of this article will not permit me to dwell on this branch of my subject. Those who desire full information, may obtain it by consulting the great work of Dr. Pritchard, of which I have already spoken.

The Theory of development as maintained in the "Vestiges" includes man, so far as his physical organization is considered, but I think, utterly fails, when applied to mind. Various parts of the old world have been assigned as the birth place and residence of the first of the human family. The Hindoo tradition refers it to Thibet; and another, locates the first of the race in the island of Ceylon.

"The development hypothesis would demand of course, that the original seat of the human race should be in a region where the qua-

drumana are rife. Now these are most abundant, both in species and individuals, in the Indian Archipelago." (*Vestiges* p 207). Fact is here opposed to theory, as from the investigations of Professor Owen the nearest approach to man, of any of the quadrumana, is to be seen in Western Africa.

In the progress of civilization and in the developement of the human mind, we see little of that regularity which is remarkable in the inferior departments of nature. Late discoveries in Egypt, particularly of a key which unlocks the Archæological mysteries of remote ages, would lead to the conclusion that there was a considerable degree of civilization in that country several thousand years before the commencement of the Jewish History. Records to establish the same fact in the Eastern part of Asia, are said to exist in the large manuscript library of the great Central temple of Thibet; they extend back fifteen thousand years.

According to the late Professor Rafflesqués, criticism and version of the Ancient Hebrew or Obri language, there is nothing in the foregoing remarks at variance with the Mosaic account of mankind from the Creation to the Flood.

Not only have individual nations who had attained to a high degree of civilization relapsed into barbarism, but "Twice within the records of history has the human race traversed the great circle of its entire destiny, and twice has the rudeness of barbarism been followed by a higher degree of refinement." (*Vest.*) p 224

Such facts are directly at variance with the theory of developement as applied to the human mind.

I have spoken of the instincts of two classes, animals and plants; man also has his instincts. The desire to preserve life, and to enjoy pleasure and avoid pain is an instinct, which man possesses in common with animals; and before an infant is directed by reason to take food, instinct directs it to the mother's breast, but reason is the great distinguishing feature of man, who forms the highest and last class on the scale of creation in this world.

Wherein then does reason differ from instinct?

Precisely as the existence of different terms of matter is inferred from the velocity of impulses, as in the case of sound compared with light; and these again compared with the causes of gravitation; so is it inferred that reason is the result of a term or terms of matter far more refined than that which is the cause of instinct, on account of the very different phenomena which they present.

In strong, well cultivated and regulated minds, reason predominates so as to make us lose sight of instinct. But in the idiot, where reason is so feeble as to be scarcely susceptible of the least developement, instinct predominates. Again, in the madman, where, owing to a deranged state of the brain, the strongest mind is incapable of exercising its powers in a reasonable manner, instinct or cunning is always great and often extraordinary.

The laws of repulsion and attraction; of crystallization; of mechanics, and vegetation, are fixed and determinate, and might with propriety be called the *truths* of repulsion &c. And notwithstanding certain objections which probably can be satisfactorily explained, instinct in animals, and in plants if such actually exist, may be considered as fixed and

unchangeable as any law of nature. It leads to uniform action always inducing them with unerring precision to adopt the necessary measures for self preservation. We know of no change in the habits of animals since facts relating to them were first observed, and their natural history was described.

If animals reason, they certainly manifest a great deal more wisdom and prudence than man ; and all the examples that have been, or that might be cited to establish the fact, *prove too much*.

Admitting for argument sake that animals reason, from a consideration of the admirable skill and intelligence which they manifest, where do we see the results of reason ? It must have existed from time immemorial ; yet no change has taken place, no improvement has been made, no progress in art or skill has occurred. And is this the character of reason ? Is it not rather the character of a fixed and absolute law ? Therefore I conclude that whatever we observe in animals, which implies reason, must be referred to such a law, which law is instinct.

If the difference between instinct and reason were only in degree, then would undoubtedly be seen a regular scale of intelligence corresponding with the physical organization. But this is not the case ; the creatures that are lowest on the scale of animal existence, as those obscure connecting links between animal and vegetable life, manifest as much intelligence as the highest, in providing for their wants, and doing what is necessary for their preservation.

If the fact be remarkable that animals have ever remained stationary in point of intelligence, as they were at first brought into existence by Creative Energy, notwithstanding the great apparent intelligence which they display—the progression of man is equally remarkable. We have opportunities of tracing his progress step by step from a state of barbarism to a condition of the highest civilization, in the enjoyment of the refinement, and comforts, and luxuries, and happiness, that intelligence moral and intellectual, and arts and sciences can produce in civilized life. The cultivation of the human mind has already been the cause of a progress in arts and sciences, as much surpassing any of the results of animal instinct, as the rapidity of the impulses of light transcend those of sound. And after the wonderful discoveries of the last quarter of a century, which have enabled man to press into his service the sun beam and the electric spark in the Daguerrotype and the Telegraph, who would venture to assign a limit to the powers of the human mind ? Man learns slowly, and his reasoning powers are gradually developed. It is not so with animals as we have already seen ; their instinct is matured from the first, and experiences no change.

Man can communicate acquired knowledge to his fellow men, and in this way knowledge is perpetuated and ever increasing. But each animal obeys the impulses of instinct, without learning from another animal, and should one be taught by harsh and compulsory measures to do some extraordinary thing, the animal so taught can never communicate the ability to another. Thousands of dogs might pass over the bridge in Paris, but they never of themselves would learn to perform the ingenious trick of the dog who procured work for his master.

I have alluded to objections to the position that instinct is as uniform and unchangeable in its results as any law of nature. The only one

of any consequence is, that certain animals can be taught to do some very curious things, to which their instinct does not naturally or directly impel them, and which seem to imply no small degree of intelligence. But this is easily explained, for it can be demonstrated that in all such instances, they only obey the impulses of instinct, of which the object is, first, self preservation from danger, pain, and premature destruction ; and secondly, to procure their necessary supply of food. Acting under the influence of these impulses, instinct may sometimes seem to depart from its fixed rules of action, to accommodate itself to the peculiar circumstances in which it may be placed by man ; yet if it departs from fixed rules of action, we shall find it to be ever governed by the same principle, or motives. For instance, in the case where dogs or other animals are trained to do some wonderful thing, great cruelty is necessary to force them to learn what man may be desirous to teach. It is sometimes necessary to keep the animal without food until it is almost dead ; and always, to beat it severely. Instinct therefore at last brings it to gratify its master by a compliance with his wishes, which it does to avoid pain, hunger, and perhaps death. Therefore in thus obeying the impulses of instinct, animals act plainly on the principle of self preservation.

Instinct appears to mislead sometimes ; for instance, there is a plant called the *Stapelia Hirsuta*, the flowers of which smell like meat in a state of putrefaction ; flies, deceived by this smell, deposit their eggs in the flowers, and want of food must necessarily destroy the living product. There are some other instances of the same kind, which we do not well comprehend ; such however can scarcely be considered as exceptions to the general rule.

If instinct then is certain in its operations, reason is the very reverse. I speak of things as they are, and not as they might, or should be. Nothing is so uncertain and varying as the conduct of man. Of all beings he is the most inconsistent, so far as practice is at variance with profession or interest. Animals are steadily employed in providing themselves with food, and they neglect nothing in their power to preserve themselves from want, suffering and premature death.

How often, on the other hand, do we see men endowed with reason and education, who, having health, reputation, the support of themselves and families, and even life itself at stake—act contrary to every dictate of common sense and every lesson of experience.

Again, are we not conscious that we sometimes act differently under the influence of the same motives ; at one time resisting, and at another time, yielding to temptation ?

Can any thing like such uncertainty and inconsistency be found in the animal, vegetable, or mineral kingdoms ? We know that the laws of nature as exhibited in those several departments, are remarkable for regularity and certainty. It is evident then that man is not governed by any such law ; that he has a choice of action, that he is a reasonable being ; a free agent, and morally responsible for his actions. An idiot, or a madman, is not amenable to penal law, because he is presumed not to enjoy the benefit of reason ; on which account he is considered as a free agent, or held morally responsible to laws which he may have violated.

I have already spoken of the extraordinary rapidity of thought, far exceeding the velocity of impulses in any known term of matter, and of the wonderful powers of the human mind, when properly cultivated, and faithfully exercised; I have also alluded to the remarkable uncertainty and inconsistency which, nevertheless, characterises the actions of man; and the position of man as a free agent, and a morally accountable being; and therefore conclude that the difference of the phenomena in man and animals, is abundantly sufficient to justify the supposition that reason and instinct are not similar entities, or results of the same term or terms of refined matter.

For if reason and instinct differ only in degree, according to the greater or less perfection of organization, as many affirm, then animals and plants should be comparatively free agents and morally responsible. But no one will contend for, or even admit, such a conclusion. Man, it is true, considered physically, is closely allied to the animal creation, constituting the order of Bimana, in the class of Mammalia, and division Vertebrata. But when we compare *his* reason with *their* instinct, the distance between them appears to be immeasurably great.

Nearly all the ancient philosophers, including Plato, Socrates and Aristotle, maintained substantially the opinions which I have advanced in relation to vitality, the human soul and the difference between instinct and reason. They appear, however, to have had no distinct idea of the mode of the soul's existence. Plato labored to prove the connection of the human soul with the original fountain of light and perfection. Socrates considered the human soul as wholly distinct from every thing material; but that it was of divine origin and connected with the Deity by reason and the power of thought.

Pythagoras is often cited as an exception to this general opinion, on account of the doctrine of Metempsychosis, or transmigration of souls which according to his notion, might successively inhabit the bodies of various animals; a doctrine however which he borrowed from Egypt.

But by devoting a little more attention to his system of philosophy, we find that in his opinion the object of this process was to purify the immortal soul—which important end accomplished, it returned to the source whence it had proceeded.

Epicurus was the principal teacher of an opposite doctrine. He taught that there was no difference between men and brutes; and, considered both to be of material origin, and alike perishable.

The opinions of these ancient philosophers are however of but little importance as authority on such subjects. They boldly proclaimed their doctrines or opinions to their admiring pupils, unsustained by either arguments or facts.

Finally there are those who consider the authority of revelation as paramount to all other; and to whose minds such declarations as the following. "God made man in His own image, and after his own likeness"—and "breathed into his nostrils the breath of life"—carry stronger conviction than the most elaborate philosophical argument.

II. *Researches, critical and experimental, upon the Capillary Circulation.*

By BENNET DOWLER, M.D., of New Orleans, corresponding Member of the Academy of Natural sciences of Philadelphia, etc.

The dynamics of the capillary system has advanced but little since that memorable era, now two hundred and thirty years since, when Harvey discovered the circulation of the blood—an event that shed upon his native land an imperishable lustre, before which swords and helmets, coronets and crowns must “pale their ineffectual fires.” This discovery, in a great degree created physiology or at least raised it from chaos to symmetry. It diffused a steadily increasing brightness, over a vast field where the darkness of countless ages had been broken, only by occasional glimmers of light, which served, to bewilder, rather than to direct the inquirer.

Although this discovery was very complete as to the mechanism of the heart and its action upon the arterial system, it did not, as Harvey supposed, account in a satisfactory manner, for the forces which circulate the blood in the capillaries, nor in the portal system, nor in the general venous system. He attributed all these motions to the heart alone,—a theory not at all tenable as will be seen hereafter.

The capillary force is a subject which has been long and ardently investigated in its two-fold character as a physical and a vital problem. The physical history, alone, which deserves a distinct treatise, would serve as the prelude, or introduction to the vital desiderata; hence a few remarks on the physical principles by which many attempt to explain the circulation, will be necessary. At the present time, one section of the physiological world maintains that the circulation is explicable upon mechanical principles, solely—a second, which is, perhaps, identical with the first, solves the problem by chemical affinity, nutritive action, the endosmotic forces of septa, suction from a vacuum in inspiration, sap-circulation, oxydation, and so on; another appeals to a nervous fluid, and ganglionic action, while, a fourth, relies upon a vital force. The separate, combined, and modifying results of these forces—their points of identity, analogy, contact, divergency, antagonism, and absolute difference, present a very complex question of the utmost importance, and of an almost limitless expansion, justifying a slight historical retrospection, prelusive to the hurried examination proposed on the present occasion.

The Italian philosophers, nearly two centuries ago, took the lead in research and discovery in the phenomenology of motion, comprehending the great principles of equilibrium, statics, dynamics, and hydrodynamics. The Italian, and, subsequently the French governments, expended the public treasure liberally in attempts to ascertain and fix the laws of motion, particularly the movements of fluids, constituting the science of hydrodynamics. Curious readers, who will examine the two hundred large quartos containing the transactions of the Academy of sciences at Paris, for nearly two centuries, will find many, if not satisfactory papers on these topics.

The following remarks by the learned and candid Mr. Whewell, will doubtlessly prove as acceptable to the reader, as they are pertinent to the general physical aspect of this subject, and the more so, as physical,

or at least chemical force, is, at the present time, trenching upon animal dynamics. "The mode of solving the problems relating to the motion of fluids, has been, to introduce certain other hypotheses, often acknowledged to be false, and almost always in some measure arbitrary, to assist in determining and obtaining the solution."—"In most cases, the solutions of problems of hydrodynamics are not satisfactorily confirmed by the results of observation."—"The assumptions of the mathematician here do not represent the conditions of nature; the rules of theory, therefore, are not a good standard to which we may refer the aberrations of particular cases; and the laws which we obtain from experiment are imperfectly illustrated by *a priori* calculation. The case of this department of knowledge, hydrodynamics, is very peculiar; we have reached the highest point of the science,—the laws of extreme simplicity and generality from which the phenomena flow; we cannot doubt that the ultimate principles which we have obtained are true ones, and those which really apply to the facts; and yet we are far from being able to apply the principles to explain or find out the facts. In order to do this, we want, in addition to what we have, true and useful principles, intermediate between the highest and the lowest;—between the extreme and almost barren generality of the laws of motion, and the endless varieties and inextricable complexity of fluid motions in special cases. The reason of this peculiarity in the science of hydrodynamics appears to be, that its general principles were not discovered with reference to the science itself, but by extension from the sister science of the mechanics of solids: If we had lived in a world in which there were no solid bodies, we should probably not yet have discovered the laws of motion; if we had lived in a world in which there were no fluids, we should have no idea how insufficient a complete possession of the laws of motion may be, to give us a true knowledge of particular results."

"The same formula expresses the general condition of statics and that of the dynamics. The tendency to generalisation which is thus introduced by analysis, makes mathematicians unwilling to acknowledge a plurality of mechanical principles; and in the most recent analytical treatises on the subject, all the doctrines are deduced from the single law of inertia." *

Stevinus, in 1586, developed the principles of equilibrium and thereby founded the whole science of statics. This seemed to have served as the point of departure for the science of motion, for about the same period Benedetti published his *Speculationum Liber*, maintaining that the "motion of a body, separately from the mover, arises by a certain natural impression from the impetuosity (*ex impetuositate*) received from the mover, and that in natural motions this impetuosity continually increases by the continued action of the cause,—namely, the propension of going to the place assigned to it by nature, and that thus the velocity increases as the body moves from the beginning of its path." †

This "natural impression", "this propension of going to the place

* Hist. Ind. Sci. i. 114 to 121.

† Mr. Whewell.

assigned to it by nature", is, it may be said, nothing but an indirect confession of ignorance. It is too true; yet, it is the best that can be given in explanation of gravity, chemical affinity, the magnetic force and its cycles of increment and decrement. The heart's force, that of the capillaries, that of the portal, that of the pulmonary, that of the lymphatic, and that of the lacteal circulations, all belong to the same category.

It is probable that all Forces, physical and vital, are imponderable substances, essences, things in themselves, and not simple attributes, mere conditions. Electricity has been regarded by the experimental school as a nonentity in itself, or as a mere quality of matter; not a substratum; not a ponderable. But, it will be seen, according to the experiments recently reported in the London Lancet, that electricity is matter—is ponderable—overflows, has gravity, and pours from a jar like water.

Trancendentally speaking, few things would seem more *real* than Force, as manifested and proved by motion, and as selfrevealed in its many laws. By transcendentalism, I mean, in the first place, a class of hypotheses and probabilities which might be true, but which have not yet been proved by the experimental method, some of which, perhaps, never will; many of which, nevertheless, will be, as they now are, leading principles of action with the learned and unlearned, much as they may deny the imputation: I mean, furthermore, propositions which are not only probable, but absolutely true, and, which cannot be proved experimentally, in all probability, which lie above, or beyond the reach of experiment, and which, in some instances, experiments and the testimony of the senses, seem to contradict. For example, the infinite divisibility of matter is not an experimental, but a transcendental truth. The infinity of space is in the same category.

If every material and immaterial entity were annihilated, space itself would remain, the negation of realism, the antithesis, nay, the very antinomy of experimental phenomenology. Here, one of the most absolute and necessary of all truths, stands in contrast to experimentalism; her longest sounding line reaches not the bottom; the Rosse telescope reaches not the *ultima thule*; the telegraphic wires stretched from planet to planet would not reach the frontiers of infinite space. Eternal duration is a truth transcendental and self evident, but strictly construed, it appears to be, really, another antinomy or contradiction to the experimental, that is, to time. Time is not even a part of eternal duration. Infinity cannot be composed of parts. Again, it is not possible to establish experimentally, the fundamental law of modern dynamics, namely, that motion is perpetual, uniform, and rectilinear, nor this truth—two straight lines which cut each other, never again can meet. Experiment may have suggested this truth which it cannot prove. Though a logician may, perhaps, show that something of this kind lies at the foundation of all generalizations which transcend experience. The transcendental doctrines in medical science, are too numerous for enumeration in this place.

So real and positive did forces appear to the minds of many great philosophers, as Descartes, Newton, Berkeley, and others, that they attri-

buted all motion to the immediate power of the Deity, and not to mere secondary qualities of inert matter as color, hardness, &c.

In the absence of proof showing that the animal forces are strictly chemical, it is as convenient as it is, necessary, to classify them separately, and to call them by another name, that is, vital forces. To suppose that there is a capillary force, the intrinsic nature of which tends to accomplish the circulations of the economy, as gravitation tends to the centre, and which is as necessary to the capillary system, as essentially inherent to it, as atoms, weight, and extension are to matter, may be called a hypothesis. So it is. The Experimental, be it remembered, is indebted to the hypothetical method for almost every advance in science. The former has, often nothing to do, but to test or verify the anticipations of the latter. A hypothesis, which, in the absence of all direct experimental proof, proceeds upon known analogies, so as to account for, or harmonize, a class of phenomena, with any degree of probability, whatsoever, is, so far, an absolute advantage. It is very different, however, when incoherent hypotheses, and scholastic abstractions, claim to be absolute truths and all-explaining theories, and, consequently, become barriers to experimentation, especially, when sanctioned by great names.

The phenomenal history of vital dynamism, presents a very strong contrast to the physico-chemical forces. What can be more unlike than a burning lamp, and the portal circulation, or than the muscular and gravitative forces? The one is intermittent, exhaustible, renewable, variable; the other constant, at the same distance, diminishing in one direction, increasing in another, and always rectilinear. Is the willow that waves in the wind a type of muscular motion? Does the sap-circulation of a tree resemble the portal circulation of man? Is it identical?

The position of the vitalist, in this relation, is by no means the worst, even if vitalism be construed in its worst sense, namely, as provisional admission of ignorance. For, the erroneous explanations, confidently advanced and received as physical axioms, are so much dead weight against the movements of true science.

The vitalist cannot display the muscular, or the capillary force in an insulated concrete form, any more than the physicist can gravitation, how real soever the former may be,—for, it is only known by its effects, is to be studied through its laws, and is to be traced in its essential conditions as conjoined with living masses.

The transcendental doctrine that the capillary force inheres in the blood, or in the capillaries themselves, as an independent one, or as a mere property, or condition of life, will be countenanced, strongly, it is believed, by the experimental part of this paper, while in itself, it is more or less plausible; but, however true, this hypothesis may be, it cannot explain everything in medical science, as most theories pretend to do. Stahl, proclaimed the *Anima*, as the All of life, the body being inert matter, while the *Anima* is an all-controlling, but an immaterial principle acting intelligibly without intelligence, rationally without reason, contriving by instinct. This, instead of being received as a hypothesis, was bowed to as a true system.

Teleology, (a method which Cuvier followed with much success), though often very uncertain, is, nevertheless, a fruitful principle in physiological research. It is always a source of pleasure to be able to discern

the final purposes or ends of nature displayed in the adaptations of organic structure. But this principle is not, always, very obvious in the capillary system, and if the doctrine that the heart's force be received as the motive power in the capillaries, teleology, will afford but little countenance, to this assumption. Hence, a slight examination of the structures concerned is as unavoidable, as I fear it will be unsatisfactory. Teleologically speaking, if the circulation be due to mechanical forces or physical principles, adaptations to such ends, might be expected in the structures concerned. A glance in this direction, is, therefore, indispensable.

As the capillary system belongs to Microscopic Anatomy, it will be necessary to dwell a few moments upon this aspect of the subject—a necessity, that I regret, not because my observations have been few, but for a different reason, namely, an extended monograph would be required to place my views on this subject in such a light, as would enable me to indulge a reasonable expectation of escaping the charge of presumption, in dissenting from the highest authorities, not, indeed, so much with respect to the exact structure of the capillary vessels, (which is little known) but, in respect to other elementary points still more uncertain. For example, the globules of the blood, so often minutely described, pictured, classified (as belonging to different animals) nay, absolutely anatomized, are probably, to a great extent illusory. Air, or rather carbonic acid gas, so abundant in the blood, with no other anatomical membrane than that which envelopes a soap or champagne bubble, would appear like globules,—or these supposed blood globules, may be heterogenous liquids, having different densities, different affinities for each other, for the serum of the blood, and for the surrounding tissues. Hence, upon the physical principle of homogenous attraction, the albumen, the fibrin, or the oily matters in the blood, would, like oil in water, assume the globular form. Besides, the microscopic analysis in the direct light of the sun, gives to blood and certain vascular tissues, a wholly new, different, and unvaried appearance. Of this method, I have much to say, but not on this occasion. I wish now, to be understood as referring to the usual degree of light, though unfortunately this important point remains without any standard of comparison, except the negative one, requiring the light of a lamp, or the indirect light of the sun. Passing by this source of illusion, the reader will be kind enough to read the following extracts, taken almost at random from a few books, lying on my table,—more would be tedious.

Prof. H. Milne Edwards, represents the globule opened, dissected, and the flaps turned back! (*Anat. and Phys.* 25.) A microscopic object, would, of course, require a microscopic dissector, and microscopic scalpels! Dr. Golding Bird, in his account of Prof. Mulder's chemical physiology of the red coloring matter of the blood, writes with apparent good faith, many passages like the following:

"The coloring matter is normally enclosed in a sac or cell composed of a thin membrane, consisting chiefly of protein, and capable of admitting of percolation through its walls under certain circumstances, although it generally retains the colored fluid it encloses sufficiently firmly to prevent its admixture with the serum. Within the sac is a

nucleus of similar chemical composition to the membrane, and the whole thus organized constitutes the *blood-disk*, *globule*, or *red particle*: The bright red corpuscles are always *biconcave*: in the dark-red blood, the corpuscles are *convex*, and the enveloping membrane is *much thinner*; they readily burst, produce exosmosis, emptying the sack, &c. &c.: On reaching the capillaries, the coating of oxyprotein is removed, the protein being employed for the repair of tissue, and the oxygen used for effecting the metamorphosis of effete and exhausted structures. The corpuscles losing their opaque covering, have their power of reflecting light diminished, their concave surfaces are lost, and the whole assumes a venous tint." (N. Y. Jour. Med. and Lond. Med. Gaz. 1845.)

Mr. G. Newport, in a paper read before the Royal Society, undertakes to define the corpuscles of insects, and other invertebrata, comparing them with those of man, and the vertebrata: He says "in the articu-
lata the forms are first, the *molecules*; secondly, the *nucleated* or *oat-shaped corpuscle*; thirdly, the *spherules*, or minute rounded bodies developed from the oat-shaped corpuscle, analogous to the free nucleoli of Valentin; and lastly, the *discs*, which are further developments of the spherules, and analogous to the true red-blood-discs of the higher animals." (Bullet. Med. Sci. 1845.)

"No single description (of the red globules of the blood) has tallied with that which went before. Leeuwenhoeck believed that he saw them consisting each of *six well compacted smaller globules*. Hewson believed that they were *bladders* which had within them *some* central body, loose and moveable; that often the central part might be seen *rolling* in its bag, &c. The Abbé Torre, said that these were not globules, but *rings*." (Bell's Anat. and Phys.)

Dr. Carpenter calls the *globules*, "*flattened discs* having a circular form." (Phys. §570.)

Sir Charles Bell relies on the microscopic examination of the frog's foot, for the proof of the capillary vessels. He says, naively enough: "We do not discover the coats of the vessels, but conclude that they exist, from the confined and certain course of the particles (of blood) which are in motion." (Anat. ii. 26).

Micography envisages much which seems

"For man's illusion given."

Thus a convex or a concave surface may under certain circumstances appear globular. The different elements of the blood, as air, water, fibrin, fatty matter, albumen, not to mention their probable form, possessing as they do, different degrees of transparency, different densities, different degrees of shading power, of irisation, and more than all, different degrees of refracting force, would all conspire to throw doubt upon the anatomical accuracy of these microscopic membranes, and central nuclei, and the configurations which are too confidently proclaimed as absolutely characterizing the globules. Still more. Physical capillarity itself, would be illusory, that is to say, a portion of liquid, or several liquids in a capillary tube, would assume a concave or convex form, according to their wetting or non-wetting properties. These and some similar facts were forced on my attention, in making dried preparations between narrow strips or plates of glass; pressure and desiccation for a whole season, did not deprive many preparations of these globules of

air, water, &c. Heat, instead of expelling them, often augmented their number, by expansion, at the same time giving fixity to their enveloping tissues.

I might adduce still further argument from dead or physical chemistry, favorable to the doctrine here advanced, as to the globules of the blood being in all probability nothing more than one or several of the constituents of the blood taking the spherical form, as may be observed, out of the living body, without an organization high and complicated. I have observed with the naked eye, in the recently dead human subject, a rapid circulation or flow of chyle in the blood; the former had not yet fully mixed or combined with the latter, as will be seen in the sequel. Hence, this element alone, might be expected to assume the form of globules, without admitting anatomical sacks, nuclei, corpuscles, &c. The very pictures of these condemn them, on the physical principles of light. The centre is said and is known to be colorless, nay, luminous, if the globule be organized with red envelopes, the rays would be absorbed to a great extent, while upon the supposition that it is simply, like a drop of water, or oil, or like a double convex lens, it would converge the light, so as to give a central luminosity corresponding with the pictorial representations. This piece of anatomy, this complete and sudden organization of the blood globule, considering the rapidity with which it is destined to pass through the minutest tubes, is, in a teleological point of view, very improbable, and, must be, upon the filtering, wetting, or endosmotic theory of the circulation, somewhat unfavorable to permeation.

I do not deny that there are globules in the blood, but I deny that they are peculiar to it, since they appear to exist equally in every animal and vegetable substances which I have examined. But of all objects, they are the least likely to appear truly represented in themi croscope either as to figure, color, or anatomical organization.

Color is an unimportant, and unexceedingly variable, not to say a deceptive element, in microscopic research. A very thin stratum of blood absorbs little, transmits much, and reflects almost no light in the direct solar ray. The so called red globules, may be absolutely colorless, though coloring matter near them, or in the axial line of vision, gives them a red hue, in the same way that a red curtain, in a room with a mild light, colors microscopic objects red? Are the peaks of an elevated mountain chain, really *blue*? the clouds that shimmer in an autumnal setting sun, really *red*? a full moon in Indian summer, *bloody*? Are solar and lunar coronæ and halos, and the rainbow reflected and refracted from falling globules of rain, *real colorations*? The tissues and their enclosed liquids, differing in density, in figure, and in diaphaneity, might be expected, themselves, in some instances, according to position, to magnify like the microscope, and to decompose light like prisms, or falling rain drops, while on the other hand, absolute transparency in the object, renders it as invisible, and as structureless, as the air itself. Moreover, a dim light, at certain angles, throws enormously long, or very short shadows. One object rises above another, while the focal point or field of vision, is always, if accurate, extremely limited; and, hence, diagrams and descriptions, however correct, explain little, where the thing itself is involved in an almost inextricable confu-

sion, whether the fluids, or their containing vessels be the object of research. For example, in the examination of fluids, I have observed a very striking, possible source of fallacy, namely, an apparently spontaneous formation of very regular capillaries, resulting from the desiccation of animal fluids. During this process, air, water, or some portion of the liquid, perhaps influenced by homogenous attraction, arranges itself at first, into river shaped channels, or tubes, with many branches, all of which contract more, until they become fixed, while innumerable anastomosing connections join with other similar trunks and twigs, forming a net-work very like capillaries, though more open, and straight. Other fluids, in desiccating, form innumerable fissures, different, it is true, from the above, and from capillaries, but the tracks of these fissures correspond very closely to some portions of the net-work of capillaries, and may be even mistaken for them. Before I ascertained the fact, first mentioned above, I had concluded, erroneously, as to the quality of mucus expectorated by a patient. I found, as I supposed, capillary vessels under the microscope. There is, as I accidentally noticed, another possible source of error, in making capillary preparations between plates of glass, which though closely bound together, and kept in box, permitted, nevertheless, the formation of cryptogamous plants, (chiefly *Fungi*) of several kinds, invisible to the naked eye, but exceedingly beautiful in the microscope, having delicate stems, which by crossing each other's path, seemed to form a capillary net-work, among the tissues, and in other places. Their radicles and trunks, though extremely minute, are much more linear and uniform in size than capillaries.

I have examined hundreds of capillary specimens, both human and comparative, both wet and dry, healthy and morbid;—in the capsule of the lens, the hyaloid membrane, the iris, the retina, the choroid, the pia mater, tunica vasculosa, the serous, mucous, and other tissues in the principal structures of the human foetus as early as the second month; also in the beautiful, and highly transparent tissues of young alligators, as the nictating membrane, retina, hyaloid, iris, mesentery, peritoneum etc. But, *a priori* conclusions and conceptions were not realized; capillaries like regular rivers growing from thousands of tributaries—capillaries like arteries and veins, with thousands of ramifications, forming a regular and continuous net-work, were not seen with that uniformity, as to enable me (whatever others may have seen), to draw a satisfactory conclusion, where a capillary vessel begins, where it ends, and what is its common direction. Isolated points, centres or reservoirs, are sometimes seen, from which capillaries radiate as if wholly independent, as if possessed of circulations within themselves. Again, a large, short canal, a spongy islet, or a stellated point, receives small vessels without any apparent outlet! The same may be affirmed of many regular arborizations, which seem as isolated as one spider's web is from another's. The capillaries, sometimes, appear rough, porous, and spongy;—conditions unfavorable to the perpetuation of motion generated in the heart. They enlarge and diminish irregularly—tap each other at every possible angle—form every possible curve, if not every possible figure.

"From the result of microscopic examinations, little doubt rests on my mind," says Prof. Jackson, of Philadelphia, "that a large proportion

of what is regarded as capillary circulation, is not in fact performed by vessels. On the contrary the blood circulates out of vessels, but in currents. The currents of globules flow in every direction; I have seen currents of globules commence when none existed—pursuing every course with great diversity in their velocity etc.,—some in a retrograde course.” (Princip. Med. 23.)

The importance of the microscope, cannot easily be overrated, in the revelation of minute objects, showing their outlines, organs, general conformation, and the *tout ensemble*, all of which, are very striking in a moderate light; but, unfortunately, it fails to a great extent, exactly where, judging *a priori*, it ought to afford the most satisfactory results: for, in many instances, instead of drawing more clearly the lines of distinction—instead of showing, in a striking manner, the specific differences between tissue and tissue, between fluid and fluid, it reveals a closer similarity, a nearer analogy, if not identity. Thus when properly prepared, the fibres of muscle, fasciæ, tendon, pleura, peritoneum, arachnoid, sclerotica, etc., will appear so much alike, that the microscopist will often have to look at the labels to tell the difference; at least such is the case as to myself, and it is still more so, to some of my friends. He who has a good eye, will probably distinguish certain objects, with it, better than with the microscope, as the difference between a muscular and nervous fibre, a nervous and arterial twig etc.; whereas, the contrary rule, holds good as to infusorial organizations, and multitudinous objects of a minute size, and of sufficient diaphaneity.

It appears to me, quite impossible, to see, and describe with anatomical accuracy the capillary vessels, the circulation of globules, and their structure, or even the groups of organs in many objects called microscopic, not to mention entire animals, as large insects, the limbs of frogs etc. Even the most delicate and transparent infusorium, is a great mass of tissues, piled one above another, presenting different results, at different perpendicular foci, and giving different appearances, with the increment, or decrement of light etc. Suppose that an entire frog, or man, in a transparent state, were placed in a microscope; millions of vessels will be found in the same field of vision, or in the same axial line, the one above the other, traversing each other, in every direction; every change of glasses will present a new focus and a new phase, *ad infinitum*. It is probable that a life-time spent in this confused method of examination, would produce no satisfactory anatomical result. Suppose a thin layer of tissue is to be examined. Here, the chances of error are diminished, but they still exist—an object is selected, as a capillary. Is it a tube, a muscular fibre or something else? For it is difficult to determine in the microscope a solid cylinder from a tube. But, suppose a tube is found isolated, not one among many in the axis of vision,—is it a true capillary, a vein, a lymphatic, or an artery? How can its structure or coats be described, as some pretend to describe them, without first laying open this tube, in order, to examine its different strata, upon an exact level? Who can do this, in an object invisible to the naked eye? It is not the experimenter, but the reader, with pictorial illustrations before him, that has clear conceptions of these things. Indeed, Schiller contended that the Ideal was always better than the

Real. Thus, of morality and liberty, he says—(he might have included Micography) :

— — — — — If we test
By the *idea* of the good, the best,
How mean our efforts and our actions are !

* * * * *

Freedom is *only* in the land of *dreams*
And only blooms the beautiful in song.

The hesitating manner and the contradictory accounts of different authors, show that the anatomy of the capillary system is but little known. The micographers of the seventeenth and eighteenth centuries, investigated this subject with great industry and ability, and it is fair to presume that this part of anatomy is attended with great difficulties, since so little has been clearly established. Never were results more sterile, notwithstanding the grandiloquence of the day.

Beclard says, that “minute injections and microscopical observations soon led anatomists to admit that instead of the supposed parenchyma interposed between the terminations of the arteries and veins, according to the ancients, everything in the body is composed of vessels ; an opinion which yet divides all the cultivators of the science.”—“The capillaries are intermediate between the arteries and veins. It is in these vessels that, insensibly and without any fixed point the arteries are converted into veins. However, the capillary vessels have been generally described as the last division of the arteries, rather than the beginning of the veins. Whether this be well founded etc., the texture of the capillary vessels cannot be observed with the naked eye. These vessels have very thin and soft parietes, invisible to the naked eye, and slightly visible with the microscope, very little different from the substance of organs, and also from the humors they convey. They seem rather formed out of the substance of the organs than provided with its (their) own parietes.” “It would be impossible to stick the point of a fine needle without opening several of these vessels.—Beclard thinks it quite impossible to decide upon the existence of the colorless or serous capillaries on which much has been written. He adds,—“Doellinger thinks that the arteries, at their extremities, cease to have any parietes, and that the blood flows unconfined in the solid substance of the body. Wilbrand goes still further : asserting that the whole of the blood is converted into organs etc.”

Professor Carpenter's anatomical history of the capillaries does not seem embarrassed with any doubts whatever, though it bears on its face very little that can be called absolute certainty. He says that “the capillary circulation is carried on through tubes which have *distinct membranous parietes* ;—originating in cells”!—in another place, he says that “the capillaries *arise* from a minutely anastomosing *network*, into which the blood is brought by the ramifications of the arteries on *one side*, and from which it is returned by the radicles of the veins on *the other*.” (Physiol. 1848.) Cells ! network ! ramifications ! radicles ! one side ! and the other side !

The Medico-Chirurgical Review, unwilling to allow the capillary system any share in vital phenomena, espouses “the Cell-Theory, as the greatest of all physiological discoveries ! Every animal and plant is developed from a *nucleated cell*, by which all the organic tissues are formed, and by

which, nutrition, secretion, absorption, assimilation, growth, decay, etc., take place; all these processes are *independent of the blood-vessels*, being *extra-vascular*, as all accurate observers in every part of Europe testify." (See this Journal for 1844-5.)

The micographical physiologists are now making great efforts to plant the tree of life in a "*cell*!"

A better type of life can be found in an animal never before described *, which, I have called, *the solar infusorium*, (because discerible only in the direct ray of the sun,) entering as it does, like bricks into the cell wall, and into every other tissue. A Nilotic crocodile, supposed to have been dead five thousand years, is, to a considerable extent, composed of this animal, which springs into life, as soon as the tissue is sufficiently dissolved. I am fully able to show that the motion of this animal is not owing to evaporation, which, as I suppose, renders this discovery complete. This animalcule is very distinctly seen with a moderate magnifying power—a high power is too inaccurate.—It is probable that it cannot be killed, certainly not by boiling, and the strongest heat short of combustion. (The latter, perhaps, only dissipates it.) These animalcules are all exactly alike. The *Vibriones* of Ehrenberg, Mandl, Mantell and others, approximate the *solar infusoria*, but are essentially different, taking the descriptions and figures of these authors for a guide. The solar infusoria enter into, and constitute the chief material of the different tissues of every *infusorium* that I have examined. In all hard bodies they are, of course, motionless, until solution be effected. Whether they are the *ultima* of organic life, is uncertain :

"Great fleas have lesser fleas, and these were made to bite them,
And these fleas have lesser fleas, and so on *ad infinitum*."

A brief examination of the principal prevailing theories which are supposed fully to explain the capillary circulation, is necessary. It would be a species of physiological cowardice, to pass these by without an attempt to answer them. If they be true, they cannot be injured—if false, they ought to be overthrown. The candid reader will judge of the argument and facts only, forgetting the great disparity between the humble writer of these lines, and the illustrious names to which allusion may be made. He who has not a perfectly satisfactory theory of his own, cannot do a better thing for science (discovery alone excepted), than to overthrow the theories of other people. Every untenable theory which is overthrown, is often one obstacle removed out of the way. Suppose that one of three theories must be true; as for example, that the capillary circulation is due to the heart—or to the chemical attraction of septa—or to an inherent vital force in the system. Now if the two first can be overthrown, the latter must be true, at least, upon this assumption, but if the latter, like the former, can be overthrown, even though none be offered in its place, so much the better. The question is then an open one. The more causative antecedents shall be restricted and limited by experiment and sound argument, the greater will be the chances of finding the true one, at last.—The era, if not expediency, justifies a free examination into scientific, as well as into governmental questions, unterrified by great names.

* See New Orleans Med. and Surg. Journ. for Nov. 1846.

Dr. Copeland (with many others) attributes the capillary circulation, "functional and organic", wholly "to the influence exerted by the ganglionic nerves"; this logic like that which attributes muscular motion to nervous fluid, may be called reasoning *per saltum*, not by the *lex continui*, still less by the *lex parsimoniae*, since it supposes that nature does not proceed continuously, but by jumps. one tissue, without adaptations, does the work of another, having adaptations, employing many, instead of the fewest possible number of laws. Teleology or the science of causes, ends and adaptations, should always be esteemed paramount, until the contrary is proved. Dr. Copeland, curiously enough, says, that besides this ganglionic force, there is another "which tends to a state of repose, and is exerted in the organic structures themselves, at the point of contact, of the solids and the globules,—compared to a vortex, whence globules constantly pass from the arterial or terminal capillaries, and are lost in the different tissues,—each one attracting from it those constituents of which itself is formed, and which are always present in healthy blood. This latter force, which was first very minutely examined by Professor Schultz, and briefly stated by M. Andral, in his Pathological Anatomy, without acknowledgements"* etc. It is difficult to see, how this statical force, this force "of repose", can be an element in vital dynamics, or vital motion, unless the physiologist like the physicist, take for his point of departure for all the forces, the law of inertia. There is no difficulty, however, in showing that Dr. Copeland, notwithstanding his vast erudition, has fallen into an error, in claiming this supposed discovery for Professor Schultz. The following extracts from Haller's First Lines of Physiology, will, it is supposed, not only prove this, but, also, prove otherwise very interesting: "As the velocity, so the slow motion of the blood in the ultimate vessels, has its peculiar effects. In the larger arteries, the most heterogenous particles are whirled about amongst each other; in the smaller branches the progressive motion of the blood being diminished, the lighter particles separate from the very ponderous and red globules, and are forced towards the circumference etc. [In the capillaries] the attractive powers of the blood are also increased—oil is separated from the blood in one part, water in another, mucus in a third. The coagulable juices are separated almost everywhere, from the arteries themselves—each in its respective place, milk is never secreted in the kidneys etc. The blood itself from which any liquid is to be secreted, assumes in various places, that peculiarity of character, that it contains more particles, of a like nature with those which nature wishes to predominate in the fluid to be secreted. Slowness [in the capillaries], facilitates attraction and viscosity; similar particles, when brought together, can better attract and join each other, so as to retain the larger canal, while the thinner parts go off by the lesser lateral branches. In the arteries there is a very great degree of friction—of the blood globules against the arteries—of the arteries contracting round the blood like an obstacle,—and of the particles of blood amongst each other by the confused and vortical [vortical] manner in which they are propelled." Other passages of similar import, might be cited from Haller, showing that the Cartesian

* Dict. Med. Art. Arteries. Blood.

vortices, the impulsion of the heart, and the inherent force of the capillaries themselves, produce the circulation. It would be difficult to imagine a more unsatisfactory explanation than that offered by Dr. Copeland, whether it be considered as a transcendental or an experimental one. No transcendental explanation is good, which is not probable, much less impossible; no remote experimental explanation is good, wherein all analogies and adaptations are rejected for a hypothesis. Why should the nervous ganglions, without any adaptations, circulate the blood in the minute terminal arteries, in preference to the heart and arteries, which, certainly have a much more favorable mechanism by which to accomplish the end? Besides, I can assert from experiment, that the ganglionic masses may be destroyed, without destroying the capillary force in the human subject.

It would be improper, to pass by the chemical theory of the capillary circulation, advocated by our distinguished countryman, Professor Draper, of New York—a theory, received in Insular Europe, with an alacrity, nay, with a rapture without parallel, judging from the encomia, by the late British Reviews, and systematic writers of established reputation—a theory which is distinctly claimed as original with Dr. Draper, and still more distinctly claimed as original with Professor Daniell, of England, who is said to have “preceded the former by several years,” and “who took a more philosophical view of the subject!” Here a remark may be allowed, on the occasional mysteriousness of trans-atlantic logic! The above example literally reads thus: Dr. Draper made a discovery, but Mr. Daniell preceded him in it, and “took a more philosophical view of the same”, but, then, Dr. Draper preceded Professor Liebig! Mr. Daniell is very quietly crowned with the originality, but then the English language is taxed to the uttermost to show the merits and claims of Dr. Draper in this particular. The British admiration for Dr. Draper is not misplaced, and being rare, is doubtlessly, very acceptable to his friends, but, in this instance, (the merits of which I do not pretend to know), if Mr. Daniell made a discovery, and “took a more philosophical view” of its import, then Dr. Draper is placed in the category, of which Malvolio discoursed,—“having greatness thrust upon him.”—Dr. Copeland, in his valuable Dictionary of Medicine, denies that “Lænnec was the discoverer of the importance of auscultation in the investigation of disease.” Dr. C. claims this discovery for his “countryman Hook”. The latter must have preceded Lænnec nearly two centuries!

The British and Foreign Medico-Chirurgical Review for July, 1848, asserts that the whole problem of the circulation is solved by this Draperian theory—“it is a satisfactory *vera causa* for the phenomenon in question—a beautiful application, fully and satisfactorily made by himself, in no hesitating or uncertain manner, with clearness and precision—a masterly demonstration of a *physical force in the capillaries*—adequate,” etc.

Professor Draper’s beautiful and learned folio, on the Forces which produce the organization of plants, published in 1845, utterly repudiates the doctrine which has prevailed ever since the days of Harvey, and which Professor Matteucci and others still adhere to, namely, that the heart carries on the entire circulation.

Prof. Carpenter, of England, seems to be perfectly satisfied with Prof. Draper's explanation of the capillary circulation, which he quotes and adopts.*

To say nothing of my own incompetency for the task, I do not possess the documents and dates necessary to a decision of the question of *originality* in this branch of Physico-Physiology. I believe, Mr. Daniell was preceded by Doctors Mitchell, Faust, Togno, Coates, Lawrence, Rogers, and last, though not least, by Dr. Draper. The publications of the gentleman last named, in the American Journal of Medical Sciences, anterior to that on Plants, afford, perhaps, the best proof, that could be desired, by a Physicist imbued with "mechanical principles", not to name those of teleology, showing the utter *impossibility* of this assumed physico-circulation. Hundreds of plates and figures, could be instanced, each being the *aninomy*, not the analogue, of the physical mechanism required in order to produce any motion identical with the circulatory systems of man. I have before me, at this moment, a paper, by Dr. Rogers, (who is by no means an *ultraist* in this "wetting" theory), in which there are 22 figured illustrations † and which with the remarks accompanying them, are very interesting in a physico-physiological point of view;—not, indeed, in the explanation of the circulation, —for endosmotic action is probably not even an auxiliary, but an antagonistic force, *to be overcome*, and necessarily so, whether the *vera causa* of the blood's motion be chemical, or vital.

Professor Harrison, of the University of Louisiana, in his work—"An Essay towards a correct Theory of the Nervous System"—published in 1844, has given a very able analysis of these experiments, with ingenious speculations on their physiological applications, under the head of ABSORPTION.

Professor Draper, says,—“Physiologists have long seen, in opposition to the popular opinion, that the heart can only exert a *very subsidiary* action.” 37. “The flow of sap and the circulation of the blood are due to the *same* powers.” 22. “The principles of the circulation of organized beings are *strictly physical*.” 23. “All capillary phenomena are cases of *electrical attraction*”; 28; and, everywhere, he maintains, that this capillary, this electrical action, and, what is called *endosmosis*, consist in the *wetting* property of vessels, and that the circulation is strictly mechanical: “in man the circulation of the blood is caused by the oxydating action of that liquid on the solid structures with which it is brought into contact.” 29. “The arterial blood has an *intense affinity for any of the tissues* with which it is brought into contact; the venous blood has *little affinity for any of those structures*, therefore, the arterial blood will drive it before it, with an *inexpressible force*; the pulmonary circulation is the converse of the former—drives the arterial blood with an inexpressible force” &c., 33–41.

To say that this is the most fanciful theory ever propounded by a great man, is but to express an opinion, which, in these days of experimental inquiry, cannot be regarded as a satisfactory test. But, when a theory appeals wholly to “*mechanical principles*,” the mechanism being open

* Physiology.

† Am. Jour. Med. Sci. Aug. 1836.

to all for examination, when a theory appeals to animal septa, the anatomy of which, the humblest dissector and micographer can see—when a theory appeals to chemical actions which every one may test in the laboratory, and when this same theory is propounded, as “a strictly physical” one, surely a little inquiry cannot shake, much less overthrow, “strictly physical principles,” “animal septa,” “the wetting properties of solid structures,” “electrical actions,” “intense chemical affinities,” and “mechanical principles.” The humblest student, and and the most authoritative professor, must, however, equally stand on the same Baconian Platform, as the servants and interpreters of nature: —“Homo, naturæ minister et interpres, tantum facit et intelligit quantum de naturæ ordine re vel mente observaverit; nec amplius scit, aut potest.” “Here will I hold:” for this doctrine of the circulation cannot be received as a transcendental one; it expressly claims to rest on pure physics or mechanics, and by this it must be tested. In the early part of this paper, the physical principles of motion have been noticed. How do they apply here?

The fundamental principle in this theory is taken for granted, or rather kept totally out of view, namely, the mechanism, or the anatomical arrangement of septa or membranes, having on one side an “intense affinity for oxydated blood,” and all so arranged, as to produce a flow in one direction, upon “strictly physical or mechanical principles.”

The anatomical portion of the theory, is neither proved, nor probable. Where are these septa? Do not the great majority of the physiological anatomists deny that there are even parenchymatous divisions between the arteries and veins, seeing that injections pass, though rarely, from one class to the other? What is the anatomical character of these septa? which is the side that attracts arterial blood? Matteucci, not to mention our own countrymen, who preceded him shows, which side of a skin or membrane, as the inside or outside, has the strongest wetting property or endosmotic action. But admitting as an anatomical fact, which no one pretends to have seen, that there are septa;—that one side is venous, and the other arterial;—that the wetting property of the one side transcends that of the other;—that the oxydated blood is attracted by the one side, and not at all by the other; and that these septa are so accommodating as to interchange affinities—the arterial side attracting oxydated blood from the left side of the heart, while in the lungs the whole is reversed, the venous blood from the right side of the heart being attracted quite as strongly notwithstanding its deoxydation, admitting, I say, all this, still, the circulation is not accounted for, either transcendently, or experimentally, nay, the induction from the whole shows, that the circulation is an impossibility upon “the mechanical principles,” assumed,—for no one pretends to have seen any mechanism in the capillary system that could by any possibility accomplish the result. All the membranous septa experimented with, have a definite arrangement, of which, diagrams and apparatusus are usually given, with descriptions, very mechanical, indeed.

Thus if a liquid or a gas permeate a membrane, in a certain direction, as from the interior to the exterior with a certain force, this direction, and this force, would be nullified by millions of septa, placed between heterogenous liquids, and gases, in every possible manner, so as to act,

not together, but in opposition : by no possibility could such a *mélange* of forces or septa, act upon purely mechanical principles, in a common direction, as in straight lines, regular curves, or circles ; by no dynamical principle, could the primary action of each septum, nor the resulting action of all the septa produce, especially among heterogenous fluids and opposing septa, circulations like those of the right and left sides of the heart, of the liver etc.; in fact, every experiment with gases, liquids, septa, tubes, jars, endosmometers, and the like, proves, not the human capillary circulation, but the existence of a force which often acts in opposition to it, like gravity, and, which, like it, is, and must be, constantly controlled by a *vital* or *contrary* force. The endosmotic action is not the type, but the *contrast* of the capillary system. And, more than all, the structural adaptations, in endosmosis and vital capillarity, are altogether different ; at least, no proof has been given that they are alike.

According to Professor Draper, endosmosis is nothing but the *wetting* property, and, as the whole composition of forces in the circulations, systemic, pulmonary, portal, lymphatic, lacteal, are so completely chemical, that, (to use his own words), "had we known *nothing* of the *circulation* but been instructed in the *chemical* relations of the blood to the soft tissues and atmospheric air, we could upon *physical principles* have *predicted* the existence of that circulation, and *shown* what its direction in different organs *must be*"—36—Shade of Harvey! how easy it would be now, since all is known, to arrange a few hundred bladders in a trough, like a galvanic battery, with oxydated blood, showing disbelievers, how, by *wetting*, all the complex circulations can be carried on "upon strictly physical principles." It is very meritorious in chemists to subdue the vital forces, and to bring them within chemical jurisdiction ; but this cannot be done, by assuming conditions and forces, as similar, nay, identical, while they actually differ in every essential particular. Dead matter is not the type of living ; a *wet* bladder produces no motion analogous to that of the muscles, or that in the capillary system. It is highly probable "from physical principles," as will be shown, that this assumed chemical action of septa, is really a cause of retardation, even more than gravity, nay, that it is an absolute contrast to vital capillarity, which latter, is only known inferentially by its multitudinous phenomena, and which, has not a single type in inorganic chemistry, nor in mere physics. Its name is nothing. "A rose would smell as sweet by any other name."

Professor Draper's theory utterly fails to account for that portion of the triune-circulatory system, called the systemic ; but if it be admitted that this part of the circulation is fully proved, then it follows, that both the pulmonary and portal circulations are absolutely impossible, since, the two latter exhibit a complete negation, or opposition to the former. This can be shown as to the pulmonary circulation ; for if the arterial affinity for the supposed septa, or tissues, should drive the venous blood before it to the right side of the heart, there it ought to remain, seeing the heart's action "is only subsidiary," for being venous, and the force mechanical that brought into the heart, and this force, being there neutralized, the blood ought to have no affinity ; it ought not to be drawn with an "inexpressible force," into the tissues of the lungs, because the tissues ought to have no affinity for deoxydated blood, especially,

when that deoxydation has reached its maximum condition of deoxydation, that is, just before it reaches its distribution in the lungs, where it is to be oxydated. Here, again, another contradiction occurs, for the pulmonary veins take a sudden affinity (which they ought not to do), for this sort of blood. They chase it down into the left auricle, which, as a physical race, is quite impossible. It gets into the heart, which it ought not to do, without many septa; here, the affinity is metamorphosed into an arterial one! But, all these contradictions are surpassed by those of the portal or abdominal circulation. Imagine a live oak with two tops, (the roots representing one top); one of these tops, by innumerable branches, (capillaries), collects the blood of the abdominal viscera into the veins, and finally into a common trunk, which latter expands in like manner on entering the liver, and thereby, like an artery, distributes the same blood to the entire organ. Here the black, carbonized blood, which according to this theory, ought to have no affinity at all, is attracted by the supposed septa, as strongly as if it were fully oxydated! The deoxydated blood being thus distributed, new difficulties arise—a new set of septa is required to draw this same blood into the *vena cava hepatica*, and thence to the heart, a most unfortunate circumstance, seeing that this deoxydated blood has no affinity for these tissues! Nor is this all! There must be another set of septa to attract the arterialized blood of the hepatic artery. Still, more. The biliary circulation, will require septa. The end is not yet. The lymphatic circulation must be provided with endosmometers. In fact these fancied septa do not appear, analogically and mechanically speaking, adapted for all these complex circulations. A town could be as well laid off into lots, streets and squares, by following a bushel of ants, poured out on the ground, each bent on a different route, as the circulation could be directed by such septa.

Analysis gives nearly the same elements for both arterial and venous blood, water being about eight-tenths of the whole. Has any one offered a single experiment, showing that one kind wets an artery, a capillary, or a membrane, while the other does not?

I protest, therefore, altogether, against these exanimate liquids, bladders and glass tubes, and their "strictly mechanical actions," as true types of human structures, and of the living circulation; seeing that the former are not simply *contrasts*, but *antinomies* of the latter, in structure, modes of action, and general results,—seeing that the wetting and non-wetting septa are not proved,—seeing that the deoxydated blood actually performs more complex movements than the oxydated blood, I will dismiss this part of the subject, with two statements, one from M. Matteucci, relating to sap-circulation, and the other from Natural History.

Professor Matteucci, recently appointed by his government to deliver a course of lectures upon the Physical phenomena of living Beings, appears to have executed that task with much ability; certainly, he has not been backward in asserting the claims of that part of physiology which is *physical*; nor has he shown any unwillingness to include in that category, laws, which, many regard, as purely *vital*; yet, with all his predilections to appropriate the latter, in favor of the former, he admits, even with respect to the *vegetable* kingdom, that "the double movement of juices in the interior of vegetables, is a thing *inexplicable* by the mere

forces of *capillarity* and *imbibition*. This force of impulsion is *incompatible* with the effects of *capillarity* and *imbibition*—a force that has its seat in the *ultimate extremities of the roots*. A liquid rising in a capillary tube, cannot overflow the tube by the effect of the *same force* which raises it. He gives examples (a celebrated one from Hales), showing that the sap of a cut vine exerts an upward force, raising mercury equal to 43 feet $3\frac{1}{2}$ inches of water. Hence, he concludes that, “the great height to which the liquid can ascend, appears to be *opposed* to the explanation of the phenomenon by considering it as an effect of *imbibition* or of *capillarity*.” 95-6.

Without delaying to explain the special anatomy of the crocodilian heart, (which has, I believe, been wholly misconceived by naturalists,) it is sufficient to say, that it has all the complexity of the human heart, with a most remarkable superaddition,—a wheel in a wheel—namely a large artery, which I will call the *Gastro-enteritic*, arising from the *right, or venous side* of the heart, which *distributes the deoxydated or venous blood to most of the abdominal viscera*. It is a matter of no importance to present argument, to show that although the arterial or left side circulates only about half of the blood, there is a sort of provisional or supplemental structure—a short canal in the abdomen—by which the natural inequilibrium shall not be further augmented; it is sufficient to say, that deoxydated or venous blood is distributed—“attracted with an inexpressible force”—as Dr. Draper would say—*without arterialization* by “the soft tissues,” just as forcibly, as if it were *oxydated* blood.*

When I come to the experimental portion of this paper, I will show that the capillary circulation in the human subject, cannot be dependent on the oxydating process.*

M. Matteucci being one of the latest, and most authoritative writers on the circulation, and withal, an advocate of the old doctrine which attributes this process to the exclusive motor power of the heart, a rapid summary of his views, will conclude the only remaining theory of importance, supposed to be explanatory of the capillary action.

M. Matteucci quotes and adopts M. Poiseuille's experiments and conclusions,—the induction from the whole, is thus expressed: “the movement of the blood in the capillaries ceases when the heart is raised or bound, and that its movement continued only for a few minutes, on account of the diminution of volume, and of the kind of contraction which the elastic coats of the vessels suffer when the blood ceases to be propelled by the heart.—The capillary circulation is uninfluenced either by a vacuum or by a pressure of eight or ten atmospheres.”†

*The principles of Teleology—the *causæ finales*, are of the utmost value in experimentation. Had I not watched, for nearly one continuous year, the habits of the alligator, I could hardly have believed the anatomical fact here adverted to, seeing it is, apparently irreconcilable with physiology. An undisturbed alligator, be it observed, breathes but rarely, even in the hot, non-hibernating season! Hence, consuming but little oxygen by respiration, it is unnecessary that all the blood should be sent to the lungs. In the structure of the latter, there is but little parenchyma; they are chiefly sacculated, or vesicular.

† Living Beings. 327-8. These very inconclusive experiments, are summed up in the *Jour. des Prog. des Sciences*.—218. Paris. 1835.

After making due allowance for the illusions incidental to vivisections of the frog's heart, I must say, that both the theory and the facts—certainly the latter—appear to me to be fallacious, nay, self-contradictory. It is contradictory to say, that motion ceases, and yet continues a few minutes!

It is a mistake, to say, that the arteries contract, (in the human subject, at least,) after death, so as to empty themselves completely.

This never happens "a few minutes after death,"—at which time there is occasionally blood in these vessels, but it soon disappears, leaving the arteries empty. It is even highly probable that the dead artery is larger than the living—the former by losing its natural or living tone, while the latter, as all know, is capable of contracting *præmortem*, upon its diminished contents, as in great losses of blood, and towards the close of life, so as to feel, under the finger, like a mere thread. This diminution of calibre, corresponding to the diminution or absence of the blood in the artery, is not seen, I repeat it, in the dead body. If "the motion of the blood ceases," as says Prof. Matteucci, "when the heart is raised or bound"—(327)—how is it that the most complete act of the human circulation, far transcending the healthy, takes place soon after death, namely, the complete emptying of the whole arterial system, including the left side of the heart itself, where, by every principle of hydrodynamics, it ought to accumulate, leaving the right side, and the entire venous system empty, and not strongly distended, as they are found to be, almost invariably? Now "this diminished volume, and this kind of contraction," would make the great arteries, striking objects, indeed, like solid rods, during contraction: for if the abdominal aorta should fail to contract completely into a solid cylinder,—if its cavity were open, and unobliterated, so as to admit even a hair, or a probe, that space would contain blood.

M. Matteucci, maintains, therefore, that the "binding or raising of the heart," completely arrests the capillary circulation, except a momentary impulse due to the mere elasticity and contraction of the arterial coats, "which these vessels suffer when the blood ceases to be propelled by the heart. By the aid of the microscope," he continues, "there was seen an immoveable layer of serum, adherent to the coats of the vessel; and the liquid thus moves in this tube, formed by its own substance. The same liquid in a glass capillary tube, and in a capillary blood vessel of a living or dead animal, follows the same laws in all cases. This fact assuredly proves, that in these various cases the liquid really circulates in a tube always formed of the same matter; that is to say, of an immoveable and adherent liquid layer, and which is the same as the liquid which flows through, whatever may be the material of the tube." 328. Without stopping to point out the internal evidences of error, physical and microscopical, contained in this statement, it is sufficient to refer the reader to the experiments, in the sequel, showing that the fundamental doctrine of Matteucci cannot be true. The merest tyro can appreciate the physical impossibility implied by these illusory tubes—*tubes formed of serum or water, inside of the arterial tubes*—formed the moment the "heart is bound or raised," seeing, that of all the constituents of the blood, the serum is the least adapted for such tubes—structures. The microscopic difficulties, here implied, may not be so self-

evident to the inexperienced, but they are not the less so, for that reason. Those who see the arterial tubes, and these firm tubes of warter or, as M. Matteucci would say, "immoveable and adherent layers of serum," "must see as through a glass darkly."

The next piece of experimental logic offered is this,—“If we tie an artery in a living animal, the vessel almost entirely empties itself of blood, and the circulation continues in it only for a short space of time (328)—a direct, though inadvertent admission that the circulatory force is more perfect without that from of the heart, than with it, for there no proof that the forces of the latter can in any case, much less in this, completely expel the whole blood in the arteries, however vigorous may be its contraction. If the artery can thus suddenly empty itself completely by its own inherent powers, why, in the name of all that is teleological, does nature establish two dynamical organs to do a work which one can perform more perfectly? Verily, it is uncharitable to charge her with such platitudes, and absurdities, without better proof.

M. Matteucci having thus detailed what he calls “the most accurate and conclusive experiments upon the various questions relating to the sanguineous circulation,”—(328)—every important point of which I have noticed—triumphantly concludes, that the whole is due “to the simple alternating *impulsion* given to this fluid (the blood), by the sudden contraction of a species of *sack*, (that is, the heart) which makes a part of the tube itself.”—331.

Dr. Marshall Hall, (like M. Matteucci,) says, “that the capillaries have no power to contribute to the motion of the blood, and that the capillary circulation depends altogether upon the action of the heart and arteries.” (Lond. Lancet, 1830. ii. 540). Again, in his later Work on the Blood, he says, “the pulsatory power of the heart is capable of moving the blood through the arteries, the capillary vessels, and the veins, even in the extreme parts of the system.” His observations were, as usual, made on frogs!

I have already shown that the capillary system itself, and all that is known of hydrodynamics, of retarding media, frictions, deflecting, and opposing currents, variable tubes, angles, and so on, render it improbable that this system should be exclusively under the control of the left ventricle. I could fortify this conclusion by many authorities. Let the following suffice: Mr. Samuel Cooper says, “the hypothesis of a *vis à tergo*, whether dependent on the heart alone, upon the arteries alone, or upon a combination of the two, has by no means proved sufficiently satisfactory, or been supported by evidence in respect to the entire circulation. Under no modification does it account for the flow of the blood through the veins.” (Good’s Study, i. 314.)

Dr. Green, (New York Jour. Med. 1844) repeatedly observed with the microscope the capillary circulation in the foot of the frog, “in vessels so minute as to admit only a single file of globules. These may often be observed running *different directions* in parallel vessels—in *different directions in the same vessels*, so as to *pass* each other—independent of any *vis à tergo*;—other evidences of this inherent power have been observed” &c.

M. Magendie, in his work on the Blood, among a multitude of assumptions, (and most awful fulminations against all vitalists), going to show

that a great vivisector can chop logic,—asserts, “that the capillaries are *exclusively passive* in their circulation; in the movement of the globules, there is *nothing vital*.” (158 to 169.) He maintains that the doctrine, that the capillary circulation is regulated by other laws than those of the great trunks, (purely physical) is one of the grand errors of the last and present century. (Ib. 169.)

Mr. Bryan, Surgeon, in the London Lancet, for April, 1845, maintains that the whole circulation begins in the left ventricle and proceeds *mechanically*, that is, the blood moves in the arteries, because the pressure is less than in the ventricle,—so of the capillaries, veins, on to the right auricle.

No one, so far as I know, has ever proved experimentally that the capillary, venous, and chyliiferous circulations existed, or could exist after the termination of the oxydating process of respiration, after the cessation of the heart's action &c. Dr. Carpenter, the able physiologist, of England, inclines, it is true, to the opinion of the independent action of the capillaries. So do many others. But, he expressly declares that, although, “the movements of the blood in the capillaries of *cold-blooded animals*, after complete excision of the heart, has been repeatedly observed: In *warm-blooded animals* this cannot be satisfactorily established by experiment, since the shock occasioned by so severe an operation,” etc. (Phys. §. 503.) Now, according to the well established doctrine of discovery, it is not the man who *suggests* or avows an opinion, who is a discoverer, but the one who *establishes* it by indubitable evidence. Columbus was not the discoverer of a New World, until he saw the land itself. Almost every discovery has been claimed by many persons of different nations.

I could multiply by authorities similar to that of Dr. Carpenter. His own strong language is quite sufficient. I offer evidence. The decision is with the reader. If I shall prove, that in the human subject, the blood circulates or moves independently of the heart, long after death, I will have proved what he says “cannot be established by experiment.” The nature of this force is not of any importance in this question.

In pathology, vivisection, the method usually pursued, is an equivocal guide, a limited portion of pathological surgery excepted. A vivisector, in physiology, is apt to be from the very nature of his vocation, a *physicist*, seeing *vital* actions only through *physical* experiments. Armed with mechanical instruments, he stuns, poisons, injects, ties, dissects, galvanizes, decomposes, waging a cruel war against poor, frail vitality, cutting the latter off, if too long, stretching her, if too short to suit his *physical standards*. The animals he selects for physical manipulation, are unusually of the lowest order, being quite unlike man. The *artificial* conditions he produces, though possibly illustrative of some *natural* conditions in the *same* kind of animal, do not often come up to the high behests of human pathology.

Sir Charles Bell, one of the most talented and laborious of experimental vivisectors, in referring to the mutilations and agonizing conditions artificially produced in animals, and fallaciously regarded as *types* of *natural conditions in man*, declares that “these experiments have never been the means of discovery; and that a survey of what has been attempted in late years in physiology, will prove that the

opening of living animals has done more to perpetuate error, than to confirm the just views from the study of anatomy and *natural motions*."

Prof. Caldwell, of Louisville, says, "Experiments that do no violence to nature, are useful; those which are the reverse of this, never fail to prove injurious."

The experiments I offer, are all prepared by the hand of nature. They were observed, not, *created*, by *artificial* methods. They are not taken from the *inferior* animals, *but* from *man*. This simplicity and naturalness, (so unlike the barren tortures of vivisection—the most unnatural thing in the world, except to show the natural history of agony), will no doubt be objects of derision, to those, who prefer whatsoever is complex and artificial, and, who, because the vital often *coincides* or *agrees* with the *physical* law, claim every thing for the latter! The facts that follow, are not referable in their main features to any merely physical, or non-vital law. They are the last acts of vitality, before yielding complete and endless submission to the laws which govern the inorganic world—the preludes to the entire extinction of life.

If these experiments shall prove nothing more than the doctrine of the independent circulatory force of the capillary system of man, they will have accomplished more in physiology, and in pathological anatomy, than most of the experiments made on frogs, with galvanism,—dignified with the name of *Electro-Physiology*, *Excito-motory-System*, &c. It is true that these vivisections (often made at the expense of the State, and by salaried professors) have, in the estimation of certain persons, so great a dignity, from the State ceremonial, as to compensate for their otherwise worthless character—experiments, as remote as *possible* from man and his healthy, and morbid conditions. It is difficult to see how yellow fever subjects can be slighted, since, no disease presents an equal proportion of young, vigorous, and muscular persons. A hundred cadavera from this disease, will, probably, present to the sculptor a greater per cent of model-forms, than a similar number of the army itself. Plants and frogs, endosmose and galvanism, (good in their way, but withal absolutely sterile), ought not altogether to supplant *man*, and his *natural conditions*, as subjects of medical science, human anatomy and pathology.

In the experiments on post-mortem venesection, elevated points and positions were chosen, when practicable, in order to cut off all possibility that the result might be influenced by gravity; at the same time, care was generally taken that the orifices in the arm, forehead, external jugular, &c., should be elevated but *little* above the level of the highest part of the body, for fear that gravity might favor the result—which it always does to a very limited extent at the first moment, though, it makes the subsequent part of the experiment far more striking and brilliant. Sometimes, the elevation was taken, by pouring water on a plank, the latter resting on the body. All the blood, at the moment, in the *distal* end of the vein, that is, beyond the orifice, would, if raised above the level, be discharged from gravity; perhaps an ounce or more would, in some cases, thus be lost in emptying the vein, but then, *no more could get there*, to replace the *first*, unless, by a power altogether *different from gravity*. Hence, it appears, that all the blood, with this

inconsiderable exception, after the *first moment*, flowed by capillary forces—without the aid of, or rather *against*, all that is called mechanical principles—against all the chemical forces incidental to respiration—against ganglionic forces, for the ganglions were often cut away,—and, always, without any aid from the heart. See, and apply for yourself.

1841. Yellow Fever. A man aged 34, died—was brought immediately &c.; a vein, in the left arm was opened—the blood flowed freely—on moving the muscles it jetted, and upon ligation, formed an arch of about eighteen inches in diameter, as in ordinary venesection. The left jugular being opened in the usual manner, bled copiously without jetting. The abdomen, and chest, were opened without delay; one of eight or ten little twigs of nearly equal size, belonging to the coronary vein of the heart, was opened upon its highest point; the blood shot out in a small, strong stream, a pint being discharged in a few minutes. The omenta and mesentery, were beautifully and forcibly distended, especially, in the venous vessels. The cavas discharged from three to four pounds of blood.

1841. Yellow Fever. A man, aged twenty-five, died, lying on his right side—soon after, he was placed on his back—in which position, in fifteen minutes after death, he was opened. The liver, which was very brittle, was penetrated upon its highest, convex surface; in a few minutes, three and a half pounds of blood were discharged. The blood was taken up from the abdomen, in a sponge, and squeezed out into a vessel and measured. A puncture with the *finger*, would not be likely to divide any large vein, nor even in the living state, produce much hæmorrhage. The capillaries of the portal system, supplied nearly all this blood, so rapidly delivered from the abdominal viscera. (At the close of life, this man had nasal hæmorrhage.) Those who practice anatomical injections, know that only a few ounces of wax can be forced into the vessels of the liver.

1841. A boatman, aged 32, of extraordinary size, muscularity, and weight—dead, from Yellow Fever, one hour: the jugulars being opened, the blood shot upward—in a short time, a gallon, by estimation, was discharged. [The time is not precisely noted]. At two and a half hours after death, when the dissection began, about one and a half pounds of blood, (not included in the above), had flowed from the orifices.

1841. An Englishman, aged 37, died while I was present, of congestive, erroneously diagnosed as yellow fever: in a few minutes, the body became extremely hot; the external veins, especially in the arms, distended, as in health, after exercise—two were opened—the blood flowed rapidly, projecting about one inch; by compressing the veins, and agitating the muscles, it shot out a foot, or more, clotting firmly, as usual. The arteries were empty. The cavas discharged about four pounds of blood. An hour after death, a fine, warm, natural sweat broke out on the face, neck, and chest; a strong febrile smell emanated from the body. (After death from *solar* asphyxia, [sunstroke], I have seen the veins of the forehead, suddenly become prominent; the face, cyanosed.)

1843. Yellow Fever. A New Yorker, aged 26:—Experiments,

from the third to the fifth hour, after death. Room 80° ; body in the axilla, and stomach 101° ;* liver $102\frac{1}{2}^{\circ}$; thigh 100° ; brain 95° ; moderate rigidity. The veins of the forehead extremely prominent, to a degree rarely seen, when a man in health stoops to raise a great weight. A block placed under the back-head, raising the forehead higher than any other part of the body, caused, for a second or more, a recession of the blood, but it quickly distended the frontal vein as before. This vein was opened at its highest point on the forehead—a strong, but not a jetting stream, ran down the temple for about a quarter of an hour, discharging about twelve ounces of blood—after which, a rapid dropping, for a similar period, produced about four ounces more. The skin of the head, face, and neck, which hitherto had been greatly congested, marbled, almost black, from blood in the capillaries, now became much more natural. During this venesection, an eye had been removed, and a thermometer thrust into the brain, and, also, the chest and abdomen had been opened, without any retardation of the current. It is supposed, that all the blood above the orifice in the frontal vein, subject to the flowing by gravity, did not exceed twenty drops. The orifice discharged nearly an ounce per minute—the original blood was replaced 24 times per minute—288 times in 12 minutes; or 1,440 times per hour—34,560 per day.

Here, it will be observed, is a large blood letting, without a bandage or pressure. Can any bleeder, by this method, get half as much blood from this vein, in a living man?

It is remarkable, that all the external veins, those of the face excepted, were, in this subject, collapsed! Capillarity, in this region, alone, persisted for five hours. This man before death had copious nasal hæmorrhages.

1843. Yellow Fever. A Baltimorean, dead half an hour, was bled from the arm in the usual manner, the orifice being on a level with the highest part of the body—the blood jetted about two inches, but soon subsided to a rapid dropping. In thirty minutes, about six ounces were discharged. Contractility, powerful—Caloricity, persisting: Axilla at successive periods of 3 to 5 minutes, thus: 105° — 106° — 107° — $107\frac{1}{2}^{\circ}$ —etc.

1843. Yellow Fever. An Irishman, aged 33—dead two hours: The subclavian veins discharged several pounds of blood. Three hours after death, the left lobe of the liver bled profusely from an incision on its convex surface, made in taking the temperature;—an incision in the right auricle of the heart, discharged much blood. At four hours after death, the eyes still continued minutely injected. Air 61° —Brain 80° ; epigastrium 84° —left chest 80° —centre of the thigh 84° . Contractility, moderate.

1843. Yellow Fever. D., before, and for an hour after death, had minute and vivid injections of the eyes.

1842. Yellow Fever. An Englishman, dead ten minutes, was, after a few experiments on the muscles, opened: The peritoneum, omenta, mesentery, etc., had their minutest vessels, arterial and venous, dist-

* For the sake of brevity, I will generally omit the experimental history of post mortem caloricity and muscular contractility,

ended, and bled freely, when punctured. The cavas etc., discharged, in less than an hour, from eight to ten pounds of blood, by estimation.

1842. Yellow Fever. Miss * * aged 23 : Dissection, without delay. The omentum, mesentery etc., distended with both venous and arterial blood, in a manner difficult to describe, flowing freely from the smallest cut, in the higher, as well as in the lower organs. The subclavians, in a few minutes, discharged about three pounds.

1842. A German, aged 23—Yellow Fever—Dead thirty minutes : Twenty ounces of blood flowed from the external jugular, in half an hour.

1843. Yellow Fever. M. dead, three hours. Hot. Blood oozing freely from a slight puncture, at an elevated point of the shoulder or deltoid region.

1841. Yellow Fever. An Irishman aged 35, dead 12 hours : a circular incision of the scalp, discharged in a few minutes, about six ounces of blood ; incisionz in the arms and legs bled freely ; blood issuing from the ears. Head elevated.

1841. Yellow Fever. A German aged 19. Dead 30 minutes : A circular incision of the scalp, discharged forty ounces of blood in as many minutes ; the head was elevated on a block.

1843. A supposed apoplectic aged, about 30 ; stout ; unknown—died with apoplectic symptoms.—Dead one and a half hours* : heat at every two to ten minutes, in the order following : axilla 91°, 89° ; rectum 95° ; thigh 92° ; epigastrium and heart 94° ; brain 88° ; slight rigidity. Face swollen, dark red flush, as in erysipilas ; pressure, as in this disease, caused the blood to recede, temporarily, but it returned quickly, even in the highest parts of the head, face etc. Incisions of the integuments of the highest part of the body, bled profusely ; eyes injected, for some time, say, about two and a half hours after death.

I will here simply remark, without copying cases illustrative of the vascularity of the subconjunctival tissue, that real, active post mortem injection, very different from the ecchymosed or infiltrated eye, takes place, or continues as it was before death, in considerable number of cases. As the body always rests upon the back, it disappears, of course, from gravity, as soon as the circulatory force is extinguished.

I forbear to detail cases, wherein the cadavera were not bled, though, the tension of the veins, and congestion of elevated parts, often showed an existing impulsive force,—an example of which I observed in the person of S., two and a half hours after death from Yellow fever :—1848. A slight incision on the tip of the nose, in this subject, discharged in a few minutes four or five drachms of blood.—Two days afterward, I met with another subject which, half an hour after death, bled copiously, (as is observed, sometimes in the living body), from the scarifications in cupping. The heart of this subject, removed from the body, gave unequivocal evidence of a circulatory capillary force, upon its exterior, as branches of the coronary vein, which were emptied by pressure, soon after, become turgid again, in a manner inconsistent

* He was probably a laborer—was sent to the *Charity* Hospital, though, among his rags, 45 dollars in gold and some other monney were found at the dissection.

with gravity. The heart, of course, exercises no mechanical influence over the post mortem capillary circulation. Nevertheless, I have experimented, in every conceivable way, upon this organ, with a view of ascertaining whether pressure might not accelerate the flow during post mortem blood-letting. Nothing of that kind occurs. If, however, there be blood in the arteries, it can be readily expelled, by previously opening an artery, as one of the mesenterics; I have, by a few artificial contractions, or rather compressions, spurted out to a distance, nearly half a pound of arterial blood. The removal of all the cranial, thoracic and abdominal viscera, does not arrest the capillary circulation in many regions.

The *quantity* of blood found in post mortem examinations of fever-victims, is an important, and often a highly characteristic element in pathological anatomy, though, it appears to have been almost wholly neglected; while on the other hand, nearly everything that has been said, by foreign writers, of its *physical appearance* (so far as the fevers of New-Orleans are concerned) is remote from the truth. All that these writers say of the liquefaction, defibrination, putrefaction, etc., of the yellow-fever-blood, is unfounded; though, at the late periods at which examinations are usually made, such appearances are quite common. In no fever, perhaps, is the blood so natural, physically speaking, as in yellow fever. In yellow fever subjects, the *quantity of blood discharged* from the division of any great vein in the centre, as the subclavian, cava, etc., probably exceeds, in a given time, the quantity from any other class of fever-subjects, owing, no doubt, chiefly to a greater quantity of blood, and a greater activity of the capillary vessels after death. I examined a subject in St. Thomas street, two hours after death from a tedious remittent, without seeing more than six ounces of blood in the brain, chest, and abdomen; the most thorough dissection of every organ was made.

I will add only one more case:

1848. Serous apoplexy: The history filling ten pages, will be slightly sketched. A stout man aged 35, ate his breakfast as usual, and labored in the hot sun, until 9½ A. M.; the minimum of the day was 77°. He complained of indisposition, and on returning home, became insensible (as was supposed, from a stroke of the sun). This was the opinion of his comrades. At 11 A. M. he died. The experiments began twenty five minutes after death, and lasted one hour and thirty five minutes, before the general dissection. Contractility, too feeble to flex the fore-arm. Incipient rigidity. The thermometer remained 55 minutes in the axilla, without being removed; the first 5 minutes, gave 105°;—5 m. 106½°, 5 m. 108°; 10 m. 108°;—10 m. 108°; 10 m. 108°; 10 m. 108¼°; stationary; legs crossed—the knees 108°; perineum 108°; rectum 7 m. 111°;—5 m. 111°;—15 m. 110°;—epigastrium 109¾°;—middle of the thigh 108¾°; chest 107°.

Great distention of the veins of the head, face and neck, with red flushing of the skin, was observed. A ligature was placed on the right arm; a vein was opened as usual; about two ounces of blood jetted from the orifice, after which the blood trickled down the arm, and in the course of two hours discharged, in addition to the above, about twelve ounces.

The skin of the face and neck was injected, dark, livid, and somewhat mottled; there was no cadaveric hyperæmia or injection of the dependent parts; the external jugular veins were distended as if ready to burst. Greater tension, I had never witnessed, in glottidian œdema, nor in convulsions, nor in the last throes of parturition. The left jugular was opened, as for ordinary bloodletting, *but no bandage or pressure* was used, the head being raised, so that the orifice was nearly on a level with the breast-bone*. The blood jetted completely, without wetting the skin, forming an arch, the diameter of which, continued to extend for five minutes; at the end of 8 minutes, the arch had contracted, owing, apparently to small clots on the margins of the orifice, and the skin having once become wet, the blood, without being materially diminished, ran down the neck, jetting occasionally on removing clots from the orifice. For about one hour the flow was copious, but, at the end of that time, was diminishing rapidly. I caught nearly three pounds at first, but as much of it did not jet out, but ran down the neck, I could only estimate the amount (which I did), at five pounds, or 80 ounces from the jugular alone. As the bloodletting progressed, the congestion and discoloration of the skin of the face, diminished.

Now it will be seen, that the orifice in the jugular did not discharge the blood as fast as the circulation replaced it—there was a surplus, because, the venous tension or jetting augmented for five minutes, and had not ceased during 8 minutes. There was, as already mentioned, no bandage, or pressure. It is fair to presume, that it would be quite impossible in this way, to bleed a living man, half as much, as collapse of the vein, clots, fainting, etc., would prevent it. Hence, the circulation in the veins was probably more active and persistent, than in health! Let it be supposed, that the upper or *distal* end of the jugular, contained one ounce, when opened—this being discharged, no more could replace it, only by a circulatory force. But here, the tube is filled 80 times in a few minutes. This may seem incredible from but one among hundreds of veins. A portion of this blood, however, was not derived from the distal but from the proximal end of the vein, as will be shown, presently, by incontestable evidence. The explanation seems to be this: The capillaries filled the veins rapidly. The forces in the cavas, contended face to face. The right side of the heart had no outlet. The equilibrium between the ascending and descending cavas was broken. The latter was weakened by the force or pressure turned off by the orifice, in the jugular. The pressure from below caused a retrograde movement—a very short one—towards the point where there was the least pressure, that is, the orifice in the vein.

While these experiments were being made, the cavities were opened. The heart contained but little blood.

In the early part of the experiment, particularly after the jetting of blood was changed to a stream down the neck, I observed, for a long time, that is while the blood flowed, what then appeared to me, an inexplicable phenomenon, namely, white, milky streaks, or soft floc-

* This was ascertained by placing a straight or level plank on the highest points of the cadaver, and by pouring water on the plank. This body was also carefully measured, but no expansions, contractions, or elongations could be detected.

culent masses, passing out of the orifice in the blood, rather swimming on the top of the current—a diffused drop or white flocculent cloud of this kind, was carried out once or twice every second. The subsequent dissection, fully explained this. The stomach contained nothing but water, of a sourish scent, with a few, very slender, tendinous fibres, apparently of beef, perhaps not amounting to five grains; but the quantity of chyle, of a pale milky, white color, thinner than paste, in the small intestines, was very extraordinary. The lacteals were gorged with this liquid,—the very same kind that I had seen passing from the jugular. I had, fortunately, opened the left jugular, near the place where the chyle is poured into the subclavian, so that it found an easy exit, along with the reflux current of blood, which latter, as before suggested, passed, or a part of it, in a retrograde course, proving at once, the independent forces of the lacteal, and blood vessels,—modified in this instance, however, by an important law of hydrodynamics, which changed their physiological directions.

This last case, (not to name others), fruitful in physiological suggestions, shows that although life, in its popular or utilitarian sense, be completely extinct, several important sub-lives, in tissues, and several functions may survive for a considerable period, as manifested in the heat generating process, in muscular contractility, in capillary action, and in the chylous circulation. As the chylous force, so clearly proved in this case, goes to confirm, collaterally, a capillary force after general death, it may be proper to allude to the subject briefly.

Haller, the greatest physiologist of his era, ascribes the chyloferous circulation,—“first to the attraction of capillary vessels—next to the peristaltic contraction of the intestines, and, lastly, to the alternate compressing force of the diaphragm. The attraction fills the villi, and peristaltic force empties them, and moves the chyle farther forwards. The rest of the motion seems to depend on the force of the membrane of the lacteal vessel *itself*.” (Phys. DCCCLXVIII). Here, several forces, altogether imaginary or inadequate, are invoked to do almost nothing, while the lacteals, *themselves*, do all the hard work by an *inherent force*. The latter hypothesis is the most rational, or, at least the most simple, and is therefore preferable. Certain writers of the present day, without any physical proof, or analogy, assume the existence of *molecules*, which, as they further assume, perform all the functions of nutrition and capillary circulation. This is, to say the least of it, a very obscure and unedifying explanation for professed *physicists* to make. The science of physics has developed no mechanical law or force analogous to the chylous.

Müller candidly acknowledges, that the powers by which the lymph and chyle are moved, are unknown. Sir Astley Cooper, in his work on the Thoracic Duct, details experiments showing that ligation of that duct in dogs, was followed by a rupture of the receptaculum chyli, by large extravasations of chyle, and by death, in a few days. This receptacle, though weaker than the duct, “is,” as he affirms, “able to bear the pressure of a column of quicksilver more than two feet in height. The force, therefore, exerted by the absorbents must be greater than that of such a column; more especially when it is remembered that *living* parts will resist a force which would readily tear them

when *dead*. The contractile powers of the absorbents are proved to be very strong—sufficient to rupture their coats.” (12.)

Another use of the above experiments, which I do not wish to dwell upon, is this : they completely overturn, as I conceive, the theory of Sir David Barry, namely, that the sole cause of the venous circulation, is a vacuum in the chest, caused by the act of inspiration, the atmospheric pressure causing the blood to rush into the lungs.

Endosmosis or physical capillarity, (the physiological import of which has been already alluded to), like gravity, constantly acts upon the body and its organs ; but from its known laws, it appears not to be even an auxiliary to the chylous and capillary forces. In fact, it must from its *nature* oppose them in most instances. There is, however, a use of this doctrine which has been strangely neglected, and one which I do not now intend to point out at length, namely a use in pathological anatomy. As the vital force sinks, the endosmotic rises. Thus in a few minutes after death, *exosmose* is very apt to take place from the gall-bladder. As the bile has a peculiar color, it is easily detected in the surrounding tissues. In some dead bodies there is no *exosmose* from the gall-bladder, until the beginning of decomposition ; but in most bodies it begins soon after, nay, probably, *before* death—certain it is, that in ten minutes after the extinction of life, large tracts of the peritoneal, or subperitoneal tissues, are often infiltrated and discolored with bile that has been attracted or transmitted through the gall-bladder.

Hence, endosmose and exosmose are fundamental considerations, if not in the healthy state, they are without doubt, during, and soon after death. An example that occurred this day, (Nov. 27th), will illustrate the point. As others often refer to plants and frogs, I may be allowed to refer to crocodiles, a far more respectable animal. One of these having perished from cold, and I, not being able to complete the dissection on the first day, threw a wet cloth over the abdomen. The gall-bladder was much distended with bile, and was not wounded. But when I returned to the dissection, the cloth was green ; the whole of the bile had left the gall-bladder. The latter was shrunk almost to nothing.

Dr. Mitchell, of Virginia, from 1737 to 1741, dissected, in all, five yellow fever subjects. Some of these were kept only “two or three days” before examination ! (See Coxe’s Med. Mus.) One of these, a negress, served the illustrious Rush, as a “clue in practice :”—“the liver,” he says, “in the above mentioned slave, was turgid and plump on its outside, *but* on its *concave* surface, two thirds of it were of a deep *black color*, and round the gall-bladder it *seemed* to be mortified and corrupted.” (Inq. iii. 90). Now the “*clue*” to this “*black color*” and this “*seeming* mortification”, is found in the transudations of bile, and other changes consequent upon the delay in the autopsy ; at least, a different appearance would be extraordinary.

It would be difficult to show, in the whole circle of medical sciences, any branch more important than pathological anatomy, and yet is there a single work extant, which, to a greater or less extent is not based upon examinations vitiated by physical causes or changes, endosmosis being one ? Excepting a few diseases, as cancer, consumption, the lesions of which are as strong as those on the field of battle, what can

be learned of color, cohesion, congestion, vascularity, turgidity, effusions, infiltrations, &c., in acute diseases, as fevers, one, two, or three days after death? Magendie, the great vivisector, of Paris, thunders in the ears of his bookmaking colleagues, in this wise—"Are you not in truth convinced, that the lesions found at our autopsies are frequently produced *after* death, and that consequently the plan hitherto followed in such inquiries is *fallacious*, and can *only lead to vague information and error*?" [Lect. on the Blood].

In this paper, I have no wish, even had I space, to make any extended applications of these researches, to physiological and morbid conditions, although, deductions, fundamentally important, are involved in the same. The scalpel is truly the greatest, and most generally successful instrument of pathological research, yet made known. But how little does it reveal in some cases of acute fever, particularly in the south, where lesions, merely secondary, or tertiary, are infrequent? for in protracted fevers, positional lesions, doubtlessly, may form *internally*, as they do *externally*. Congestions of the lungs, of the cardia and great curvature of the stomach, of the mesenteric glands, of the mucous membranes &c, are sometimes, the result of position as much as bed sores. I was called twenty five miles, to see a young lady, [in Virginia,] affected with typhus, she refused all medicine; fell into a delirium for three weeks; the fever then declined, leaving a mortification of the back, from which she died three weeks after. Another young lady whom I attended daily for typhoid, during *one hundred days*, had, only a few days before death, the eruption of rose colored, lenticular spots, described by M. Louis.

There is a constant bias, in pathological anatomy, leading to the centre, as the stomach, intestines, or some other central organs exclusively for the explanation of the cause of death, while, the circumference scarcely falls within the radius of pathological vision. Observe, in the early stage of yellow fever, what red flushings or capillary engorgement discolours the conjunctiva, the skin of the face, neck, and breast. As death approaches, this often is replaced by yellowness. After death, in those cases, where capillary action is most energetic, these same parts, occasionally, become much more engorged than ever,—flushed, livid, mottled, marbled, cyanosed, nay black, apparently tumid,—and this too upon parts the most elevated. Now this is as much a morbid appearance, as it would be in the cerebral or abdominal centres. This appearance is of easy explanation, upon the theory of post mortem capillary circulation. The heart discharges no blood. The capillaries deliver their blood into the veins,—these fill, as well as those large subcutaneous capillaries, that had suffered by dilatation during the disease. It will be found, that as soon as the capillaries cease to act, the blood begins to gravitate; sometimes, subsiding like a water-line, or level; the higher portions of the skin as on the cheek, fore-head, tip of the nose, &c., become white. In cases where this capillary action is inconsiderable, and, in cases where it has ceased, the blood can be made to flow, and reflow, in a few seconds or minutes, by position, that is from side to side, by turning the body over; and, exactly, in those tissues where the engorgement was greatest during life, will the greatest degree of positional or gravitative engorgement be found after death. There is,

however, a variety of *post mortem cyanosis*, due to capilarity, a blueness of the face, neck, &c.

Capillary hyperæmias, particularly of organs beyond sight, occur, short of softening, induration, and other phenomena characterizing inflammation* in its extreme and concentrated conditions as met with in maladies less general. Now if the capillary circulation survives the general death, as it probably does, in some degree, in every case of fever, it is easy to understand, in acute hyperæmia, short of disorganization, how the capillaries, by filling up the great veins, emptying the arteries, and by altering the *præ-mortem* condition of their own vessels, may obliterate, almost all traces of some of the most fatal morbid conditions—for inequilibrium in the circulation, is as fatal as cancer, gangrene of the lungs, or aneurisms.

For reasons not necessary to explain, I beg leave to quote from Folio X. M S., and from the Western Journal of Medicine, some dates and statements : (see my paper, *Post-Mortem Researches*, in the Journal ; dated Jan. 1843. April No.).

November 13th, 1841 ;—at this date, I find the following statement : The capillary circulation in yellow fever, and other acute fevers, probably survives respiration, and the heart's action ; when it ceases, cadaveric hyperæmia takes place.

In perhaps one fourth of these dissections, the bodies were carried to the dissecting room a few minutes after death. The external veins, chiefly of the arms, and neck, sometimes became distended—these being opened, the blood often flowed in a good stream—sometimes, shot out a foot or more ; (that is, formed an arch having that diameter). In some cases, by putting a ligature upon the arm, or by grasping it above the elbow, the blood was made to flow more freely, and, by moving the muscles, as the patient is wont to do in ordinary blood letting, the blood shot out some distance. Punctures in the middle of the subclavian, discharged blood, which, arose in a full stream, against gravity, two or three inches forming, sometimes, an arch as it fell. The coronary veins of the heart, discharged blood rapidly, and with surprising force : I have observed within a few minutes after death, the flow of from twenty to thirty ounces of blood from the scalp, the head being raised :—Prominence of the veins, after death, is doubtlessly owing to the capillary circulation, which first fills the central, and then the external veins to excess : These dissections show conclusively the independent action of the capillaries. If capillary action exist independently of the heart, congestions &c, may disappear, and others form during or after death &c. Hæmorrhages after death &c. Post-mortem blood letting proves, that the heart's action is not necessary to the capillary circulation. The proofs are many, though they might have been greatly multiplied, had I taken a just view of this matter sooner—distention of the veins, copious hæmorrhages, crimsoned flushings of the face, passed for nothing—not supposing it possible that the veins could discharge more blood than what they contained at the moment of death, nor that capillary life survived the extinction of the general life, as manifested in sensation,

* Professor Gross, of Louisville, in his able work on Pathological Anatomy, thus speaks concerning the *seat* of inflammation : “that this is in the capillary vessels is a fact concerning which there exists no dispute.”—31.

respiration, systemic circulation &c. But, as soon as I had discovered the curious and complex laws of post-mortem contractility, the possibility of the doctrine of a post mortem, vital capillarity, flashed across my mind. (Various co-ordinating phenomena were subsequently noticed, particularly, post-mortem calorificity, the laws of which are so peculiar as to constitute a distinct branch of thermotics.) Are not hæmorrhages in yellow fever due to capillary congestion? &c. (Many other passages of similar date and import might be copied from the originals, which are open to any one).

From the *Western Journal*, I take the following extract, omitting the ten decided cases, each of which, proves the existence of the post-mortem capillary circulation: "The doctrine of the capillary circulation, as surviving that of the heart, and large arteries, renders tardy dissection a most fallacious guide in judging of venous congestions, vascular tumescence, organic reddenings, blanchings, &c." "I may be permitted to say, that the following facts prove that the direct action of the heart, and its indirect or suction power joined to the suction produced by respiration, are not necessary to the rapid motion of the venous blood. To say nothing of cadaveric injection and exudations, the transporting power of the capillaries might have deposited the blood in new situations, sponging it out from structures that had suffered during life from acute hyperæmia, engorging others that had been healthy, blanching the former, reddening the latter. At the right side of the heart, venous stagnation probably begins, its vessels being wholly passive. The capillary power is soon neutralized by an opposing, mechanical force, or ended by an entire cessation of vital action."

I make these extracts for the consideration of honest *Æsculapians*, (these are not few) who are willing to give every man his due. If any one has ever before established the independent action of the capillary circulation in the living or dead human subject, if any one has applied this discovery, a fruitful one, in explanation of pathology and morbid anatomy, I am ignorant of the fact.

The number of illustrative experiments accompanying this paper, might have been greatly increased. I fear that some of the most valuable have been omitted, owing to my want of leisure to search for them in ten *Folios*, (from *M S.* Vol. IX to XIX)—some of which contain nearly 1000 pages,—all of which are imperfectly indexed, in relation to the subject now submitted to the reader. Although, long ago, I promised—voluntarily, it must be confessed—to give this experimental sketch, yet, nothing in the way of composition and arrangement was done, until late in November; one portion having been written, while the other was at the printer's hands. Of arrangement there is little or none—the cases were copied, exactly as they were found, in turning over my *M S S.* I, therefore, have now, as upon former occasions, to ask indulgence for hurried papers incidental to more extended aims, and plans, which require much labor and time. But of this self imposed task, I have no right to complain. Happy, if I shall, in the smallest degree contribute to advance that noblest science, whose high behests require of its votaries to do all they can, to promote the well-being, the health, longevity, and euthanasia of mankind.

New Orleans, Decr. 1st, 1848.

ADDENDUM.

Since this paper was sent to the press, Cholera has appeared in New Orleans. Not wishing at present to enter into the pathology, nor morbid anatomy of this remarkable malady, I may, nevertheless, be allowed to remark, that the rôle of the capillary system is a fundamental consideration, a point of departure in the investigation of this disease. Intimately connected with capillary derangement, or collapsion, including physical alterations of the blood, is that most characteristic feature, in fully developed Cholera, namely, a loss of animal heat, at every accessible point of the body, as under the tongue, in the palm, bend of the arm, popliteal region, axilla, and so on. In the axilla, the heat is least depressed. The temperature, before, and after death, from Cholera, has been noted with exactitude, probably for the first time in the history of this plague. Whether algidity be regarded as the cause or the effect of capillary derangement, it is an element of the greatest value, characterizing the disease, and indicating its pathology. The heat, in the dead body, sometimes, mounts to $110\frac{1}{2}^{\circ}$, but, it reaches its culminating point slowly, often requiring from one to two hours or more. It often remains stationary, or very nearly so, for a considerable period, and, what is the most extraordinary of all, this heat fluctuates, rising—falling—rising again;—a fact which I have observed in Yellow Fever. A portion of a case noted this day, (Decr. 24th, 1848), will serve as an illustration; at about an hour and a half after death from Cholera, the body of H. P., reached its maximum; in the mean time, the thermometrical readings of the rectum, at intervals of five minutes, noted consecutively, were as follows; 108° , $106\frac{1}{2}^{\circ}$, $106\frac{1}{3}^{\circ}$, 108° , $108\frac{1}{2}^{\circ}$, 109° .—On ligating the arm, and opening a vein, in this subject, the stream arose at least six inches perpendicularly.

In cases wherein indications of post-mortem capillarity exist, these are tardy in developement. The veins of the arms are rarely, the jugulars frequently, filled. From the latter, I have taken in a few minutes, as in ordinary venesection, six or seven ounces of blood, having, as is usual in this disease, a black color, and a thick, glutinous consistence, forming a soft, sticky, adhesive clot, yielding in twenty four hours, only a slight quantity of serosity. The subclavians, and other large venous vessels of the centre, usually discharge, in a short period, sometimes, in half an hour, from four to five pounds of blood, similar to that already mentioned, flowing, not all at once, but in a gradual manner, indicative of capillary action. In confirmation of this, I could relate a number of experiments on Cholera subjects, made from one to six hours after death, upon the small veins, and arteries of the heart, the mesentery, omentum, and pia mater, raised, artificially, above the highest part of the body, showing, that after these vessels have been emptied, they refill, in opposition to gravity, the blood flowing in the direction peculiar to the living condition of that part of the vascular system in which it is found. These results are the more remarkable, when the quality, and the reduced quantity of the entire mass of the blood are considered.

Heat, contractility, and capillarity, do not always bear towards each other a definite ratio, nor even a coincidence. P. K., whose temperature before death was low, after death not very high, and who had no exter-

nal indicants of capillarity, at an hour and a half after death, spontaneously contracted his thumb and fore-finger, six or seven times, as if about to take a pinch of snuff; spasmodic twitchings took place, also, in the little finger. Do the arteries and the heart undergo spasmodic contractions and collapsions? Even before the general collapse, paroxysms of pulselessness occur, not only during the cramps of the muscles, but in the intervals. In grave cases, the pulse or radial artery, is inexpressibly small, a mere thread—probably not the one tenth of the size of that observed in Yellow Fever, or even in the dead body. It is probable that the left side of the heart, and the arteries, (the pulmonary arteries only excepted,) are nearly empty in the collapsed stage, owing to the general failure of the capillary circulation. Hence the shriveled or rather *withered* condition of the skin, and often of the lungs, while it may reasonably be supposed, that the capillaries of the soft, spongy, mucous membrane of the stomach and bowels, including the imperfectly valved lacteals, assume, either an inverted action, or yield to the pressure from the circumference, and, consequently, pour out the white blood, and sometimes, chyle, into the intestinal canal, producing what might be called an acute, serous hæmorrhage, or an apoplexy of that vastly extended cavity, and, thereby, in a great degree washing away, or preventing the absorption of, and, consequently, the efficacy belonging to, remedial agents. Without pretending to know anything of the *vera causa* of this epidemic, which is absolutely unknown, I would ask—Is not capillary collapsion of the surface, and capillary effusions of the centre, *Cholera itself*, the invariable antecedents of algidity, asphyxia, cramps, pulselessness, and death?

[For the New Orleans Medical and Surgical Journal.]

III.—COOPER'S WELL. *Remarks upon the virtues of its waters.*

The medical properties of this water, are cathartic or aperient according to quantity, diuretic, sudorific, tonic and alterative, all of which are possessed in a high degree. It is adapted to an extensive range of disease, chiefly chronic. The digestive, nutritive and secretory functions are brought directly into action from almost any point of derangement. As an alterative it is unequalled by any medicine, either simple or compound yet in use. The numerous class of diseases unmanageable in the hands of physicians, relieved by this water, recommend it eminently to the medical profession. And both the promptness and certainty of its action entitle it to the highest consideration of the invalid. *Dyspepsia* in all its forms and at any stage, *Chronic Diarrhæa*, utterly hopeless under medical treatment, dropsies and the multitude of affections subjected to alterative and tonic treatment, are controlled by its agency. The whole glandular system is powerfully aroused, including of course the biliary secretion. Its chalybeate properties enrich the blood, imparting tone and vigor to the system, and the quantity of carbonic acid gas with which the water is charged, is immediately perceptible from its proper exhilarating influences.

The numerous instances of recovery from obstinate disease that take place from the use of the water, will ultimately command the attention and immense patronage of the whole southern community of invalids, to whose diseases it is adapted.

Considerations of humanity require that this remarkable well, the discovery and early history of which indicate clearly the hand of Providence in its existence, be extensively known, and that it receives the early and efficient publicity of the press.

Although this water has been analysed, sometime since, yet with the view to its being made more generally known and to meet the wishes of many, a few gallons will be sent to Professor Riddell, whose well known scientific attainments, will give entire satisfaction and assurance in the correctness and precision of the analysis, that he may make. He will confer a public favor by giving to the Med. Journal of N. O. and to the Concordia Intelligencer, a tabular statement of its analysis.

Dug in a chasm, surrounded on all sides by bold and romantic hills, whose summits are covered with lofty and venerable pines, this well, at the great depth of one hundred and one feet, seventy five of which penetrate solid rock, sends forth its healing waters.

The value and destiny of this recently discovered watering place, now in comparative obscurity, are inestimable to the south. Independently of its own real and intrinsic virtue it will sustain itself, and become a great infirmary to our hemisphere.

It is situated in Hinds Co., Miss., about forty miles east of Vicksburg from which point it is reached by a Rail Road.

Waterproof, Ten. Parish, La.

Aug. 11th 1848.

WM. D. ANDERSON, M. D.

IV.—*Stricture of the Duodenum.* By J. C. ANDERSON, M. D. of *Eutaw, Alab.*

George, slave, aged 35 years: about the year 1835 was taken with pain in the Epigastric and right Hypochondriac regions, with other symptoms resembling dyspepsia for which disease he was treated by several physicians. Of the true history of the case I could ascertain but little; I was requested to make an autopsy on the 15th last month; I found the body very much emaciated, pericardium contained half an ounce of serum, heart healthy,—somewhat diminished in size; lungs natural, slight adhesions at the lower part of the right lung. On opening the abdomen, the stomach was found very much enlarged, and the Pyloric orifice obliterated by an enlargement of the duodenum. On tracing the duodenum, a stricture was found seated about two inches below the entrance of the Ductus Communis. On opening the duodenum above, and introducing the finger, a perfect *cul de sac* seemed to be formed. The stricture could with difficulty have received a goose quill. At the point of stricture, there was a great thickening of the muscular coat; width of stricture one $\frac{1}{2}$ inch—length $\frac{3}{4}$ inch, mucous coat of duodenum above the stricture and the contiguous two thirds of the stomach much thickened, with a corresponding thickening of the muscular coat." One or two bright patches of inflammation near the pylorus; cardiac portion of stomach nearly natural in appearance. Liver natural. Spleen very much diminished in size, firm—of deep bluish color. Small intestines healthy; slightly atrophied. Rectum and descending colon, much diminished in size, resembling a cord.

Eutaw, Ala. July 20th., 1848.

V.—*Effects of the Yellow Fever on the Nails.* By Dr. N. WALKLY, of Mobile, Al.

After recovering from a slight attack of yellow fever, in the fall of 1847, I observed an indentation at the root of each nail indicating the point at which their growth was suspended by the disease.

My curiosity was excited to know whether this was a constant effect of this disease and peculiar to it, or whether it was not an effect of other diseases, I examined and noted the appearance of the nails of the fingers in forty one cases of well marked cases of yellow fever occurring in my own practice and that of other physicians, and in every case, I found this distinctive mark which continued until it grew off the nail.

I also noticed a number of cases of bilious and vomiting fevers, occurring about the same time, and since I have not seen this mark left in any of these cases, except in one case of Typhoid fever occurring in February last in which a slight mark was visible on a part of the nails similar, but not so distinct as in the yellow fever cases. I have recently seen a case of scarlatina in which the growth of the nails was entirely suspended and came off at the time of the epidemic desquamation. I desire to call the attention of the Profession to this means of Diagnosis, (if it be a correct one). I hope that they will observe the cases that may come under their charge this season, and if it should prove an unvarying mark of this disease in their practice that they will make it public. It is common in epidemics of Yellow Fever for the ordinary fevers which are prevailing to run into or simulate the epidemic disease, and undoubtedly many of those cases in which persons are said to have had this disease two or more times, have been deceived and unnecessarily alarmed by an ordinary billious or remittent attack simulating the prevailing epidemic.

Mobile. Aug. 1848.

Part Second.

REVIEWS AND NOTICES OF NEW WORKS.

- 1.—*The nature and treatment of Deafness and diseases of the Ear ; and the treatment of the Deaf and Dumb.* By WILLIAM DUFTON, M. R. C. S. Philadel. Lea and Blanchard, 1848, pp. 120.

— — — *Sero medicina paratur
Cum mala per longas convaluere moras.*

The diseases of the ear, like those of the eye, are in some respects, special and peculiar to those organs ; hence it is that the general practitioner, for want of a better knowledge of these, so to speak, special affections, treats them, when they come under his care, upon general principles, and failing in this, as he needs most, in many instances, he either turns the case over as incurable, or continues his experiments until disease has totally destroyed the organ. Hence also the necessity of treatises on the diseases of these particular organs ; not however because the majority of their ailments differ from those of the other organs of the body, but because they require that attention to minutiae which few will be found willing to bestow. As much of our knowledge as well as pleasure is received through this sense, its preservation from disease and destruction is of no small consequence. The first chapter treats of *Acute and chronic inflammation of the ear* ; the 2d of *Diseases of the ear not purely inflammatory* ; the 3d, *on nervous diseases of the ear* ; the fourth, *on the treatment of the deaf and dumb, &c.* The diseases of this organ sometimes exist independent of any other affection ; again it may be secondarily affected in the progress of other diseases, —such as typhus, scarlatina, and some other of the eruptive, or exanthematous fevers.

In these latter cases, it does not usually attract much attention, since in the majority of cases the local affection subsides with, or soon after, the disappearance of the febrile disturbance.

Affections of the throat, such as tonsillitis, all forms of cyananche, from whatever cause, are exceedingly apt to affect the hearing, by involving the Eustachian tube.

The hearing is frequently surdified, and we have heard of a few persons in whom this sense has been permanently injured, and even destroyed, by large doses of the sulph. of quinine.

Diseases of the ear may be divided into external and internal; acute and chronic,—nervous and inflammatory.

Some of the acute diseases of the internal ear may, if neglected or improperly treated, end in death; the inflammation in some cases extends to the membranes and substance of the brain itself, producing delirium, coma and finally death.—These cases are usually attended with considerable febrile excitement, great—even intolerable pain in the organ, much restlessness and great cephalalgia. Such require depletion, both, general and local; purgatives and antiphlogistics. The practice of pouring stimulating lotions etc. into the meatus under such circumstances, deserves the severest condemnation of the profession. It is chiefly in the acute form of inflammation of the submucous cellular tissue of the tympanum, that constitutional symptoms manifest themselves.

The pain in these case, says our Author, is described as “pricking, —burning—tearing—boring and dragging.” In these instances the inflammation sometimes extends throughout the course of the Eustachian tube, reaching the pharynx and tonsils, and sometimes penetrates the mastoid cells.

It is in such cases that danger is to be apprehended, from extension to the brain.

Should the disease be permitted to progress uncontrolled, matter will form and escape through the meatus, discharging with it fragments of the small bones of the ear.—Dr. Dufton says, the matter is sometimes discharged through the Eustachian tube, provided it be not closed by adhesive inflammation, as is generally the case.—The abscess thus formed, discharges itself through a rupture of the membrana tympani, and then the violence of all the symptoms usually subsides. The discharge, however, in feeble and delicate constitutions, may induce hectic, with wasting and general atrophy and even death.

In the early stages, leeches and cups—the first within the *meatus* and the second, around and over the mastoid processes, aided, if necessary, or required, by venesection. Hot pediluvium—active cathartics, diaphoretics, may produce a resolution of the inflammation and a restoration to health.

Kramer, a German, who has written the best treatise on the diseases of the ear, recommends dropping a weak solution of the sugar of lead into the canal in these cases. Evaporating lotions, we think, might aid in subduing the excitement and inflammation.

Dr. Dufton lays down the following method by which to ascertain if the Eustachian tube be obstructed: “When deafness occurs after repeated attacks of inflammation of the tonsils, and if, on examining the external ear, no appearance of disease present itself, either in the meatus or in the membrana tympani, and if upon a forcible expiration, the mouth and nostrils being held, air is not driven into the ear, it may be strongly suspected that there is a closure of the Eustachian tube, which is more or less complete.”

To ascertain, whether the Eustachian tube be pervious or impervious, Dr. Kramer advises us, to place a small watch in the mouth, then close it, and if the patient distinctly recognizes the tick, we may infer from this fact, that the tube is unobstructed. When the Eustachian

canal is found to be impervious, or when it is even suspected, we can at one and the same time not only remove all doubt on the subject, but likewise, in many cases, remove the obstruction, by performing upon the patient, what now-a-days is called *Catheterism*. We must refer the student to the work for further instructions on this head; let him, however, begin, after furnishing himself with a properly shaped silver catheter (a very simple little instrument by the bye) and perform the operation on the dead subject, then upon his friends or himself, and he will readily acquire the requisite dexterity in the operation.—Through the catheter, which should be pervious, vapour, air or fluids may be thrown, by means of a small syringe adapted to the purpose, into the middle ear, and thus act medically upon the diseased parts.—This is called the “*Douche*”,—water-douche, vapour-douche, air-douche, etc., according to the article used.—Between Kramer, Itard, Pilcher and others, there exists some difference of opinion, as to the comparative merits of air, water, and vapour when applied by the *douche*.

The hearing may be seriously incommoded by inspissated *cerumen*, blocking up the passage; of course, every one knows, that cleanliness—the use of the syringe and tepid water will clear the passage and restore the hearing.

Our author, with others, recommends puncturing the *membrana tympani* when we have reason to believe matter has formed and accumulated behind it.—Sometimes nature relieves herself either through the Eustachian tube or by ulceration and perforation of the tympanum.

To illustrate several important principles laid down by writers on aural medicine, Dr. D. adduced a number of instructive cases, some of which we would be pleased to reproduce, had we space. The reader must consult the work itself. The auditory passages may be the seat of *herpetic* ulcerations and other morbid alterations, all of which must be treated according to the established principles which usually govern us in such cases.

A writer in a late number of the *London Lancet* accidentally discovered, that by introducing a small pledget of loose fine wool into the meatus and down to the *tympanum*, the hearing might be greatly benefited in cases of partial destruction, by ulceration or other means, of the tympanum. For this means to effect any good, the tympanum must not be entirely destroyed; in this event, it did not improve the hearing. As this treatment is of easy application, and the means always at hand, it should receive a fair trial. Foreign bodies sometimes find their way or are introduced into the auditory passages; suitable instruments must be employed for their removal, if the body becomes impacted, or is too large and heavy to be washed out by a syringe.

The *larvæ* of insects may be occasionally detected in the ear, and also the *entozoa*; the former may be expelled by filling the meatus with almond or sweet-oil; it has no such effect upon the latter.

Dr. Dufton devotes his 3d chapter to *nervous diseases of the ear*. When we consider the delicate structure of which the auditory apparatus is composed, its nervous filaments,—its nicely balanced parts,—the rude shocks to which this organ is exposed, not only from external causes, but from internal influences, we should rather be surprised to find it so rarely affected or destroyed.

Nervous diseases of the ear require no particular remarks from us, as they require much the same treatment as similar affections in other parts of the body.

The book concludes with interesting statistics on the *deaf and dumb*, for some of which we shall give place :

Dr. Dufton specifies the following as the diseases to which the causes of deafness have been ascribed in 59 Children : scarlet fever 7, measles 2, small-pox 1, inflammatory fever 15, typhus fever 7, severe accidents 3, fits 3, hooping-cough 2, disease of brain 2, frights 1, paralysis 1, inflammation of brain 3, teething 1, uncertain 12, making 59.

The greater number of them lost their hearing during the first and second years of existence.

We must here close a very desultory notice of a useful little work for reference. It is far less copious and elaborate than Kramer, Itard or Pilcher, still it is no less practical than any of them.

I.—*An Analytical compendium of the various branches of Medical Science for the use and examination of students.* By JOHN NEILL M. D. *Demonstrator of Anatomy in the University of Pennsylvania; Lecturer on Anatomy, etc*—and FRANCIS GURNEY SMITH, M. D., *Lecturer on Physiology, etc; Fellow of the College of Physicians, etc., etc.* Philadelphia: Lee & Blanchard. 1848.

This work embraces Anatomy, Physiology, Surgery, Obstetrics, Materia Medica and Therapeutics, Chemistry, and the Practice of Medicine, enough in all conscience, to make up a large library. The book treats of these different subjects separately, and each embraces an analytical view of all such questions as may be considered elementary. It aims to give a condensed account of every question touching the several branches of Medicine, and in this account, will arrest the attention of every candidate for his doctorate. It is not simply a work for the instruction of the noviciates, it may be consulted by the general practitioner with infinite advantage. It will recall to his recollection many important facts, and stimulate him to increased study, in the several branches of Medicine.

The different departments of which it treats are illustrated by handsome plates, and will serve to impress the mind of the student with clear and definite ideas on the various subjects comprehended in the work. It is, we think, an excellent book of the kind, and will no doubt become highly popular with the students throughout the United States. It enters somewhat extensively as a compendium, into some of the great physiological questions of the day, and embraces such facts as may be regarded as positively settled by the scientific world. Its preparation must have required great labor and patience on the part of the Authors, and we trust they will receive, as they deserve, a rich reward from the profession.

To the medical student it may be confidently recommended as well as to the general practitioner, whose constant occupation will not allow him the necessary leisure to read more elaborate and comprehensive works.

III.—*An account of some of the most important Diseases peculiar to women.* By ROBERT GOOCH, M. D., with illustrations—Second Edition. Philad. Barrington & Haswell. 1848.

This work has been before the Profession a number of years and stands, as it deserves, very high in the estimation of practical men. Gooch's opportunities for studying the diseases peculiar to females were of the very first kind and no one who has read his work will say that he has not turned them to the great advantage of the Profession. For sound practical good sense, for careful observation of the phenomena of disease, few have equalled, and none surpassed, our Author in his particular department. He may always be consulted with safety, if not with advantage, under the most trying circumstances that can surround the general practitioner. He never speculates, but in facts, and facts form the basis of all his reasonings. The book is perfectly original; not a compilation of other men's thoughts and reflections—nor does the Author aim to captivate the reader by setting forth any new theory on the nature of disease in a brilliant and dazzling style.

His manner is as simple as his matter is valuable, and no one can read the work without admiring the great, good sense of the Author.

But it is needless to say more of a book already appreciated by the Profession.

To the Publishers, through Mr. Steel of this city, we are indebted for the copy before us.

IV.—*A system of Clinical Medicine.* By ROBERT JAMES GRAVES, M. D. M. R. I. A. *Physician to Meath Hospital, Dublin, formerly Professor of Institutes of Medicine, and Honorary Corresponding Member of the Medical Societies of Berlin, Vienna, Hamburgh, Tübingen, Bruges, Montreal, etc., etc.* With notes and a series of Lectures, By W. W. GERHARD, M. D., *Lecturer on Clinical Medicine to University of Penn., one of the Physicians to Pennsylvania Hospital, etc., etc., etc.* Third American Edition. Philadelphia, Barrington & Haswell, 1848.

In this practical age, "Clinical Medicine" is among many the most popular, because it treats, or rather should, treat of something tangible, appreciable, useful; appealing at once to the senses and thereby stamping indelible impressions upon the mind. These lectures were delivered *ex tempore*, and are therefore the expression of honest convictions, uttered at the bed-side, and in all probability beyond the influence of theoretical tendencies.—Indeed the Dublin faculty are notoriously practical men and hence the great value attached to the writings of some of her medical men.

Among the most eminent of these may be named Stokes and Graves, who have labored for years to advance our knowledge of practical medicine. As these "lectures" have been before the Profession in this country for several years, we regard it as superfluous to attempt any extended notice of the work. From these lectures however, we shall make a few extracts, touching such subjects as may be considered interesting to the profession at this particular time, or in this locality.

As the Asiatic Cholera is exciting a good deal of speculation and inquiry as to its nature and treatment, and as in all probability it will soon be among us, we shall make no other apology for the following extracts on this subject from the work before us.

After speaking of the efficacy of sugar of lead in some forms of diarrhoea and other bowel complaints, Dr. Graves remarks—"With these impressions I came to the resolution of trying the acetate of lead in the next case of *Cholera* which offered a chance of deriving benefit from any kind of treatment. It is known that there are some cases in which the disease at once assumes so frightful a malignity, that the patient is lost from the very moment of his seizure. This hopeless and intractable malignity is not peculiar to *Cholera*; it is seen in fever, scarlatina, croup, measles, and hydrocephalus; in fact there are certain forms of all diseases in which the best directed efforts of medical skill not only fail in curing the disease, but even in retarding its progress. But there are cases of *Cholera* where the patient is not struck down at once, when the disease is not developed at once in all its awful intensity and when time, brief though the space may be, is allowed for the play of therapeutic agencies. It is in such cases when the *acetate of lead* may be given with some prospect of success, and it is by such cases alone, and not by those which are necessarily fatal *ab initio*, that its value is to be tested"). As an argument against the use of calomel in *Cholera*, Dr. Graves remarks,—("The liver ceases to secrete, not only in consequence of the injury done to its vitality by the proximate cause of the *Cholera*, whatever that may be, but also from a mechanical cause, namely, from a diminution in its supply of blood.

In a case of *Cholera*, when the capillary vessels of the intestinal canal from the stomach to the rectum are actively engaged in taking up the serum from the whole mass of blood, and pouring it into the cavity of the digestive tube, there is an enormous drainage from the system, and there must be, consequently, a deficiency of blood somewhere.") For these, as well as other reasons, deducible from the foregoing, theoretical as well as practical observations, Dr. G. condemns in unqualified terms, the use of calomel in *Cholera*. He says ("with regard to the quantity of acetate of lead which may be given in this disease, and the mode of administering it, a few words are necessary. The mode in which I prescribed it was this; a scruple of the acetate of lead, combined with a grain of opium, was divided into twelve pills, and of these, one was given every half hour, until the rice-water discharges from the stomach and rectum began to diminish. In all cases where medicine promised any chance of relief, this remedy was attended with the very best effects.") According to Dr. Graves' experience, it checked the vomiting and arrested in a short time the serous or rice-water discharges.

To arrest the vomiting and control the serous evacuations, in this disease is of the first importance, and the experiments with the acetate of lead in the hands of Dr. Graves would lead one to place great reliance upon this article in this remarkable disease.

At the instance of Dr. Gomany, medical gentlemen of Dublin were induced to give the opium and lead treatment a fair trial, and almost all concurred with him in the efficacy of these two articles.

The testimony of these gentlemen is too respectable and decided to be called in question, yet we are free to confess that, although in particular instances, doubtless the lead and opium treatment might aid in checking the violent and urgent symptoms of the disease, yet we should prefer some other combination as a standard prescription. From various communications, recently published in the Medical Journals on the character and treatment of the form of Cholera, now prevailing on the other side of the Atlantic, we find the great majority of practitioners recommend and practice blood-letting in the early stages of the disease, especially in persons of robust constitutions and plethoric habit, with the very best effects. Theoretical reasoning alone would lead us to anticipate good results from the abstraction of blood, and happily for humanity, the practice of those who have given it a trial, confirms the correctness of the theory. If we regarded the evacuations in this disease as *hemorrhagic* (and why not?, since the serum is drawn directly from the blood and other circulating fluids), our prejudices would readily melt away before the omnipotent term *hemorrhage*, and the lancet might be frequently brought into requisition with advantage; whereas the fear of depleting a subject, clearly laboring under wasting evacuations, no doubt deters many from a discriminating use of venesection. However the case, &c., should determine the treatment to be adopted.

In the early stages of the disease, before the system has become completely prostrated and when the stomach and bowels still respond to reapeutic agents, we should place much confidence in a combination of opium, calomel and cayenne; or the former and the latter combined with camphor or the sulphate of quinine. From some knowledge of the nature of the disease, derived from reading the Journals both of this and other countries, as also from the known action of the above mentioned articles, we think they might be so combined and varied as to control the disease, at least in some of its stages. During the prevalence of the Cholera in this country in 1832, many practitioners placed their main dependance in large doses of calomel; of course this practice succeeded in a few cases and hence the calomel treatment was hailed by its advocates as successful; forgetting that some cases will recover under, and almost in spite of, any treatment; they made the exceptions establish the rule, hence their false deductions. We have seen as much as *one ounce* of calomel administered at a dose, in a case of Cholera, and that too in an aged and delicate female; strange to say, she recovered *malgré le traitement*.

In a work like the present, it were superfluous to follow the author through the various subjects of which he treats in these lectures; digressions mark almost every page and no attempt is made to deliver a connected series of observations on practical medicine; yet every sentence embraces sound practical good sense and useful information. Few men have observed more correctly and accurately than Dr. Graves, and none understands better the difficulties with which the practitioner has to contend.

The Clinical Lectures by Dr. Gerhard, are full of that useful species of information for which the author is so celebrated. We have witnessed, in times of yore, his devotion to clinical medicine and pathological science, and we have unbounded confidence in almost every thing that

falls from his pen or his lips—His lectures must enhance materially the value of the work.

We are indebted to the publishers, through J. B. Steel of this city, for a copy of the work.

V.—*An Inquiry into the Degree of Certainty in Medicine; and into the Nature and extent of its power over Disease.* By ELISHA BARTLETT, M. D., *Professor in Transylvania University. etc, etc.* Philadelphia, Lea & Blanchard, 1848.

Since medicine began to assume the character of a science, there have not been wanting those who attempted to decry its claims and to ridicule its pretensions; partly because they were totally ignorant of its first principles and therefore incompetent to appreciate its practical value, and partly on account of the acknowledged uncertainty which must ever obtain in a science that professes to deal with an ever changing and easily affected organism. The poet, with more sarcasm than charity, has styled the Doctor, the "*minister of death*,"—declares he cures by conjecture, and is unfit to be trusted. We forgive all such, they know not what they say. One of the chief beauties and charms of the profession, is what is called its uncertainty, without this, all inquiry into its mysteries would be at an end, and the mind of the medical philosopher would be bounded on all sides by a circle beyond which it could not, nor would not desire to travel. Indeed were medicine absolutely certain in all its appointments, in the application of remedies for the cure of disease, we should violate that beautiful and conservative law which condemns our bodies to death and decomposition.

Again, we contend that the *principles* of our science are as true as those of almost any other; it is in the application of these principles to particular cases, that this uncertainty is made manifest. This must ever be from the nature of the case. If the constitution of every individual were precisely identical in all respects, affected in the same manner and to the same extent by the same agents, then indeed, the application of our principles to individual cases, as well as to a multitude, would produce certain and uniform results. If these things obtained in medicine, we might in the facetious language of an old writer, treat and cure the case according to the squares of the constitution.

Dr. Bartlett's inquiry opens with the following declaration, to the truth of which we are not prepared to assent :

"I am stating", says he, "only what every body knows to be true, when I say that the general confidence which has heretofore existed in the science and art of medicine, as this science has been studied, and as this art has been practised, has within the last few years been violently shaken and disturbed, and is now greatly lessened and impaired. The hold which medicine has so long had upon the popular mind is loosened; there is a wide spread scepticism as to its power of curing diseases, and men are every where to be found who deny its pretensions as a science, and reject the benefits and blessings which it proffers them as an art."

Individually, we are by no means willing to admit that the public mind has recently become more skeptical in regard to the efficacy and power of our art in controlling and curing disease than in times past ; on the contrary, the very reverse seems to be the case ; medicine both as a science and an art is not only entitled at the present day, to more respect and confidence, from the public generally, but it may, we think, be readily demonstrated that such is indeed the fact, and we are not a little surprised that the author of the "*Medical Philosophy*" should have taken this view of the subject. If faith in our art be the offspring of intelligence, it will hardly be denied that the public mind is more enlightened and better informed at the present day than at any preceding age. It is true that quackery and charlatanry with all its kindred brood, find subjects upon which to fix their vampire gripe, yet this is in itself no trifling evidence that mankind, generally poor judges of the qualifications of the man of science, still look with hope and confidence to the pharmacopia for that relief from their pains and aches, which they seek in vain from other sources.

The success of the nostrum trade and of quacks does not argue any want of confidence on the part of the public in the certainty of medicine, it only demonstrates the credulity of mankind, and illustrates the important fact that by promising a certain and speedy cure the invalid is entrapped.

Dr. Bartlett, after alluding to the science of anatomy as so certain and well established as to require nothing to be said in its defence, glances at the department of physiology, which he admits is, as yet, far from being fully understood, although the functions and powers of many of the organs of the body have been positively determined.

"The charges against our science", says Professor Bartlett, "are that it deceives itself in this matter, (pathology and therapeutics,) that its pretensions are either altogether false, or greatly exaggerated ; that its knowledge of disease is vastly less than it professes to be, and especially that its power of curing and of mitigating disease, has been immensely over-estimated and over-stated. These charges some times deny altogether the existence of this power, although in most instances, they content themselves with the allegation that it is very limited in extent, and very uncertain in its application."

It cannot be denied that these charges, as stated by Dr. B., are to some extent true, and we fear that they may hold good for some time to come ; unless, indeed, the human intellect should receive endowments higher from the great source of all wisdom and goodness. To show the degree of certainty to which we have attained in diagnosis, prognosis, therapeutics, etc., etc., in medicine, our author selects out of a number of diseases as illustrative of his position, the one designated *pneumonia*; not only because it prevails to some extent in all climates and at all seasons, but if suffered to run on unchecked, will prove fatal in the great majority of cases.

With our present means of diagnosis, we can pretty well decide as to the nature, seat, and extent of inflammation of the lungs ; and the treatment for this affection, if judiciously applied and promptly enforced, will rarely fail to arrest or at least modify favourably, the progress of the inflammation. So far we agree with Dr. Bartlett, that our science is entitled to much confidence, because of the degree of certainty to be

attached to our remedies in the cure of pneumonia. Even in that intractable disease, consumption, our art, if it does not, unfortunately, enable us to cure it, with how much certainty, (alas! too true,) can we detect tubercles, cavities, and indeed all that train of morbid changes which will sooner or later conduct the unsuspecting victim to the tomb! Shall it be said that it is nothing to predict with almost unerring certainty, the fatal progress of a consumption, to be able to convince the patient that, "*Lethalis arundo hæc sit lateri*," that the sands of life are waning, that the heart is stricken with an incurable disease; that the walls thereof are either abnormally thickened or attenuated, with or without dilatation; that the valves are ossified, causing regurgitation, with dyspnœa, &c.; that the pericardium is a sac of water, and finally, that some of the great deep-seated vessels are affected with an irremediable aneurism.

These, with many more, are a few of the proud triumphs of our noble science—of our glorious art; and are not these things sufficient to stamp medicine with a high degree of certainty? If the principles of our profession are rightly and skilfully applied, results will be obtained scarcely less certain and positive, than those from the application of the principles of any other science to the affairs of practical life. As well might we call in question the principles of mathematics, because the geometrician should fail to demonstrate a proposition from assuming false premises; or doubt the science of astronomy, because the astronomer chanced to blunder in his calculation. Dr. Bartlett has omitted to state many strong points in this essay, and we fear his remarks although well meant, will not dissipate the doubts of the skeptical, or confirm the impressions of the wavering, in the certainty of medicine. Our author has entered upon an extensive field, a field co-extensive with mankind; he has but gleaned a few facts, gathered a few flowers, a little fruit, and then retired, leaving those who follow him, dissatisfied, disappointed. Volumes might be written on the subject—a world of facts might be adduced in favor of the certainty of medical science.

We shall close our remarks by quoting from the author, the following philosophic rhapsody:

"With these convictions of the powers and capabilities of our art, and of the general worthiness of its practitioners, we may rest assured, if we are only true to ourselves and to it, that the regard in which it has been held since the days of Hippocrates is in no danger of being permanently withdrawn. We must needs be visited occasionally by medical as by manifold other delusions; but it is a part of their nature always to pass rapidly away and to be soon forgotten. They are like fluttering eddies that cross the main current of the Mississippi or the Amazon; to him who happens to be caught in the tiny whirlpools, they may seem like the majestic tide of the great river itself, but they are soon inevitably lost and swallowed up in the rush of its resistless waters, to appear and to be seen no more. No, there is no danger. The work of two thousand years is not to be demolished by the noisy clamor of a few penny trumpets. As certainly as there is truth in the foregoing inquiry, will the present feeling of distrust towards our science and our art pass away. The ancient confidence will be restored; the old love will come back again, truer and deeper for the transient and passing estrangement. The constellations themselves, Orion and Pleiades, are sometimes apparently blotted out from the heavens, by the gorgeous glare of rockets and other artificial fireworks, kindled with sulphurous and nitrous compounds; but, courage my

friends, and a little patience, the show will be soon be over ; the parti-colored flame that would rival and eclipse the planets is even now dying away ; all that will remain of the blazing illumination will be some noisome gases in the atmosphere, and a few burnt out sticks on the ground ; but lo ! still looking down upon us, with their dear old smile of affectionate recognition, from their blue depths in the firmament, undimmed in their brightness and unchangeable in their beauty, the everlasting stars."

VI. *On the Pathology of Congenital Dislocation of the Head of the Femur upon the dorsum of the Ilium.* By JOHN MURRAY CARNOCHAN, M.D., etc. With Plates. (Communicated for the New-York Journal of Medicine.) New-York, Langley, 1848.

Fourteen pages are occupied with a description of this rather uncommon and anomalous affection. Diseases and injuries of the hip-joint are among the most serious, with which the surgeon has to contend—serious, because, in the first place, they are difficultly diagnosed, and in the second, because, if neglected or mismanaged, disastrous consequences are entailed upon the patient.

The author of this monograph, while engaged in the dissecting rooms of the *Ecole pratique* of Paris, had the good fortune to meet with three cases of congenital dislocation of the coxo-femoral joint ; he very wisely availed himself of his bon fortune, and the result is given in the pamphlet before us.

"The subject from which the following description is taken, was a female, says our author, between 60 and 70 years of age. Great disproportion was observed between the size of the trunk and the length of the inferior extremities, owing, says the narrator, to the ascent of the heads of the femurs, upon the dorsa of the iliac bones."

"The head, thorax, and abdomen, present nothing abnormal while the body is in a recumbent position, but being placed in an erect attitude, the superior extremities appear too long for the trunk ; the trunk itself appearing as it were wedged and sunk between the upper portion of the femurs. The thorax and abdomen, are thrown forward with a salient anterior curve, while the region of the loins, and the lower part of the dorsal vertebræ, present a curvature with a corresponding concavity. In this latter position, also, the anterior superior spinous process of the ilia, are thrown forwards, while the lower part of the sacrum and coccyx take a direction upwards and backwards ; the base of the sacrum being consequently tilted forwards and downwards. The cristæ of the iliac bones are almost vertical, and the trochanters can be felt standing out at a right angle to the external surface of the dorsum, nearly on a level with the posterior portion of the crests, and forming the apex of a conoid eminence, the base of which is at the ilium itself. The heads of both femurs enjoy free motion upon the dorsa of the iliac bones, and, while the trochanters can be pushed upwards and outwards, so as to be within half an inch of the crests of the ilia, when traction in the opposite direction is exerted, they are felt to descend through a space of nearly two inches, and become nearly on a level with a direct line drawn from the depressed anterior and superior spinous processes. The pelvis itself, with the exception of the unusual inclination of its axes, and the unnatural projection caused posteriorly by the abnormal position of the trochanters, appears to be, in other respects, tolerably well formed ; the transverse diameter being, however, somewhat increased at the expense of the antero-posterior, but neither as regards the pelvis, nor the other osseous parts of the extremities or trunk, are there to be seen any evidences of a rachitic disposition. Between the anterior and spinous processes, there is a distance of

eight inches ; between the anterior superior spinous process and the tuberosity of the pubes, four inches and a half ; while between the tuberosities of the ischia there is a space of five inches and three quarters, showing a considerably exaggerated diameter in this direction. The nearly equilateral triangle observed in the natural position of the parts, between the top of the trochanter, the tuberosity of the pubes, and the anterior superior spinous process of the ilium, is altogether changed, the measurements between these several points differing materially according as the head of the femur is pushed upwards, or drawn downwards upon the dorsum of the ilium. The play of motion which the head of the femur must have had in the act of progression during life, may be said to have been about two inches, as that extent of change of position of the head of the bone can be obtained, by an alternating force applied to its shaft, of traction downwards, or propulsion upwards. In the attempt to rotate, or move in different directions, the inferior extremity, a sensation of crepitus is felt, analogous to that produced by effusion or thickening of the ligamentous structure about an articulation, and the various motions can be effected with less facility than is generally observed in a healthy joint. Adduction and the action of flexing the thigh upon the pelvis, is less impeded than the other motions, but abduction, and extension, are permitted to a very limited extent."

On dissection the muscular tissue was found to be much changed ; those about the joint were pale and soft, and the muscular structure had degenerated into a yellowish fatty tissue. The displacement of the head of the femur and its lodgement upon the ilium, caused, of course, a corresponding modification in the course and position of the muscles about the joint. These changes are all pointed out with much anatomical precision and clearness, by the author ; we can not enter into details. After describing these changes of the muscular structure, Dr. C. adverts to the vessels, and describes the *arteries* as being much smaller than natural, and remarkably tortuous in their distribution, not unlike the flexuosities of the vessels of the impregnated uterus. The *veins*, corresponding to the arteries, were rather larger than usual, he thinks, and unlike these latter, they took their usual course and distribution,—presenting no flexuosities like those of the arteries. —

No change was remarked in the nervous centers, only they were below the ordinary size. In the corpus callosum and near the corpus dentatum, small spots of blood, the result of slight effusion, were observed.

Dr. Carnochan gives the subjoined description of the anatomical changes which have taken place in the coxo-femoral articulations of these two cases :

"It is not easy to describe by words the complex changes which the articular apparatus of the joint has undergone, but by a reference to the accompanying diagram, the description of the parts, as disclosed by dissection, will be better understood. Upon each side, the coxo-femoral articulation has become, as it were, furnished with two capsular ligaments, one which is the remains of the ancient capsule, and the other which is of more recent formation, and in which the head of the femur has been lodged since its escape from the original capsule. The old capsule, retaining its attachments at the margin of the acetabulum, has changed its usual direction, and is stretched upwards and outwards, in proportion as the head of the femur has mounted upon the dorsum illi. The head of the bone becoming independent of the acetabulum, and placed during the erect posture of the subject at the upper portion of the interior of the natural capsule, would naturally ascend and descend, during the act of locomotion, and remain within the original capsule until, in the progress of time, further changes occur. Owing to the weight of the body being now thrown

chiefly on the capsular ligament, and the consequent pressure of the head of the femur against its upper portion, particularly during progression, it is reasonable to suppose that this part more especially, of the capsular ligament, would be subjected to material changes. This apparently has been the case. The head of the femur acting from below upon the superior and inner portion of the old capsule, while the weight of the body afforded the counter-resistance from above, this portion of the capsule, thus situated between the internal part of the head of the os femoris and the dorsum of the ilium, being continually subjected during the actions of locomotion to the influences of powerful traction and pressure between two hard bodies, gives way, either by rupture, or more probably has become; as it were, worn through (usé), by a species of progressive absorption. The perforation of the capsule being now accomplished, and the weight of the body still continuing to be thrown upon the inferior extremities, the head of the os femoris pressing in one direction and the superincumbent weight of the body in another, escapes at length through this perforation, and now becomes situated upon the dorsum of the ilium, in contact, superiorly and externally, with the deep surface of the glutæus minimus, and inferiorly with the upper surface of the old capsule; an annular portion of which remains attached around, and thus encircles the cervix femoris.

As happens in traumatic dislocations, the formation of a new capsule has followed the escape of the head of the femur, from the confines of its original receptacle. This new capsule which entirely surrounds the head of the femur in its new position, is of a fibroligamentous character, mixed in some points with a considerable quantity of yellowish, dense, adipose structure. It is inelastic, but from the length of its fibres allows a free motion of the bone in every direction after the muscles have been removed. Internally, it is attached to the margin of an oval depression situated on the dorsum of the ilium in front of the ischiatic notch; externally, it has formed adhesions with the annular perforation in the ancient capsule through which the head of the bone has passed. The external surface of the capsule is generally surrounded by a celluloadipose tissue, but above, it is confounded with the fibrous structure of the deep surface of the glutæus minimus, and below, with the outer and superior part of the original capsule. The internal surface of the new capsular ligament is smooth and shining, studded in various points with numerous pediculated granular bodies of a yellowish adipose appearance, and at the lower and external portion, may be observed the perforation already mentioned, and which is seen to allow a free communication between the ancient capsule and that of more recent formation. Within the cavities of both capsules, a considerable quantity of sinovial liquid is found, which most probably has been secreted from the lining membrane of the original capsule.

The ligamentum teres, most likely after being ruptured or worn through, on account of the stretching it must have undergone by the displacement of the parts, has entirely disappeared, leaving no trace of its existence behind it, either at its attachment to the head of the femur, or at the bottom of the cotyloid cavity.

The head of the femur has changed its character in regard to its size, shape and consistence. Its dimensions are much smaller than natural, being at least one-third below the natural standard. The aspect upon which the round ligament is generally attached, is flattened and seems either to have never been developed in this direction, or to have been absorbed, or as it were worn down by the friction to which it must have been subjected upon the dorsum of the ilium during progression. This flattened aspect presents superficially a thin lamina of bone, which is entirely denuded of articular cartilage, and on the surface of which are seen numerous irregular elevations and depressions perforated with small foramina, as if the subjacent cancellated tissue had been encroached upon. The osseous tissue of what remains of the head of the femur is entirely altered. Instead of the spherical form and resisting tissue of the adult normal bone, the head of the femur

in the subject before us presents an unshapely, stunted appearance, covered externally by a thin, friable osseous shell, which lies upon a spongy, cellular tissue of bone, so soft as to be easily crushed between the fingers, and incapable of resisting the pressure of a common writing quill. The articular cartilage exists, but in a very limited extent, and is seen to end abruptly in a greyish undulating line, leaving that portion of the head in contact with the dorsum of the ilium, as has already been mentioned, entirely bare. The neck of the femur is shorter and more slender than natural, and forms a right angle with the shaft of the bone, which arrangement, however, may in part be accounted for by the advanced age of the patient. The ancient acetabula present changes no less remarkable than the corresponding heads of the femurs which have escaped from them. Instead of the round cup-like cavity, there is seen a triangular depression, not more than half an inch in depth, and two and three quarter inches in perimeter. The upper and inferior borders of this triangle are nearly equal in length, but the inner is shorter and presents an excuvation which is probably the remains of the notch, on the margin of the acetabulum, for the passage of the articular vessels into the joint. This notch is crossed by a transverse ligament which completes the border of the cavity, but the cotyloid ligament whose use in the natural state of the parts, is to deepen the acetabulum, exists but in a rudimentary degree. To the margin of the acetabulum thus constituted, is attached the pelvic portion of the old capsular ligament. The capsular ligament in its passage across the cotyloid cavity to be attached to the cervix femoris becomes immediately connected with the cellulo-adipose tissue which fills up that depression, still by reason of its distinct fibrous structure it can be distinctly traced to its attachments at the margin of the acetabulum. The bottom of the acetabulum is entirely ossified, showing no signs of an *arrêt de développement*. There is no remains of the *ligamentum teres*, and what fills up the cavity may be said to have a resemblance to the fatty mass usually situated at the bottom of the acetabulum, and which has received the name of the glands of Havers."

Our author then compares the pelves of these cases with that of a denuded one from a well formed female. The upper straits were found to be below the usual measurement, both in the transverse and antero-posterior diameters. In a healthy pelvis, the transverse diameter of the pelvis on examination was found to be $4\frac{1}{4}$ inches, while in the unnatural specimen it was $4\frac{1}{8}$ inches: the antero-posterior diameter in the healthy specimen was $3\frac{3}{8}$ inches, in the other instance it was only $2\frac{3}{4}$ inches." Still greater changes were observed in the inferior strait.

"Between the apex of the coccyx and the arch of the pubes, the measurement is $1\frac{7}{8}$ inches, against 4 inches in the healthy pelvis: while the distance between the tuberosities of the ischia, making up, as it were, for the small antero-posterior diameter, is $5\frac{3}{8}$ inches, the healthy pelvis in this direction being only $4\frac{1}{4}$ inches."

We have noticed these morbid deviations of the human skeleton more to gratify the curious than with any hope to advance practical medicine. Such cases deserve a place in the archives of natural science, because, fortunately, they are rare. The reports are well drawn up and the description most minute and, no doubt, as accurate as possible.

Part Third.

EXCERPTA.

1.—*Prophylactic indications to be observed against the Asiatic Cholera.*

At the time when Asiatic Cholera seems on the point of reaching us, we deem it our duty to make known to our readers an interesting report on the prophylactic indications to be observed against this terrible malady, a report of which the Belgian Academy of Medicine is now discussing the results. This report suggests painful reflections. In effect, while around us, in Germany, in England, in Belgium, everywhere, the public mind is pre-occupied with the coming of the Scourge, the French Government alone seems content to remain quiet and inactive. The following is the summary of the results of the report.

1.—*To improve the sanitary condition.* 1st, In the ignorance which exists of the virtual or efficient cause of epidemic Cholera Morbus, to endeavor to combat effectually the auxiliary or predisposing causes, which singularly favor its invasion, and influence so powerfully its propagation, its malignity, its treatment and its termination.

2. To observe carefully the preservation of the public health; to seek by all disposable means to destroy, to correct or at least to diminish the causes of insalubrity, by placing the poor and laborious classes in physical conditions similar to those in which independent persons are generally found.

3. To provide for the healthfulness of cities and of the country, as well as for the improvement of the condition of the indigent and for their education.

4. To direct Magistrates to neglect nothing for the removal of whatever may favor the development of the disease.

5. To take care to preserve the greatest cleanliness in inhabited places, in dwellings, and of person and clothing.

6. In the imminence of the disease to have the streets, lanes, crossings, public places, markets &c, in cities and towns, and villages, swept frequently, not only during the day, but even at night also, and never to suffer dirt, filth, sweepings, excrement, animal and vegetable matter in a state of putrefaction to remain there.

7. To facilitate the flowing off of rain water, slops &c, falling on the surface of the ground, and to permit no where near habitations, the formation of muddy and putrid pools.

8. To cleanse the sewers, ditches, ponds, canals, flax ponds, dung heaps &c, during the winter or at the commencement of spring.

9. To arrange the perimeter of marshes, ditches, and ponds, and even of rivers with a gentle current, whose beds are partly exposed during the summer, so that their waters may be constantly elevated and may keep the banks submerged unless circumstances may not permit them to be cleansed or desiccated before the appearance of the epidemic.

10. If the disease makes its appearance in any particular locality, to defer or to forbid the cleansing or desiccation of the surrounding stagnant water,

unless imperious necessity compels a different course; to prevent at that time even fishing in the lakes and ponds when it cannot be done without first drawing off the water, and exposing the putrescible mud of their bottoms.

11. To cause the inspection and cleansing of wells, cisterns, fountains, pumps, and watering places.

12. Public and private privies, when it may be requisite, should be examined and emptied; and those of them should be closed up whose flagrant insalubrity is irremediable for the want of a regular drain, or other means suitable to insure their innocence.

13. To exercise a special and constant superintendence over all public institutions, where large masses of people are assembled together, such as Theatres, Barracks, Prisons, Hospitals, Schools, Colleges, Universities, as well as workshops, and factories, in order to maintain in them the most perfect salubrity, particularly those which are reputed to be insalubrious, likely to compromise the public health by being badly kept, and by the noxious vapors which they diffuse, regarding private rights, however, as far as possible.

14. To pursue the same course towards slaughter houses, butcheries, cemetries, lumber yards, warehouses, shops, cellars, granaries, stables, stalls, furnished rooms, houses occupied by poor families, ragmen, gut-spinners, cattle-merchants, persons who raise hogs, chickens, rabbits, &c, places in which the air is often impure, contaminated, and imperfectly renewed.

15. To sprinkle chlorides frequently in necessities and water closets, in kitchen sinks, in the sewers of slop water, in places where large numbers of persons assemble, in butcheries, slaughter houses, fish markets, in dead chambers, &c, in short, wherever noxious emanations are formed.

16. In situations other than those enumerated, (15) the best and the principal remedy is a free circulation and renewal of pure air.

17. The habitations of the poor should always be kept clean, and rendered healthy; they should guard against the crowding of lodgers, and where it exists, it should be speedily obviated.

18. At the approach of the disease, to disperse a part of the population which is crowded in narrow and unhealthy habitations, by procuring for them spacious lodgings, well aired and ventilated, until the danger is past.

19. Houses which have been recently invaded by the overflowing of rivers should not be inhabited until after they have been perfectly dried and purified throughout.

20. To furnish necessary supplies, and to secure subsistence.

21. To cause to be inspected, and approved by experts, the condition of articles of food offered for sale, in all places whatsoever, to prohibit by severe penalties all those which are of doubtful or bad quality, as well as those which have marks of alteration or adulterations. Unripe fruits, vegetables, the flesh of swine, salted, smoked or dried meats,* pickled fish, melons, cucumbers &c, demand above all especial attention on the part of the sanitary police.

22. To recommend temperance and sobriety, and the observance of a nourishing regimen, not exclusively vegetable, but composed of animal and vegetable substances.

23. To remind the municipal authorities of the principal duties prescribed by law with regard to food and drinks.

24. To warn the people, by every possible means of publicity, that an improper regimen and acts of intemperance frequently provoke the developement of the epidemic cholera, and that the use of indigestible food, excess in eating and drinking, the abuse of alcoholic liquors, drunkenness, incontinence, the use of

**Viandes faisandées*, are the words of the original which I have translated *dried meats*. They mean meats which have been kept until they have acquired a taste of venison; or as an American would say, until they have become tainted, and would no longer require the practiced nose of an inspector to condemn them as articles of food.

ices and sherbets, the imbibing of very cold draughts, are also so many causes likely to produce the disease.

25. To persuade the people to abstain from the use of any remedy whatever, preservative or curative, without the advice and consent of a physician.

II.—To make every preparation in advance for medical treatment.

26. Not to establish on the continental frontiers either *cordons* of health, or pest houses, or quarantines, for the purpose of preventing the invasion of the cholera, experience having shown that these means are more productive of inconvenience than of benefit.

27. Nevertheless if there should be in any of our maritime ports vessels on board of which there are cases of sickness or deaths from the disease, such vessels should not be permitted to have free *intercourse* with the inhabitants but should be treated as evidently suspicious, and subjected to a quarantine of twelve days at least.

28. To increase the means of public relief afforded to the indigent sick, and to secure to the poor the means of subsistence; to procure for them clothing, fuel, blankets, and to distribute among them frequently fresh straw for their beds.

29. To establish in every community sanitary commissions for the purpose of observing everything which concerns the public health. These commissions composed of the burgomaster, of notable inhabitants, of physicians and apothecaries, shall give advice as to the changes and ameliorations of which the localities confided to their superintendence may be susceptible, in order to arrest the progress of the Cholera, and to aid those who may be attacked with the disease.

30. In each division, district or section of cities to establish besides sub-commissions of health, whose business it shall be to examine the streets, public places, markets, public and private institutions, houses, &c., to investigate the causes of insalubrity, and to make them known, in order to point out the danger to the inhabitants, persuading them to remedy these causes as much as possible, under the direction of the authorities. To these commissions might be granted all the powers which may be thought useful for the improvement of the condition of the poor and of the public health: they should correspond with the central sanitary commissions and the authorities of the community to which they belong, and should be composed of the curate or vicar of the parish, of three notables, the head of the poor establishment, or a member of the Society of Benevolence, of a physician, a surgeon and an apothecary.

31. To persuade the heads of the poor, the members of benevolent Societies, the ecclesiastics of the parishes, and all charitable persons who have influence with the wretched and less enlightened portion of the population, to visit indigent families in order to make them feel that a want of cleanliness, humidity, crowding, want of air, of ventilation, and of solar light in their habitations and of suitable clothing, intemperance, prolonged exposure to the inclemency of the weather, excesses of every kind, especially drunkenness, favor the development of the disease and aggravate its effects. They should endeavor to prevent many patients from lying together, or being confined in a locality too small, or containing other individuals in good health.

32. To increase the number of physicians of the poor or of charity, so that all the sick without distinction may be visited and relieved without delay.

33. In each division of populous cities and in all the communities, to establish, under the direction of the Sanitary Commission, offices of relief, to which shall be attached two physicians at least, so that they may relieve each other when occasion may require, and that there may be always one in waiting, night and day, ready to give advice and attention to such as may present themselves for that purpose.

34. These offices of relief should be provided :

1st. With a covered litter furnished with a matrass, with blankets and every thing necessary for the transportation of the sick ; 2nd, with a medicine chest ; 3rd, with utensils indispensable for the use and administration of the first remedies.

35. As the disease, from the time it has entered a house is not only formidable to those whom it has attacked, but threatens also the inmates who may be in good health, one of the principal means to be employed for the protection of the latter, is to cause them to remove, by procuring for them salubrious dwellings, remote from the *foci* of infection.

36. When the sick belong to the indigent class they should be prevailed on by persuasion to suffer themselves to be transported immediately to some of the hospitals destined for Cholera patients.

38.* It is indispensable that temporary Hospitals, well organized, should be established, combining everything requisite for the treatment and cure of the sick, and the preservative of other individuals.

39. These asylums, opened by public benevolence to afflicted humanity, should not only receive gratuitously all the indigent who may be attacked with the disease, of whatever country they may be, but moreover every person who may demand to be admitted on paying so much for every day that he may remain.

40. These temporary hospitals should be furnished with whatever is indispensable for the medical service, and should have their directors, their physicians, their resident students, apothecaries and stewards.

41. In each city or community there should be at least one of these hospitals for every one hundred thousand inhabitants.

42. They should be on dry ground, in the most elevated or exposed situations, well ventilated, remote from the evaporations of rivers, ponds, marshes, sewers and ditches, and in cites near the districts inhabited by the poor, upon whom particularly the scourge exerts its ravages.

43. It would be better to increase these temporary hospitals than to diminish their number by giving each of them too great extent ; the air of these large hospitals, the wards of which contain many sick, is the chief obstacle to their cure.

44. In cholera hospitals three divisions should be established : one for those who are suspected of having the disease, another for confirmed cases, and the third for convalescents.

45. If these temporary hospitals do not admit of the divisions just mentioned, houses of refuge or of health should be established near them, for those who are suspected of having the disease, and houses destined for the reception of convalescents.

46. To enjoin upon the Commissaries of the police and other officers to keep a Register of every event relating to the public health, which they should communicate daily to the Sanitary Commission of the place.

47. To request the proprietors and principal tenants of houses, hotel and innkeepers, lodgers, and all those who keep furnished apartments, to make known with as little delay as possible, to the nearest office of relief, all that relates to the Cholera.

48. When a sick person is in a condition to receive immediately the necessary relief, the director of the office should send him a physician without delay. After the administration of the first remedies, he should be removed to the nearest temporary hospital, if the patient or his friends consent.

49. To cause to be washed, bleached or disinfected, the litter, linen and clothing which have been used by Cholera patients, before permitting them to be used by healthy persons.

50. The bodies of those who have died of the disease after having been

* I suppose 37 is omitted as not applicable. I give the numbers as I find them.

sprinkled with choride of lime, should be removed as soon as the death has been clearly ascertained, so that they may be immediately transported in carriages well covered, to the place destined for their interment.

51. The bodies should be interred in the ordinary cemeteries, or in some place set apart for that purpose, remote from habitations, never in churches, or chapels, or gardens, or private houses, and should be buried in ditches of a meter and a half in depth, without waiting, as the custom is, until other bodies have arrived.

52. To prohibit the exposure of the bodies of Cholera patients in the churches, where they might greatly prejudice the health of those who assist at the religious ceremonies.

53. To transport bodies to the cemetery during the night, before the rising or after the setting of the sun, without noise, without the ringing of bells, and without religious parade, from which it would also be well to abstain in going to administer the sacraments to the sick.

54. During the continuance of the epidemic the sounding of the agony or of the funeral knell should be forbidden; at the same time, as numerous assemblages are much to be feared, they should also be avoided.

55. In short, to persuade the public, particularly the common people, to call physicians to their assistance, as soon as the first symptoms of the disease manifest themselves. M.

Phlegmatia alba dolens--Death--Phlebitis with obliteration of the right Iliac vein.

It is still a question with authors to determine to what anatomical cause the condition known as *Phlegmatia alba dolens*, or acute œdema, should be ascribed. Some regard it as the result of anatomical changes very different in their character, of inflammations either in the sanguineous or lymphatic vessels, or in the cellular tissue itself. Others, on the contrary, relying principally upon the researches of Messrs. Trousseau, Bouillaud and Velpeau, maintain that *Phlegmatia alba dolens* is always accompanied with phlebitis, which has produced an obliteration complete or incomplete, of the inflamed vein. The case we are about to report, tends to confirm the latter opinion, which is fortified moreover by observations at once more numerous and more conclusive.

Louise Prevot, a servant, aged twenty-two, entered the Neckar Hospital, in the ward of M. Trousseau, at No. 11, Hall of St. Anne, a month after her confinement. The labor had been long, and it became necessary to use the forceps. Fifteen days after delivery, she was taken with *phlegmatia alba dolens*, which was accompanied with fever, and compelled to keep her bed. It is shown that at the time of her entrance into the Hospital, she had an œdematous tumefaction of the right leg. Pressure excited acute pain in the posterior and the third of the superior part of the right leg. The belly was yielding and inactive, the fever was violent, and there was considerable oppression. Auscultation discovered a pneumonia occupying the right side in its whole extent, with pleuritic effusion in the inferior part. She was bled twice. The blood indicated a high degree of inflammation. Ipecac. was administered and a pectoral tisane.

Notwithstanding the use of these means and the application of a large blister-plaster on the back, the disease made very rapid progress. The pneumonia increased, and with it the oppression and the fever. The pulse became small and depressible, while it continued to beat with extreme frequency; adynamy ensued, respiration became more and more difficult, the skin was excoriated on a level with the sacrum, and the patient died very soon, six days after her entrance into the Hospital.

Autopsy was made twenty hours after death. The right primitive iliac vein as far as its junction with the *vena cava*, the external iliac, the femoral as far as five or six centimeters below the crural arch, were completely obliterated.

The femoral vein was empty as far as the popliteal, and all the deep seated veins of the right leg were obliterated. In the primitive iliac the obliteration was characterised by a fibrinous mass, containing a remarkable quantity of a liquid analogous to serous pus. The periphery of the fibrinous coat, within the extent of eight to ten centimeters, adhering to the venous wall, was as a false membrane would be with the pleura. At this point the venous walls were thick and rigid, like those of a large artery, but without redness; immediately below a puriform liquid filled the vein, and lower still the obliteration was caused by a clot of blood, partly fibrinous and partly cruoric. The clots in the deep seated veins of the right leg were partly fibrinous and partly cruoric.

Thorax—purulent effusion on the right, pneumonia of the same side, with small purulent abscesses disseminated, surrounded by peri-pneumonic *noyaux* in the two lungs. All the viscera of the abdomen were perfectly healthy; and there was no appreciable obliteration nor inflammation of the uterine sinuses; nor any alteration in the internal surface, nor in the thickness even of the tissue of the uterus.

M.

Translated from Bull. Gen. de Therapeutic.

3.—Fees from Clergymen.

At a recent meeting of the State Medical Society of Connecticut, a resolution was introduced by a member, to the effect that the present practice of prescribing gratuitously for clergymen and their families, ought to be abandoned. This resolution has given rise to considerable discussion. Several communications have appeared in the Boston Med. and Surg. Journal, pro and con, and the editor of the N. Y. Annalist enters quite heartily into the subject.

The ground taken by those who are in favor of exacting fees from clergymen as well as others, is, mainly, that there is no good reason why we should not, and specially that the clerical profession do more to uphold and extend quackery, in its various forms, than any other equal number of men.

Clergymen, it is contended, are generally no subjects for charity.—They are comfortably supported, and receive, like others, their hire.—Some of them are wealthy. They are not, as a body, like them of old; they have both “purse and scrip,” and many of them are “clothed in purple and fine linen and fare sumptuously every day.” Their poor parishioners who receive, perhaps, but their twelve or twenty dollars per month, are compelled to pay to the uttermost farthing; why should not the clergyman? Again, it is said, that the clergy sustain and extend quackery, and constantly step out of their own sphere to meddle with that which does not directly concern them and of which they are profoundly ignorant. One can scarcely take up a newspaper that he does not see the names of numbers of Reverend gentlemen, affixed to certificates attesting to the most ridiculous and impossible absurdities, and recommending, in the most extravagant terms, medicines, the very composition of which, they do not know, and the nature and effects of which, they would not understand if they did. They, moreover, are frequently intermeddling with the regular profession, recommending this man because he is a member of their particular church, and deservyng that, because he belongs to another, and they are, besides, the most active and influential advocates of hydropathy, homœopathy, eclecticism and other forms of quackery.

The above, we remark, contains the strongest part of the objections urged against further gratuitous service. Without stating formally the other side of the question, we shall, in as few words as possible, give our own views on the subject. We regard it as one of some importance, the discussion of which may do good.

We have a high respect for the clerical profession. Their mission is the most important that can be conceived. Compared with it all the professions, businesses, and pursuits of life sink into absolute nothingness. They are dig-

nified by their calling—the greatest, the best man in the universe cannot dignify it. On this account they ought to be respected; but the very sanctity of their profession turns the eyes of all men upon them. They are men, nevertheless, and subject to like passions and infirmities with us. They are obnoxious to mistakes and errors like us, and some of them are no better informed, even in what relates to their own calling, than they should be. There are exceptions to all general rules, but, taken as a whole, the clergy of the United States are an educated, refined, and able set of men. As a body, they are not guilty of the charges preferred against them. Many ignorant or superficial ones (those who are varnished over with a thin scum of universal knowledge, we mean,) there are, and these are they, for the most part, whose names are found in the lying advertisements of the newspapers. We will go farther. There are some denominations very nearly or entirely free from the sin laid to their charge. No well-educated, faithful, pious, conscientious clergyman, who has a proper respect for his profession and himself, and we have many such, will ever, under any circumstances, lend himself to the propagation of error and falsehood, in the way spoken of. The vast majority of clergymen appreciate as it deserves, the medical profession, and are found everywhere, its considerate and consistent supporters. The whole body should not be made to suffer for the delinquencies of a few. Our own skirts are not entirely clear. Recreant M. D's., who love gold better than honesty or honor, as well as Reverends, appear appended to newspaper puffs, and gaseous advertisements. We spurn them both alike. We discriminate in the one case, why not in the other?

As a profession, then, we acquit the clergy of any attempt or desire to depreciate or injure in any way, the legitimate science of medicine. So far as this charge is concerned, we would have every physician to act for himself. Whenever and whenever a clergyman is found encouraging, in any way, quackery in our profession, let him be marked. If he consorts with quacks, to their tender mercies consign him; and if any professional services are rendered, charge to the extent of the law, and collect the fees.

But how is it in reference to the pecuniary ability of clergymen? In the large cities they often receive liberal salaries, besides numerous perquisites, but it is perfectly notorious that the great majority throughout the country are miserably remunerated for their services. No class of men, of equal attainments, are any thing like as poorly paid. We verily believe, that if all the salaries were averaged, the resulting sum would not be over \$500 per annum. Out of this pittance, families are to be supported, and old age provided for. An equal amount of talent and learning devoted to almost any other pursuit, would secure a competence, and the very fact that a young man is willing to forego his earthly prospects, and embrace a life of self-denial, if not of actual privation, is "confirmation strong as holy writ," of the purity of his motives—we had almost said, of the depth of his piety. Clergymen are almost universally poor. Now and then one has inherited a patrimony, or married a *rich wife*, but the exceptions only prove the rule.

We think, in view of the whole subject, which we have, however, barely glanced at, that the following is the proper course to be pursued. When a clergyman is wealthy, or has an income, independent of his salary, he should pay his physician's as well as his grocer's bill. In all other cases, with the exceptions mentioned before, which should be absolute, we hope the practice of the profession will be as it has heretofore been, and that the clergymen and their families will receive, as a general rule, without fee, the best services we can render.

4.—*Cæsarian operation after the death of the Mother—Child saved.*

The *Bulletin General, Medical et Chirurgical*, copies from *El Telegrapho Medico* for 1848, the particulars of a very interesting case of Cæsarian section, of which we shall give an abridged account. A female 30 years of age,

of a rachitic constitution, a deformed pelvis and a malformed chest, yet the former seemed not to oppose any serious obstacle to the delivery of the child. During the progress of the labour, when the head of the fœtus had entered the pelvis, the mother was seized with tetanic convulsions and expired on the spot. Efforts being made for a few moments to restore the woman, and failing, M. Pelapo instantly decided to perform the Cæsarian operation to save the child. With the aid of an old sage-femme only, he opened the abdomen, then the uterus, after the method advised by Mauriceau, and extracted a small female child, quite robust, but asphyxiated. By means of insufflation and dry frictions along the spine and over the chest, he had the satisfaction to bring back the little creature "untimely riped" from its mother's womb, to life. M. Pelayo states that during a practice of 27 years, he has performed this operation six times after the death of the mother, and this is the only instance in which it had proved successful. Should any doubt arise in the mind of the practitioner as to the death of the fœtus, auscultation will settle the question, and the operator may thereby ascertain when to omit or to perform the Cæsarian operation.

5.—*Asiatic cholera—Cholera asphyxia.*

As this disease has and continues to occupy a large share of public attention, we shall make no apology for the subjoined interesting extracts from "*Braithwaite's Retrospect of Practical Medicine and Surgery*,"—containing the views of several eminent physicians on the nature and treatment of Asiatic Cholera, as observed by them in different parts of the world, where this disease has prevailed. Like many other diseases, whether epidemic or endemic, the Cholera is no doubt modified in many of its features by climate and local causes; still the main characteristics of the diseases are preserved in all climates and in all latitudes. We have sometimes thought that this pestilence was a modern form of the plague such as prevailed in London in the time of Sydenham, a *stereotyped* edition of an obsolete disease. Be this as it may, the Cholera is the plague of the 19th century and deserves the serious attention of the scientific physician.—But to the extract. Ed.

On the Nature and Treatment of Asiatic Cholera.—By Dr. W. C. Bell, K. L. S., Physician to the Manchester Infirmary; late Physician to H. M. Embassy in Persia.

[The following is Dr. Bell's opinion of the nature of Cholera. He says.]

1st. With many able authors, I hold Cholera to be merely a form of ague; and, in addition, I maintain its type to be quotidian. To determine the nature of ague, therefore, if this position be true, is to determine both the nature and treatment of Cholera. To do this, it is necessary to have a clear conception of what is really essential in an ague. In the first place, I may venture to premise that I have enjoyed very ample opportunity of seeing ague in every variety of type and degree of severity; yet I have never seen a single attack of any kind of ague in which the whole cycle of its stages—that is, in which each individual paroxysm—was not completed within twenty-four hours. I have often witnessed several paroxysms completed within that time, but never one incomplete; and I have never met with a description of an ague in which the paroxysm exceeded twenty-four hours in duration, except where the author would appear to have considered Asiatic cholera to be such an ague.

2dly. The congestive or cold stage is the only essential which is never wanting in ague. It may be severe or slight, partial or general—be with or without shivering; but in ague, and every disease allied to it, such as masked and misplaced agues, as they are called, periodical headache, and neuralgia, and all remittent fevers I have yet met with, this stage is never wanting, though sometimes obscure.

The second, or hot stage, may, or may not occur; reaction may, and often does, take place quietly, and without fever, and unsucceeded by a period of relaxation or sweating. This condition frequently occurs in what is called the fortnight ague—a form with which all who served in the late campaigns in Scinde are well acquainted, where the shivering is severe and hysterical, but where the febrile and sweating stages are often entirely wanting; and yet this is the form most remarkable in its periodicity, and in its return for one or more days, every fortnight, just previous to the lunar changes.

In other forms, more especially the quartan, all three stages generally prove exceedingly severe, and in this form the period of relaxation may perhaps pass the twenty-fourth hour from the accession, but I have not seen it do so, and I should be inclined to define ague as a paroxysm of congestion of the internal veins, subsiding within twenty-four hours, and often followed by febrile action and relaxation of the capillaries. It appears to consist in a gradual change in the action of the extreme capillaries, and apparent constriction of them, by which the blood is rapidly driven inward upon the great veins. When this has reached to such a point as to oppress the action of the heart, yawning first, and then shivering, or a sense of suffocation and pain in the præcordia are the indications of oppressed circulation, and of the commencing effort of the heart to overcome the mass of blood which is stifling it. If, by the application of tourniquets to the limbs, or by bleeding, part of the blood which is rushing from the extremities to increase this congestion is prevented from reaching the great veins,—the heart excited to increased action, is enabled by this relief more quickly to overcome the obstruction and restore the balance of the circulation, and the paroxysm passes off. If not thus mechanically aided, the heart, after a severe struggle to maintain the circulation during the period of constriction, is at length relieved, by this nervous disturbance or spasm of the capillary circulation passing off of itself, and then the heart and arteries, so long excited by the struggle, maintain for a time their increased action after the obstruction in the capillaries is removed, and produce apparent febrile action. Presently this excitement subsides, the vessels become relaxed, and sweat succeeds. The vessels continue in this state for a longer or shorter period, according to circumstances, till they at length recover their ordinary tone of action in the intermission. This fever, however, is not fever properly so called, but reaction; and the sweating not critical or essential, but relaxation. The cold stage is alone essential, and is the physiological cause of the subsequent stages.

[Dr. Bell's opinion was formed from his experience of a series of epidemics which raged in Persia in 1842, when Dr. B. was attached to the embassy, and was in medical charge of the Persian army. He says:]

The first of these was dysentery; then a peculiar periodical disease, till then undescribed, attended with intense disturbance of the circulation of the nervous system; which also appears to have been observed at Strasburgh. Then came tropical remittant, then congestive ague: and this finally terminated in true cholera.

To this series of the diseases, I am indebted for being led—as I cannot but think I have been—by a species of natural analysis, and by easy transitions, to a clearer insight into the nature of the disease; and to a practice, unbiassed by a theory, and simply arising out of a process of accommodating my remedies to the changing type of disease: which then proved successful in every case I attended, where the feet had not already become warm, while the legs and body remained cold. This is a symptom which my whole experience teaches me to consider as a sign of actual death. In this, most authors of experience bear me out, and I have invariably found that every interference with the patient who presented this fatal symptom only increased the spasms and suffering, and hastened the consummation.

Almost every intelligent author on this subject, has classed cholera with the cold fit of ague—as, indeed, the whole of its features render nearly inevitable;

and viewing it in this light, the rationale and use of bleeding, as recommended by Mr. Bell and other authors, could not be better laid down than it is in his work on Cholera Asphyxia. Yet among these authors I have searched in vain for a single statement that cholera is—what I am satisfied it will invariably be found to be—a congestive ague of quotidian type. All these words I have read consider it as, *in its whole course*, merely the cold fit of the ague, and that the fever which occasionally succeeds to it on recovery is the hot stage.

In my belief, no single paroxysm of ague of any kind ever occupied more than twenty-four hours in passing through all its stages, and, according to my experience, (whenever its progress is sufficiently slow to run that course) invariably completes an entire paroxysm within that period, consisting of the congestive or cold stage and the remission. The stage of reaction which follows congestion, can scarcely be said to exist in cholera, and the sweating stage, or stage of relaxation, is only occasionally perceptible, being so little marked as to be nearly undistinguishable from the short period of remission or intermission, but in every case of the epidemic which I had most opportunity of observing, there was invariably a diurnal remission and quotidian accession; and I am greatly inclined to believe this universal in all forms of cholera, where not prevented by previous death or cure.

The symptoms of these stages are so little marked as to require the most attentive observation to distinguish them, and until the opportunity shall present itself of actually looking for them, I do not expect to be credited, and all that I can hope for at present, from those most conversant with the disease, and who have not as yet entertained this view, is, that they will at least give it their consideration, and try to recollect whether in the more protracted cases that have fallen under their treatment, they have not observed that the patient, after tossing about for many hours in an agony of suffocation, seems at last exhausted, and after long jactitation and continuing the throw off every covering, he at last for a short period remains quiet and submits to the load of bed clothes. This temporary repose is in most cases almost the only symptom, except the more tranquil action of the heart, that marks the intermission, and forms the most obvious and almost the only guide to the essential part of the treatment of a severe case. In a very short time this period of comparative tranquility ceases—the patient begins again to yawn, to throw up his arms like one bleeding to death, and in a few minutes more to toss about again, and to show every symptoms of suffering agony.

These symptoms of the return of the congestive stage have, in the mass of my cases, appeared exactly twenty-four hours after the first accession.

[Dr. Bell further remarks upon the cold stage of cholera, or stage of “collapse” as it is termed, that it differs much from that state of the system to which the term collapse is ordinarily applied. He observes:]

Collapse, if it mean anything, surely means a sinking from the cessation of the power which supported the vital actions, but the more the condition so called in cholera is examined, the more, I am convinced, it will be found that it arises from an active cause controlling these powers; that, if collapse from sinking or hæmorrhage may be termed adynamic, we shall be forced to express that which occurs in cholera by the contradictory term dynamic; that it is an active, not a passive, condition of the capillary circulation. It is obvious that, if this distinction be not made by the practitioner, he will inevitably attempt to treat collapse in cholera on the same principles that he would adopt in an apparent similar condition of the system proceeding from a deficiency of action. It is true that both in adynamic collapse with sinking and syncope, and in that of cholera, which I have termed dynamic there is diminished circulation and arterial action; but in the first the pulse is open, soft, and feeble, and unless proceeding from hæmorrhage, becoming generally slower, softer, and feebler, till it ceases; whereas in cholera it is contracted, small and wiry, as long as it can be felt.

[But whatever name we may give to the cold stage of ague or cholera, it

results from a peculiar condition of the capillary circulation to which Dr. Bell gives the term "*spasm*," using this term not to indicate a theory, but as being a convenient form of expression. This condition of spasm it is which, when confirmed puts a stop to secretion; a good example of this is found in the state of the tongue, which is always *free from fur*, pale and cold, in Asiatic, while it is as certainly loaded in English cholera. Another effect of this state of spasm is the diminished temperature of the body, which seems irremovable by any external application of heat. As a proof that this coldness is "a product of action rather than effect of the cessation of action," we find that when all action has ceased in death, the body becomes warmer, and the warmth commences, as death commences, in the feet. In summing up this part of the subject, Dr. Bell says:]

That the ordinary actions of the capillary circulation are put a stop to, or strangely altered, during the condition which we have termed spasm. That this spasm has a tendency of itself to relax in due time, and again to recur unless prevented by a change of action, induced either by natural or artificial means. And we have observed, that in consequence of this action the blood is driven inwardly from the extreme capillaries into the great interior veins, leaving the extremities bloodless and chilly. We shall now proceed to consider the effects of this derangement in the distribution of the circulating fluid.

The first effect or symptom of this overloading of the internal blood-vessels is pain at the præcordia and oppression of the heart, with congestion of the spleen and vessels around the stomach. In examining the comparative anatomy of these organs in the phocæ, the cetacæ, and other diving animals, it is found that they are the means provided by nature temporarily to receive, as into a reservoir, the excess of venous blood which, unable to pass through the lungs during suspended respiration, would otherwise accumulate upon the right side of the heart and oppress its action. The spleen and neighbouring veins serve exactly the same purpose in man, though in a less degree, because less required. By their capability of receiving into their expanding and elastic structure a considerable quantity of blood without injury, and so withdrawing it temporarily from the circulation, these organs afford a means of obviating that oppression of the heart and injurious distention of the important secreting organs, which might otherwise prove fatal every time that such passing cause occurred to impede the circulation through the lungs, as a fit of choking or sneezing, or that a cold chill happened to drive the blood suddenly inwards, from the skin upon the heart; when, however, this reservoir becomes filled to its utmost extent, as in ague, and the blood still continues to be poured inwards from the external veins, the spleen is no longer capable of acting as a safety valve, and the overflow then passing with great freedom through the portal vessels, distends and strains the cavæ and right auricle, choking and oppressing the action of the heart.

The ordinary symptom of reaction from this condition is shivering, but if the oppression be greater than the power of the heart can overcome, not shivering but pain in the region of the heart and spleen, gasping and a sense of suffocation are induced, and these symptoms are often accompanied with sickness of stomach.

At the same time that the blood over distends the cava and oppresses the heart, being still poured inwards from the extremities to which their well-valved veins permit no regurgitation, it is necessarily driven back from the gorged vessels upon all the internal venous branches unprovided with valves; the renal veins are distended, and the returning current of blood from the kidneys stopped; consequently a first effect or symptom of such congestion is the cessation of the secretion of urine, and the first urine passed on recovery from cholera is often mixed with serum and blood-globules. In like manner the hepatic circulation is oppressed, impending the secretion of bile; these two constitute the second class of mechanical effects or symptoms. At the same time another effect is produced by the same cause; the portal circulation, at

first impeded by meeting the reflux from the vena cava, is presently reversed, and the blood is thrust back into the mesenteric veins, till it distends their extreme ramifications on the mucous membrane, where it finds a species of vent; for from this surface the repellant force squeezes out the fluid portion of the blood into the bowels, carrying with it the mucous epithelium, and constituting that serous or rice-water evacuation which is esteemed characteristic of cholera, but which has no title whatever to the name of secretion, or to be treated as such, being a mere exudation which is the very reverse of a vital action.

[These three then are obviously mechanical effects of the congestion, and manifest themselves in the form of symptoms, as:]

1st. Pain at the præcordia, sickness, oppression and struggling of the heart. 2nd. Cessation of the secretions of bile and urine. 3rd. Serous purging.

As all of these are the direct effect of too much blood being driven in upon the right side of the heart, it is not difficult to see that by diminishing the volume of this blood, their mechanical cause would be removed; and thus it is that the purging itself tends to relief, although less effectually than direct abstraction of blood, because only its fluid parts are hereby abstracted, leaving the bulk of it in a condition comparatively unfitted for circulation through its loss of serum.

But there are three things to be considered in attempting this mechanical relief of blood-letting:—First, how? second, when? and third, the ultimate effect on the disease?

The answer to the first of these questions is simple enough; it must be done, if possible, by direct diminution of the mass of blood that distends the cava. To do this, however, from the vessels most oppressed, is impracticable from their situation, and we are therefore forced to attempt to fulfil this object indirectly by opening a vein in one of the extremities which is yet sending its tide to augment this congestion: this indirect abstraction of blood acts not only by relieving congestion mechanically in the great veins, but the renewed motion of the blood excites the general circulation by continuous sympathy, and what is of more importance, it would appear that the bleeding exerts a direct effect in relaxing the capillary spasm. Some, in ignorance of the object of bleeding in cholera and ague, have opened an artery, as if it were their intention to cut off, instead of promoting, the small current of life that still circulates, but doing so has invariably induced fatal collapse.

The second question, as to time, is not so easily disposed of.

It has been said, that whenever blood has been got to flow with tolerable freedom the patient is safe. This is certainly not literally the case either in congestive ague or in cholera. In the first two cases which I witnessed of the former disease, unaware of its nature, and believing that I was treating acute dysentery, I bled freely, but, as it appeared, I did so in the hot stage, in which it was my misfortune first to see my patients. The bleeding was followed in a few minutes by the most sudden and fatal collapse, with every symptom of cholera. The following day, with the abundance of similar cases which it brought, showed me my error, and then the lancet, fatal to the first two patients by being used at the wrong time, saved hundreds when used at the very commencement of the period of congestion; and I am happy to say that in many thousand similar cases no such accident happened to me afterwards.

[Dr. Bell tells us that the native Persian Practitioners bled, but generally with a fatal result, because they bled when reaction was commencing; and he thinks that when bleeding was found to be unsuccessful by the medical officers of the East India Company, it was from the same cause. Dr. B. proceeds to say:]

When the exudation from the bowels or skin is yet flowing freely,—for in different forms of the epidemic either symptom may predominate,—and when

the heart is heard to be struggling in an extraordinary state of excitement, and in the active endeavor to overcome obstruction it is *churning* as if it would burst,—a sound difficult to describe, but once heard, never to be forgotten,—when, I would say, the sound of the heart conveys to the listener the impression that it is laboring with unabated vigor of reaction, the lancet cannot be used too promptly; but when the attack has continued for four or five hours,—when the purging has become less copious,—and when instead of manifesting a continuous struggle, the heart appears only to be roused up to reaction at intervals, when the natural warmth of the skin is a little more extended on the neck and chest, and the patient though still tossing uneasily, is not now in his previous agony, I would hold my hand, and trust to medicine for some hours to come; I would then wait until I observed him rest a little more quietly, and bear the weight of the bed clothes for a time, and until the heart's action continued comparatively tranquil for half an hour or an hour; then, indeed, on the very first symptom of renewed distress, or even sooner, I would bleed without hesitation, till the thick tarry blood, that at the first is squeezed and kneaded from the arm with difficulty, flows freely from the orifice, and becomes changed to a florid hue.

[And in another place:]

All depends upon the period at which bleeding is resorted to. If early in the congestive stage, or just previous to its second accession, it is invariably successful; if just as the congestive stage is passing off, when the pulse begins to acquire a little power, it is invariably fatal.

[This fact Dr. Bell explains in the following manner. In the first stage the heart is excited to the utmost by distension of its cavities, from behind, and opposition to its action by spasm of the capillaries in front; bleeding gives relief from pressure *à tergo*, and probably aids in relaxing the spasm, while at the same time by relieving the congested state of the great secreting organs, it enlists their sympathies in support of the vital actions; and the power of the heart being unimpaired, it can now carry on the circulation with vigor. But in the second stage, the heart's energy is much exhausted, and its irritability impaired, by long continued distension; and syncope and relapse will be the probable effects of bleeding. If then we cannot bleed, what are we to do? We must use those medicines which will invigorate and regulate the circulation generally, and control the tendency to periodical disturbance. Of these, *quinine* first suggests itself; it has been tried, and *failed*. Upon this subject, Dr. Bell says:]

I have found it fail in like manner when used alone in ague, but when combined with iron,* I have found it eminently curative in cholera; and although quite unable to explain the reason of this anomaly, I may, per-

* It is very remarkable how much the general use of iron as a medicine has increased in this country since the last visitation of Cholera. I conceive that this arises from a greater than previous tendency to local congestions; early in the century acute inflammations were much more frequent than now, and many of the fevers yielded only to free bleeding; the lancet was constantly in the hand of the practitioner; now its use in fever is almost obsolete.

Iron seems especially used for the purpose of facilitating the oxygenation of the blood, and enriching it as in chlorosis, and in aiding the action of a feeble heart, probably by its influence in causing the blood to pass more freely through the lungs, and thus diminishing the tendency to congestion. It is useful alike in menorrhagia and amenorrhea, in congested kidney, spleen, and prostate, and in the most nervous diseases, arising from feeble and deranged circulation, &c. In fact, to combat the tendency to venous congestion, and as a corroborative and regulator of the circulation both general and local. It is the most direct opponent to mercury, the best cure for salivation, and certainly in my own experience in epidemic agues and remittents, including that known as gulf fever, (the terror of Europeans in the Persian gulf,) iron has proved as beneficial as I have invariably found calomel pernicious.

haps, be permitted to digress a little in order to illustrate the effect of this combination.

Much practice in the treatment of enlarged spleen, soon led me, contrary to what is advised in many English works of high authority, to regard calomel as an absolute poison in that disease, and the acquisition of Mr. Twining's valuable book on the diseases of Bengal, informed me of the value of iron in its management. I found the routine treatment of the Calcutta Hospital advised by him, viz., purging with salts and sulphate of iron, combined with periodical bleeding, infinitely preferable to the French and American plan of giving large doses of quinine, but I do not think of combining the two methods, till it occurred to me to do so in the case of one of the gentlemen of the Russian embassy, who was attacked by a much more severe form of ague than I had before met with in Persia.

[Dr. B. then gives quinine as an anti-periodic, and iron as an anti-congestive. He says,—]

My ordinary treatment in cases of cholera was a wine glassful of a mixture composed of—

℞. Quinæ. dissulph. gr. xij. to ℥j.; ferri sulph. gr. ix. ; acid. sulph. dilut. m℥l., aquæ puræ. Oiss;

repeated from quarter of an hour to every hour, according to circumstances.

I believe this and other watery fluids to be much more rapidly absorbed during the intervals of exudation than is generally imagined. I have generally found the first dose or two put a stop to vomiting. In those attacks which I have ventured to class along with cholera, where the exudation from the skin or into the loose cellular tissue, took the place of that into the bowels, I added to the above mixture an ounce of Epsom salts, as a derivative.

With regard to diffusible stimulants, they are often successfully employed in the first moments of an attack, and immediately on the occurrence of the first purging, but not often after congestion is fully established; any one on listening to the heart in this stage, and hearing it laboring so vigorously, but in vain, against a force superior to its utmost power, will be convinced that it is impossible that increased stimulus can make it prevail. My own experience is totally against their use, unless in combination with bleeding, or in the very outset. I have satisfied myself by experiment that in the congestive stage each dose is attended with instantaneous increase of the congestion and exudation; besides, it would seem only probable that to stimulate still further the already over-excited heart can only tend the sooner to exhaust it, unless at the same time mechanical relief to the congestion be afforded. In the third stage, or remission, stimulants will probably again prove beneficial; but I have not latterly used them, and would leave their employment to be regulated by circumstances.

[Dr. Bell thinks that *opium* is likely to prove injurious in the treatment of cholera by diminishing the sensibility (irritability?) of the heart. He disapproves even of the ordinary use of *calomel* while cholera prevails, and still more of its employment in this disease itself. As to *applications* he prefers cold, having tried heat extensively without any good result; he says,—]

All the patient's desires are for cold, and considering spasm of the capillaries as the chief cause, cold applications ought to be a powerful means of removing this; but I have never had sufficient confidence to try the applications of the wet sheet, having hitherto had reason to be satisfied with the practice I have already recommended; but many instances have come to my knowledge, where the application of cold water has been successful, without one equally convincing proof of the efficacy of the hot bath, or oven practice. I am well acquainted with three persons, who, after they had been laid out for dead, on being washed previous to interment in the open court-yard with water, to obtain which the ice had to be broken, recovered in consequence, and lived many years. I received last year from a friend in Persia, Lieut. Ferrier, a French officer, an account, which I think a little exaggerated, of his success in the treatment of cholera in Tehran, by keeping his patients immersed for several hours in

a cold bath; and, only last week, I received from Erzeroon, in Turkey, a letter from our excellent consul, M. Brant, who states, that Dr. Dickson of that place was then curing more patients by frictions with snow and ice than by any other treatment.

The same practice is reported to have been the most effectual in Russia. I mention this not for the purpose of recommending it, but as the most recent intelligence, and in order that this, along with other methods, may be regarded in reference to the principle which I have been endeavoring to illustrate, that the cause is active,—that it is of temporary duration—that it tends to recur, and that whatever the mode of treatment adopted, it must have reference to the cause of, the existence, and the effects of congestion, and to its remission and return. In conclusion, I would beg to state, that even should all that has been advanced be considered mere unfounded hypothesis, I would still venture on the ground of experience to beg the attention of practitioners to a consideration of the rules attempted to be deduced from it, viz:—

The use of quinine and iron in combination very frequently; abstinence from the use of the lancet, except in the very commencement while the heart's power is evidently unsubdued, or after the patient has had an interval of repose—(not sleep, for that never occurs in cholera):—and except in those two periods rather to abstain from the use of stimulants, which are extremely painful to the patient.

After hours have elapsed, I consider the practitioner more fortunate who fails altogether to obtain blood than he who draws four or five ounces, and unable to get more, binds up the arm; the first patient may recover; the other, though apparently the most promising subject, seldom will. I have often known it occur that where every effort to obtain blood has failed, the same veins have bled freely after the exhibition of several doses of quinine and iron. At all times the use of the lancet requires the utmost determination; and when blood has once began to trickle, no apparent cruelty must be shunned to order to get it flow freely till its florid color shows the circulation to have become fully re-established; for, if abandoned before this is effected, fatal collapse is almost cerat in to ensue.

[Upon another interesting point in the history of cholera, Dr. Bell observes,—]

The direct evidence in favour of non-contagion has always appeared to me infinitely to preponderate over that to the contrary; and I know none more conclusive than the fact of its penetrating the triple cordon sanitaire established by the Prussian Government on the Oder in 1831 at exactly the same rate (four German miles a day) that it proceeded at both before and after its encountering what was expected to prove so formidable a barrier to its progress. *Med. Gaz. Nov. 5, 1847, p. 797, & 14, 1848, pp. 8 & 52.*

[On this interesting subject we find recorded the opinions of several other gentlemen who have been placed in circumstances favourable to its study. Mr. Thom's remarks are founded upon observation of the cholera as it occurred in Her Majesty's 86th regiment, in June, 1846. Mr. Thom states his opinion that the existence of a hot atmosphere loaded with moisture, and at the same time in a stagnant state, is a cause of cholera, independent of any chemical change which may have occurred in the air itself. He then proceeds to say,—]

With an increase of temperature and moisture, there is also an augmentation of the bulk, and diminution of the specific gravity, of the air; so that a volume which fills the lungs contains a proportionately less quantity of oxygen than it otherwise would do. This would be the case even in a dry hot air; and, in all probability, to this may be traced the origin of certain diseases peculiar to such a state—as the “apoplexy” of hot winds—and not to mere elevation of temperature acting on the skin. Now, the introduction of a large quantity of aqueous vapour, absolutely yielding no oxygen, into such an air, although it in some degree lowers the temperature, renders it still more unfit for respiration, by its occupying a considerable proportion of each volume inhaled. From these phenomena, I calculate that, at the lowest rate, with the thermometer at

90, and dew point at 83° , a given bulk of atmosphere contained one-eighth less oxygen during the month of June, than it did in January and February. In temperate regions, two pounds eight ounces of oxygen are daily consumed by the lungs; but, instead of this, only two pounds three ounces would have been inhaled at Kurrachee, even if the number of respirations per minute were as great in hot as in cold climates, which is not the case.

We also know that the soldier's ration is the same at all seasons, and he consumes as much animal food in summer as in winter—ay, in an Indian summer, or Canadian winter—and that animal diet requires more oxygen than vegetable, to render it fit for assimilation. In India, during the hot season, the just balance between these necessities of life is lost, and a tendency to morbid change must always exist in a more or less degree, according to the intensity of the pre-disposing and exciting causes.

Changes in the Blood dependant on the Climate.—Yet we admit that this is not alone sufficient to produce those death-blasts which pass over India in certain seasons, but that it is merely the first link in the chain of causation. The result is a carbonaceous state of the blood, diminished formation of fibrine and albuminous matters: while the crude matters from which these necessary elements are formed, require unusual energy of the system to throw them off. The liver acquires increased action to remove the carbon; while the skin, in consequence of the renal secretion being diminished in hot climates, has not only to carry off the fluid, albuminous, and saline parts of the blood, but the nitrogenized matters also.

That such a condition of the blood existed at Kurrachee in June, was evident in every case where it was examined, whether in cholera or other cases: nor is this new or unexpected, although an important fact. In men, previously sound in health, the immediate evils arising from this are, no doubt, very frequently averted by the adaptation of our functions to relieve nature in many ways that must ever elude our scrutiny. But it must be pregnant with danger, while extraneous or trifling source of irritation must tend to produce open disease; and the preservation of health under such circumstances must depend on an uninterrupted activity in the hepatic and cutaneous excretions.

Congestion, along with Deteriorated Blood.—Let us suppose any sudden or unusual congestion to take place in a system already impregnated with so much grave, although latent, germs of disease; I mean simple congestion, unconnected with any specific poison inhaled by the lungs or received by contact. It is almost needless to observe that such a phenomenon would lead to the most dangerous and fatal results—such, I firmly believe, as our regiment experienced. I am not prepared to say that the altered function of some organ may not produce a decreased secretion, which may act as a deadly poison by getting into the blood, and be the immediate cause of death; all I contend for is, that even this is but a consequence or part of the series of morbid actions merging into cholera.

Congestion Fostered by Moist Air.—The next point which forces itself upon our notice is the presence and agency of phenomena during and preceding cholera, which were eminently favourable to a morbid congestion of the circulating mass, whether previously deteriorated or not; these I consider to be undue moistures in the air, and a deficiency of the ordinary currents of winds; both being equally unfavourable to the process of evaporation.

The Air.—At the time when cholera existed, the dew point was from 82 to 83° ; every cubic foot of air contained twelve grains of aqueous vapour; and, had the external temperature been the same, all evaporation must have ceased from the skin and lungs; but as the latter was at 96° in tents, this salutary process was not wholly arrested, although the quantity of vapour carried off from the skin must have been utterly disproportionate to the necessities of the system at such a time. At night, however, when the air fell to 85 or 86° —within two or three degrees of the point of deposition—this must have been almost entirely arrested, and no doubt was so under certain circumstances.

The cutaneous exhalents may even be in activity, and pour out the contents on the surface of the body ; here, however, the perspired matter will remain, clogging the pores, congesting the extreme capillaries, and, with the body thus bathed in deteriorated fluid, the oxygen of the air is excluded from the skin, and the heat accumulates, which ought to be steadily removed by evaporation. The first stages of this state are rather active, but, if prolonged, passive congestion is the result, and a train of signs indicative of a cholera diathesis ensues.

Cutaneous Exhalation.—It is scarcely necessary to allude to the vital importance of evaporation from the skin, and escape of vapour from the lungs, or by the renal emunctories, in all climates or countries ; and every one, without knowing why, can appreciate the exhilarating and elastic feeling caused by *dry* winds, and languor and depression induced by humid ones ; and in no country is this more perceptible than in India, even when there is very high temperature. The quantity of liquids, in one form or another, which is introduced into the body in hot countries, exceeds that of cold ones by at least one-half, and in many individuals to double or treble the amount. The nature of things requires that it should be so ; but it is equally important that this superabundant fluid should be carried off as quickly as possible, before it is replaced by a fresh supply of this necessary element, to become, in turn, the vehicle of extra heat and effete matters. I do not overrate the quantity of drink used by a soldier living in a tent at 90 or 96°, who consumes his ration of animal food and takes drams of arrack, in stating it to be at least ten pints in the day ; whereas, during a calm, even if the body was without any covering, the air could only carry off from six to seven pints ; and, the secretion from the kidneys being too insignificant to compensate for the loss of the cutaneous action, a portion of this must accumulate in the vascular system, and produce congestion ; that, by daily increase, and by gradually disordering the functions, would create additional thirst, and add to the primary evils. It happens, however, that the body is more or less covered with clothing, and shut up in close tents or confined barracks at night, all tending to arrest the circulation of air and evaporation of the cutaneous exhalations during the night-time, when the thermometer falling from 90 to 86°, within 3° of the dew point, would lessen the amount of fluid removed from the body to nearly three pints, or less than one-half. This is, in all probability, connected with the fact of many more men being attacked at night, or towards morning, than in the day-time.

United Action of Morbid Agencies.—If, then—with constitutions long exposed to a bad climate, enervated by high temperature, and the qualities of the blood gradually changed by a deficiency of oxygen in the atmosphere—bodies of men become exposed to another and equally influential morbid agency, humid air, such as we have essayed to explain, we must anticipate the occurrence of much mischief. Simple congestion, if general, in a hot season, is alone sufficient to endanger life ; for every tissue becomes engorged, and every function disturbed or altered. The arterial, venous, and lymphatic vessels and capillaries all participate in the disorder ; the respiratory process is enfeebled and slow ; the heart is oppressed, and unequal to its load ; the brain and nervous system become torpid ; the chylopoietic viscera lose their tone ; and the portal system in a great measure removed from the general circulation is affected by this universal obstruction in a very marked degree. The sudoriferous vessels on the surface are in an atonic state, and passively allow their contents to escape on the skin, which is so profusely bathed in moisture as to lead to the supposition that this secretion is increased, while in reality it is, comparatively speaking, diminished ; but from the non-evaporating state of the air it is not imbibed, and leads to this mistaken idea.

Latent condition of Cholera.—The state of the system referred to, as resultant on chemical change of the constituents of the air—in which carbon is accumulated in the blood, and fibrin and albumen diminished—will vary in degree according to idiosyncrasies, habits, and constitution, so that certain

numbers of a community will be affected to an extent bordering on, or breaking out into, open disease. Noxious agencies, whether of atmospheric origin acting on the skin and lungs, or as poison introduced through the assimilating functions, when applied in a minute degree, but steadily kept up for a length of time, have a tendency to produce effects that are called accumulative. Their action is latent, but not the less certain, till all of a sudden it is developed as if the whole had been suddenly concentrated into one overwhelming dose.

Connection with Scurvy.—The scorbutic diathesis furnishes a forcible example of this; and sudden death is not only induced by slight causes of excitement, in men labouring under it, but even those who have exhibited no alarming signs have been equally affected. This is exceedingly applicable to cholera, between which and scurvy there is a great analogy in the state of the blood; and on the former subsiding, the latter appeared in our regiment, and also in other corps.

Sudden Climax to Accumulative Morbid Changes.—If, then, by a sudden increase of all the causes of this latent diathesis, a state of the weather, inducing universal congestion almost approaching to obstruction of the vascular system, occurs, can we be astonished that life will, in many, be abruptly cut short, as if some lethiferous draught were suddenly swallowed? Such, I am firmly persuaded, is the only rational way of accounting for those numerous cases of cholera which terminated fatally in a few hours; with few of those symptoms which nature usually exhibits in a salutary effort to remove local or general congestion. Sound blood might circulate slowly without destroying the nervous power; but, if it is deprived in a great measure of its vitality, it is quite impossible that it can be *delayed* in the brain without producing rapid dissolution.

Med. Times. March 11, 1848, p. 388.

[On the treatment of this disease in the stage of collapse, we next give the opinion of DR. WARD, whose conclusions are drawn from the investigation of numerous returns and reports made to government; and by this process he has been led to regard with more favour certain remedies which from his own experience he had considered even worse than useless. In a paper which he read before the Westminster Medical Society on this subject, Dr. WARD said.]

The idea of the nature of cholera that seemed most consistent with its symptoms and with the effects of treatment was, that it is the prolonged cold stages of a peculiar form of fever, presenting more analogies with continued than intermittent fever, the effect of a poisonous miasm, which, if very intense, destroys the patient, without the occurrence of any active symptoms, in a few hours; but in other cases induces a gradually increasing congestion of the venous system, which relieves itself by the effusion of serous fluid into the stomach and bowels, which is as speedily removed by the action of vomiting and purging. The blood thus deprived of its serum, becomes thick and black, and is deprived of its vital properties to such a degree, that, unable to imbibe oxygen in the lungs, it ceases to stimulate the heart, which organ is thus unable to propel the blood through the extremities or through the lungs. This is the stage of collapse, in which the patient is blue, cold, pulseless, and voiceless, and his features are shrunk to such a degree that his dearest and nearest relatives do not recognise him; and this is the most fatal stage, and that least under the influence of remedies. Nevertheless, even under these deplorable circumstances we must not relax our exertions; and the list of curative means employed with success, prove that much may still be done for the relief of the patient. The most approved external remedies were, external heat, cold affusion and counter irritants. The author attempted to account for the favourable opinion of the application of cold in Persia, and heat in Russia and the rest of the northern countries, partly by the concordance of such ideas with the ordinary feelings of the inhabitants in such different climates, and partly by the notion that in Persia, where the thermometer is above 98° in the sun, the patients exhausted of their fluids by the disease, would be dried up and mummified by any attempt to keep up the temperature of the body by exposure to the sun's heat; whereas

in Russia, &c., it was supposed, and probably with truth, that as cold is a direct sedative, its opposite, heat, was necessary to restore the vital powers.

All the reports concur in the efficacy of cold effusion in inducing reaction ; and its success appears to have been in proportion to the violence of the shock. Counter-irritants were successful only in connection with other remedies ; but they deserve attention from their stimulating power, and their readiness of application. Bloodletting was useful in every stage in relieving the congestion and rousing the heart by removing the load that oppressed it. In the state of complete collapse salt and mustard emetics, to excite the system, followed by bleeding, was a favourable mode of treatment. The internal remedies most to be relied on in the stage of collapse may be classed under the heads of revulsives, stimulants and specifics. The first class, besides emetics of salt and mustard, comprised tartar emetic and croton oil, and calomel in large doses. The effect of each was to check the vomiting and purging ; but the three last were remarkable for their power of restoring the flow of bile, of a dark-green color, when tartrate of antimony or croton oil had been used ; and like blue ointment, when the calomel had been given. The action of all three is supposed by the author to be irritating to the mucous membrane of the stomach and bowels, of which it stops the secretion by changing the action of the part. The blood thus diverted from the membrane returns without loss into the portal system, and, by the secretion of fresh bile, the bile previously pent up in the gall-bladder is expelled, the spasm of the duct having been relaxed by the irritation of its orifice, produced by the tartrate of antimony, calomel or croton oil. The blue color of the bile when calomel was used, may perhaps be explained by the decomposition of the salt by the alkali of the bile. From the number of returns in its favor, besides the extensive experience of the author in its use, he is disposed to place most reliance on the croton oil, as its action is simply irritant (many of the patients complaining that it made their throats sore when given in solution or suspension) ; whereas the tartar emetic is a direct sedative, and, hence, may be dangerous ; and the author had given calomel most extensively in large doses without ever having seen such effects produced as those which he has mentioned above from the reports of others. Stimulants, after a fair trial, were almost universally condemned ; and yet it is remarkable that M. Magendie had more success with his punch than any other of the Parisian physicians. His great rival, Broussais, was so unsuccessful, that he entirely relinquished his care of cholera patients at Val de Grace. Opium, either in small doses as a stimulant, or in large ones as a sedative, was equally unfit to be relied upon. The last class—specifics—comprises calomel, with, or without opium, cold water, salines, and quinine, although the author never met with a single case of real cholera in which he could trace the recovery of the patient to the influence of calomel, nor ever observed that it produced any specific effect whatever ; still, from the almost unanimous approval it has met with, and his own experience of its benefit in English cholera, he would strongly recommend it for future experiment, as being, at the least, perfectly harmless, though taken in enormous doses. He would also adopt the use of cold water *ad libitum*, upon the faith of the reports in its favor, though his own experience is decidedly opposed to it. Salines, on the other hand, when well diluted, have, besides a number of most favourable reports of their efficacy, this hypothesis in their support—viz., that they restore to the blood by their endosmosis through the coats of the vessels, if not by their being absorbed directly into the circulation, the saline matter removed by the serous evacuations ; whereas, the water, if it be not rejected by vomiting, as in the author's experience, could scarcely be absorbed, the tendency of the venous system being to empty itself ; nor, if it were absorbed, could it supply to the blood those saline elements of which it had been deprived. In the author's experience, the best mode, though a painful one, of arresting vomiting in all cases, is to keep the stomach empty ; when, after a time, it will cease to suffer the action of vomiting, whatever that may be. From an attack of English cholera he suffered, and thus cured, the last efforts

of which produced only bloody mucus, as well as from other similar results after emetics, the author believes that the stomach contracts itself during vomiting. Quinine, viewed in reference to the hypothesis of the intermittent nature of cholera, seems worthy of the still further trial than it has yet experienced. In conclusion, the treatment recommended by the author in the stage of collapse would be the following, and much in the same order as the remedies are stated:—Cold effusions; hot air; external counter-irritation, and frictions; venesection; mustard-and-salt-emetics; cold water *ad libitum*, or Dr. Stevens' salines; calomel, tartrate of antimony, alternately in large doses; and, if all failed, croton oil.

Mr. French said, that in conducting the treatment of the collapse of cholera, before using indiscriminately the articles of the materia medica, we should study the curative process adopted by nature to this end. We readily enough admit that this kind of knowledge is absolutely necessary in conducting the treatment of mechanical injuries, and that surgery is only a successful art when this principle is borne in mind. The mode in which reaction is accomplished by nature from the state of collapse in cholera, is a process with which I am perfectly familiar, by direct observation, and it is very simple. It consists in absorption of water into the bloodvessels, and in vomiting. It may be confidently stated, that a patient who is no longer purged, and who is vomiting, is undergoing reaction in the most favourable way possible; just as "adhesion" constitutes the most favorable and rapid cure of certain wounds. But to state the method more fully, it may thus be expressed:

1st. Absorption of water into the bloodvessels; the patient's intense thirst inducing him to take this fluid freely.

2ndly. Nausea; which produces a general relaxation of the system, thus diminishing obstruction to the passage of the blood in the vessels.

3rdly. Vomiting; which mechanically assists in driving forward the blood in the congested vessels.

In the more intense stages of collapse the process of reaction is not established before the vomiting has continued for three days. In slighter cases, collapse, reaction, and convalescence, may all occur in twenty four hours. When, however, the disease pursues this course, cases which presented the most hopeless aspect may do perfectly well, either becoming directly convalescent, or if local congestions and inflammations ensue, they assume an active character, and are under the control of art, while if this process has been frustrated, they commonly pass into a state of congestive and typhoid fever, in which they either sink or recover with great difficulty. As to the essential nature of the disease, I believe it consists in a poisonous influence which is exerted directly on the heart, depressing its action; for these reasons:—

1st. The heart's action is always diminished in this disease. 2ndly. No essential lesion of any other vital organs really exists. 3rdly. All the other symptoms and physiological conditions admit of explanation on this hypothesis.

The first argument is admitted by all observers. It will be also admitted that the functions of the brain are remarkably well performed until the very period of death. Then, there is no asphyxia for these reasons—the mechanical apparatus is undisturbed. We are familiar with the symptom, "coldness of breath," (inconsistent with asphyxia); the recumbent position, so necessary to the cholera patient, would not be suitable for the state of asphyxia. Dr. Parkes, who has recently published a work replete with interesting facts on cholera, denies the paralytic condition of the heart, because the left ventricle was always empty; he also found that the lungs were extremely collapsed, and infers that the cause of the arrest of the circulation must be sought for in the blood itself. The average duration of the disease, in the cases in which Dr. Parkes made post-mortem examinations, was ten hours; the observations were made sometimes a quarter of an hour after death; in all the cases the heart was found inirritable to the stimulus of the knife, while the

muscles contracted under this stimulus. With the hope of learning something on these points, I destroyed a rabbit in two hours by repeated doses of infusion of digitalis; the animal became gradually too feeble to move, breathed quickly, and died; on opening the body, the heart was still feebly pulsating. The lungs were completely collapsed, and the left lung was so firmly contracted, that it sank in water; it readily admitted of inflation. The right side of the heart contained blood, and there was a very small clot in the left auricle, but the left ventricle was perfectly empty. The heart was quite as irritable to the galvanic stimulus as the muscles. From this experiment I infer, that the influence of cholera on the circulation is exerted in the same way as that of digitalis. With regard to the condition of the blood in cholera, I believe it to be the best under the peculiar physiological condition of the patient. Blood in the normal state is intended to be constantly circulated with a regulated amount of force. If this force be exceeded, we find that the fluidity of the blood is increased, that it acquires a brighter hue, and that it possesses the power of coagulating more firmly when stagnant. The converse of all this obtains with diminished force of circulation. To this alteration in the constituents of the blood is possibly owing the absence of fibrinous deposits in the heart or the vessels; the retention of its homogenous condition, and its adaptation for the re-admixture with water during reaction, so soon as the baneful effects of the poison shall cease to be exerted in the system. Thus to sum up:—In proportion to the force with which the circulation is controlled, either death results, or the blood is diminished in quantity, and altered in quality, by a secretion from the alimentary canal—the overwhelming effects of sudden congestion and fibrinous deposits being probably thus prevented.

[DR. KING, who had the charge of a large district during the prevalence of cholera, stated that after he had tried many modes of treatment for the stage of collapse, as Stevens' salines, calomel and opium, stimulants, &c., and found them equally unsuccessful, he adopted Mr. French's plan, placing a pail of cold water by the bed-side, and letting the patient drink *ad libitum*: at the same time he gave large doses of calomel, and when vomiting or hiccup came on, the patient generally did well.]

Med. Gaz. Dec. 17, 1847. p. 1074.

[DR. MASSY'S experience of cholera was gained during an epidemic which attacked Umballa, a large, dirty, and densely populated Indian city, in the year 1845:—an epidemic which ravaged Cabul, Afghanistan, and the Punjaub. Dr. Massy discriminated three forms of the disease. The first began by vomiting and purging of a greenish and brownish fluid, like the sporadic bilious cholera; but in a couple of hours the colour of the fluid changed, becoming whitish, and at the same time thinner and more copious. The administration of acetate of lead and opium in pills, opiate injections, or chalk mixture with opium and aromatic confection prevented the disease progressing. Further, Dr. Massy says:—]

The best means of allaying the vomiting is by giving effervescing draughts with tincture of opium. Keep the patient warm and in bed, and the next day, give a large emollient purgative injection; but if you do not check this disease in the beginning, you may depend upon its assuming the severe characters in a few hours, when you will have to resort to the more active means, which I will mention just now. The next form of the disease is that which I propose denominating the second species; it begins by purging and vomiting of whitish fluid, or mixed with green; the patient feels weak in the commencement; the skin at first is warm, but very soon the temperature wonderfully decreases; the pulse, at first quick and small, like the heat and skin, soon sinks below the natural standard; the tongue is white and dry; the patient asks for a drink, and in a second or two more begs for another.

You will also at this time perceive a change in the voice; I have fre-

quently seen this one of the earliest symptoms. The symptoms increase if not checked by medicine; cramps of the most violent description come on, chiefly in the feet, calves of the legs, thighs, and hands, which cause such excruciating torture, that I have rarely met even the most resolute able to resist screeching out. The skin now becomes quite cold, assumes generally the blueish hue so remarkable in cholera, and in a short time you will find it corrugated in almost every part, reminding one exactly of a woman's hand after washing for several hours. You will also be struck by the very peculiar countenance of the patient; the features are all sharpened; the eyes sunk in the orbits, but starting wildly; the tongue, if you place your finger on it, will be found quite cold; the patient's breath, if he breathes on your hand, is also cold. I shall never forget the first time a man in the advanced stage of cholera breathed on my hand—it actually made me start, it was so much colder than the atmosphere; all this time the patient screams out for cold water to drink; if you ask him is he cold, he says no, he is burning hot, he wants something cold to drink. I never actually tested how much a man in this state would drink, but fancy it would be something enormous, for he generally throws it up as fast as he drinks it;—sometimes, one would fancy, by a spasm of the œsophagus, for it has hardly time to reach the stomach, and still while he is vomiting he stretches out his livid hands, crying more, more! It is a remarkable feature in cholera that a patient seldom makes water during the existence of the disease; indeed I cannot call to mind having seen more than three or four instances of patients voiding urine in true Asiatic cholera; this was so remarkable at Umballa, that when a man did so his friends used to cheer him by telling him he was recovering; yet many of the other secretions exist. The skin secretes perspiration, the mucous membrane mucus, and the salivary glands saliva; for frequently have I seen a patient in the last stage of cholera, when his strength was completely exhausted, spit nearly across the room, and in the same way eject water while drinking. The change in the countenance is most wonderful in so short a time; men whom I have known intimately I have been unable to recognize. Restlessness is a very marked feature, it is almost the only sign of raving I have usually remarked; a patient will tell all about his symptoms, how he was attacked, talk of his will and all of his affairs in a most rational manner, yet he will constantly try, while no one is watching, to get out of bed, to turn from one side to the other, kick off the bed-clothes, and rail at everything; this species of case, if it ends fatally, generally does so in one or two ways—either the purging, and occasionally the vomiting, continue almost to the end, or else both cease, and the patient dies laboring under the symptoms of effusion on the brain; the pupils are dilated and fixed, it is impossible to rouse him, and his breathing is stertorous. The treatment of this case depends in the first instance on bleeding, and largely, if the patient's pulse is good, giving at the same time twenty grains of calomel with one of opium. This, I think, will be found the best practice, at least we found it so in the hospital of the 31st Regt. Often have I seen the purging and vomiting cease almost instantaneously. The calomel and opium in the first place act as an immediate sedative, and the bleeding relieves the circulation more than purging, removing a mass of blood instead of depriving it of its fluid part. After twenty minutes give ten grains more of calomel and half a grain of opium; this will further prolong the cessation of the symptoms, and in about an hour you will find the calomel acting freely on the liver, when quantities of bile will be passed by stool. When it has acted in this way for some time you may give opium, but not till then; of the various species of practice I have seen tried, I think a reliance on opium in this form of cholera the most faulty, you may exhibit any amount of opium, while a man is purging and vomiting a white fluid, without producing a single sign that the medicine has been swallowed,—the purging and vomiting still continue. If you happen not to see your patient (as is too often, the

case among soldiers) until the disease is farther advanced,—until his voice is changed, pulse a good deal reduced in strength and frequency, feature sunken, temperature reduced, skin blueish, tongue cool, &c.—I would recommend the bleeding to be smaller. You may be able to repeat it; but, as you draw blood, stimulate, give punch, brandy, or wine and water, or carbonate of ammonia. Apply friction, with stimulating and hot liniments to the extremities, warm sand-bags to the feet, sinapisms to the calves of the legs and pit of stomach; for if once you can raise the pulse, the chances in favor of recovery will be vastly increased. I have seen more than one case where the pulse improved after this treatment and again fell away, when it was deemed advisable to take blood a second time with decided advantage, stimulating at the same time.

Cases of this kind cannot endure the loss of blood, even in small quantities, without stimulants. This we learned by sad experience at Umballa, most of the cases we bled in this sunk without a rally. We were about to condemn bleeding in cholera, when once the symptoms of collapse set in, but we tried stimulants together with the bleeding, and found the practice the most successful of all we adopted. When the pulse and temperature of the body improved, and the purging was checked, we were in the habit of giving calomel and opium, as above: by this promoting the secretions, and, after they were fully established, checking them by opium, &c. Many are advocates for injections; but I always found, while purging existed, stimulating injections increased it, still draining the blood of its fluid parts, augmenting the disease. Every one has remarked the thick dark blood that flows from venesection in this stage of cholera; it hardly coagulates, and is devoid of serum; for though in the worst cases, where neither purging nor vomiting exists, the blood is found thick and dark, still it stands to reason that where they are present, the great quantity of fluid rejected must be eliminated from the blood, thereby increasing the tendency to this thickened condition.

It is recommended by many to allow the patient to drink largely of water, for the purpose of its supplying this deficiency. I have likewise given alkaline mixtures largely, both by mouth and injection, warm and cold, but never could perceive the least advantage result from either practice. When the cramps are distressing, I have seen anodynes and anti-spasmodics exhibited; but the best means of allaying them is by continued friction with stimulating liniments. I have also administered tonics, quinine, &c: but in this disease, as I have seen it in India, the practice was not successful.

The next, which I propose as the third form of the disease, is one I have met, though not near so frequently as the last; it is that in which the patient is brought into hospital in a state of collapse, and, if you inquire, you will find that he has hardly been purged at all, probably not vomited either, his features shrunk; eyes sunk in the head; skin, all over the body, corrugated; body quite blue; he has no cramps; tongue, skin, breath—all as cold as death; pulse not to be felt. He has no energy; asks merely for a drink of cold water; complains of burning heat at the epigastrium. This is the most fatal of all; but, thank goodness, it is not a common form of the disease, though the other species often terminate in this way.

All means of treatment that I have seen in this stage have been most unsuccessful, though I have tried nearly every remedy I ever heard of. If you open a vein, no blood flows, except a few drops, thick, dark and semi-fluid. The only means you can use are, turpentine, injections, mustard emetics; afterwards stimulants by the mouth, hot baths, sinapisms to the legs and epigastrium; blisters to the nape of the neck. Continued friction with turpentine warm; for you will find that the skin is hardly affected by blisters. In this stage, hot turpentine rubbed along the spine is not a bad means of stimulating. I have seen cold effusion practised in this and the former stage of the disease; and though I have heard some men of expe-

rience in India (whose opinion cannot be held lightly) recommend this mode of treatment as a stimulant to the circulation, I am sorry to say, in the cases tried on the thirty-first, it was not deemed a favourable mode of practice.

I recollect almost the last case of this sort I saw in India was that of one of the paymaster's clerks. I tried to bleed him from the arm, but no blood would flow. I then opened the anterior branch of the temporal artery; the blood flowed at first slowly in a small stream, as if coming from a vein, not *per saltem*. In a few minutes, however, it came in a stronger stream; the pulse at the wrist was soon perceptible, and the man finally recovered without a bad symptom. The change in the pulse, and the sudden improvement in this case, was the most remarkable I ever saw. Had I not frequently tried the same means unsuccessfully before, I should have vaunted this as a specific. I suspect it is from similar origins the different specifics for cholera have arisen. I think the treatment I propose will be found the most successful. I know it was the plan adopted by many medical men at Umballa during this epidemic, always bearing in mind that if the collapse is great you must stimulate while bleeding.

I have but one other subject connected with cholera to remark on, namely, the supposition of infection or contagion, neither of which, I am satisfied, exist. There were four medical officers in the 31st at this time, a number of apothecaries and apprentices, besides a great many servants connected with the hospital in constant attendance on the sick. I have, together with the other medical officers, spent hours by night and day in the cholera ward, and out of twelve or fourteen Europeans and some forty natives, none had a symptom of the disease. I think one of the strongest arguments that can be adduced in favour of non-contagion, is the fact that the natives in India, who have more opportunities than any other persons of forming a correct opinion, believe not in it, and if they had a shadow of belief, the Bengal coolies, who are proverbially the greatest cowards in existence, would not sit as they do quite unconcernedly alongside of a dying man. Many times have I been obliged to kick the coolies, as, with their chudders wrapped around them, they have fallen to sleep while rubbing a man suffering from cramps, with their heads against the bed upon which the patient was lying. I have seen two epidemics of cholera in India, besides many sporadic cases, and never knew an instance where contagion for a moment could be substantiated.

Med. Gaz. Jan 28, 1848, p. 144.

[The following is Mr. McCoy's summary of his experience, on the subject :—]

1st. That stimulants given internally, in any stage or form of malignant cholera, of whatever they may consist, or in what doses exhibited, are decidedly injurious. 2nd. That external stimulants, as frictions with mustard, &c., to the patient's limbs, are of no use whatever. 3d. That attempts to raise the temperature of the body by artificial heat are always useless, greatly distress the patient, and, I am disposed to think, sometimes do harm even. 4th. That cold drinks, and cold water particularly, are the most useful, and by far the most agreeable that can be given. 5th. That calomel has been, by far, the best medicine I ever used, or seen others use, and that I have never seen injury from its exhibition, in any quantity, in cholera. 6th. That the first appearance of bile in ejections from the stomach or bowels is evidence of recovery. 7th. That consecutive fever, or local inflammation, is no part of cholera.

Dub. Med. Press, March, 1, 1848, p. 140.

[In his Lectures on the Practice of Medicine, Dr. WATSON, when speaking of the various plans of treatment adopted in Asiatic Cholera, observes :]

But when the regular symptoms peculiar to the severe form of cholera had set in, medicine, I repeat, had very little influence upon it; and accordingly, as might have been expected, a hundred different cures of the disease were announced, most of them all but infallible. Some persons held that timely bleeding would save the patient; others relied confidently on mustard emetics. Hot air baths were manufactured, and sold to a great extent, to meet the apprehended attack in that manner without delay. Certain practitioners maintained that the disease was to be remedied by introducing into the system a large

quantity of neutral salts, which were to liquefy and redden the blood, and to restore the functions of the circulation. But of this practice it was said in a sorry but true jest, that however it might be with pigs or herrings, *salt*ing a patient in cholera was not always the same thing as *curing* him. In a great number of patients the blood was mechanically diluted by pouring warm water, or salt and water, into their veins. Some physicians put their trust in brandy, some in opium, some in cajeput oil, which rose to I know not what price in the market; some, again, in calomel alone. *Med. Gaz.* April 7, 1848, p. 584.

[DR. TURNBULL, after referring to the impracticability of raising the diminished temperature of the body in cholera patients by ordinary means, says:]

My plan of treatment consists in endeavoring, by means which can be repeatedly applied without excoriation of the skin, to re-excite an action of heat on the surface of the body, and thereby to restore the lost balance of circulation and nervous energy. For this purpose I employ an extract made of capsicum, with alcohol, reduced to the consistence of jelly. Three drachms of the extract to be well mixed with six drachms of purified lard; the patient to be well rubbed over the abdomen, heart, and calves of the legs, several times a day; and at all times if there be any coldness over the surface of the body, or when spasm takes place in the abdomen or calves of the legs. The rubbing ought to be continued until such time as the patient expresses that the heat is intolerable. Another form of the employment of capsicum is the following embrocation:—Concentrated tincture of capsicum; viz., capsicum pods, four ounces; rectified spirit, twelve ounces; macerate for a week, and strain. To increase its energy, upon the nervous system, when required, I add two or four grains of delphinia, or veratria, to the tincture. Another method of obtaining the advantages of capsicum, speedily, and without much expense,—and which may be considered a household recipe for cholera until medical assistance can be obtained, is to boil four ounces of capsicum in a pint of olive oil, for six hours, and strain. To free the capsicum from the chloride of sodium with which it is generally united, it is necessary to add water, and strain, previously to mixing it with oil, otherwise it will produce vesication. The chloride of sodium is the chief, if not the only, material whereby vesication is produced when capsicum is used externally.

The advantages that the capsicum possesses over many other stimulants is, that it produces no vesication, and therefore can be repeated very often. Another advantage is its very permanent and certain effects; and there is no part to which it may not be applied without injury. The capsicum will be found a most powerful rubefacient; it assists in restoring heat and circulation to the surface, while the delphinia and veratria united with the capsicum restore the lost nervous energy. This plan of treatment can be pursued without much disturbance to the patient, the movement of whom is attended with the worst consequences. I have often used the different preparations of capsicum with or without veratria and delphinia, in congestive fevers, with very good effect, and also in cases of cramp or spasm. This stimulative plan does not interfere with the administration of other medicines, but on the contrary, greatly assists in restoring the lost power of the nervous energy and equalizing the balance of circulation.

Lancet. Jan. 29, 1848, p. 119.

6. On Flatulence.—By Dr. ROBERT DICK.

Flatulence of the stomach and bowels has two principal sources—the liquid and solid alimentary ingesta, and (as some assert) exhalation from the mucous membrane. We must frankly own that we have yet met with no grounds other than conjectural for the latter view; we are aware of no *facts* that prove it. True, indeed, John Hunter—an authority not lightly to be questioned—supposed exhalation from the mucous surface to be an occasional source of gaseous distention; still we must repeat our opinion, that the alleged fact rests on no *positive* evidence; while there are not a few strong presumptions against it, into the consideration of which, however, it would not be expedient to enter now. Suffice it only here to observe, that if the rapid meteorismi or pneumatosis

which arise in the last stages of adynamic fevers, &c., seem to prove the fact of the sudden secretion of gases by the mucous membrane, is it not just as likely, we would ask, that the phenomena named are due to the suspension of secretion and nervous action in the stomach and bowels, and the opportunity thence afforded for the play of the ordinary chemical affinities in the aliment or *excretions* in the stomach or intestines—nay, perhaps, to some morbid secretions, the consequence of depressed or dormant vital power, and which actually favour the occurrence of ordinary chemical action in the contents of the bowels, and the thence resulting extrication of gases? This, at least, is a more *probable* supposition than the other.

On the same principle, I would account for the air eructated in gastritis, hepatitis, &c. The vital and conservative power of the mucous membrane being in these cases greatly reduced, while, at the same time, the temperature of the stomach is greatly augmented, the play of *non-vital* chemical affinities is favoured—the stomach's own *infra-natural* secretions become the ready subject of these. A third source of gaseous fluid in the stomach and intestines may be named, though we consider it as of little importance—namely, the atmospheric air swallowed in the acts of mastication and deglutition, and mechanically contained in the articles eaten, as, for example, in the pores of bread, &c.

The gases of the stomach are principally nitrogen, oxygen, and carbonic acid, nearly in the proportions of atmospheric air. The gases of the intestines are those now named, and, in addition, carburetted hydrogen, hydrogen, and occasionally sulphuretted hydrogen. The intestinal gases are further often loaded with vaporous particles of the foetid contents of the bowels.

Flatulence, as we have formerly remarked, is often owing to an inefficient action of the liver, and a deficiency of bile in the intestines. Whatever promotes the hepatic secretion tends to remove flatulence of this origin; hence a few drops of colchicum wine are often effectual. Still more sure are minute doses of mercury. An ante-dinner and an evening pill, consisting of a grain of the blue pill and three of extract of rhubarb, acts with wonderful good effect in many cases of this kind, in which, along with flatulence, there are slight constipation, yellow-furred tongue, ill-tasted mouth, &c. As in gastro-duodenitis, there is often, from the vascular tumescence of the duodenal mucous membrane, a constriction, and sometimes complete temporary occlusion of the mouth of the ductus communis choledochus, with, of course, interruption to the discharge of bile; hence, in part, the flatulent eructations, &c., which accompany gastro-duodenitis. It is far from unlikely that the pancreatic duct and secretion are often affected in a similar way; but for some unaccountable reason, it has not pleased pathologists of any age to pay much attention to this unobtrusive viscus—some seeming even to think that it, as well as the spleen and supra-renal capsules, are not necessary, because not understood.

Treatment.—When the tongue is pale, when there is no tenderness on pressure at the epigastrium, or in the right hypochondrium, when there is no thirst, no dry heat of skin, and no quickness of pulse, flatulence requires carminatives, bitters, and even stimulants. Thus the patient may be directed to use freely any of the following waters: cinnamon, fennel, cassia, pimento, peppermint, pennyroyal, mint, Cologne, lavender, caraway, aniseed, dill, balm; to these, some of the respective tinctures may be added. With the carminative waters just named, one or more of the following bitters may be given—camomile, quassia, columba, absinthium, rhubarb, to which may be added valerian, castoreum, and camphor. As an expellent of flatus existing in the bowels, assafoetida, or oil of turpentine, the former given by the mouth, or in injection, the latter in injection, are superior to all things else, excepting, perhaps, the infusion and spirit of armoracia.

Secondly. If flatulence is accompanied with a dry and preternaturally red tongue and fauces, with thirst, heat of skin, tenderness of epigastrium, scanty and high coloured urine, heartburn, &c.—in short, with symptoms of inflammatory irritation of the gastro-duodenal mucous membrane, then alteratives are

clearly indicated, or rather such substances as promote the secretions of the mucous membrane; these are ipecacuan, sulphur, potassio-tartrate of antimony, the various preparations of mercury, magnesia, iodine, nitrate of silver. These we would be disposed to give a trial to successively, almost in the order in which we have named them. But a great variety of other means may be tried, and among these the following, in those cases in which flatulence is accompanied with obvious torpor and fulness of the liver, as well as with gastric irritation. The wine of colchicum, for example, may be given with a few grains of the sulphate of potass, or if there are acid eructations and heartburn, with carbonate of magnesia, the infusion or tincture of arnica may be given in the same combinations, and so may the powder and extract of cusparia. In short, instead of perplexing our minds with the confused subdivisions of authors, whose classifications betray they had no clear and scientific notions of the proper treatment of flatulence, the simple point to be ascertained and kept in view is, whether flatulence (always a mere symptom) is or is not accompanied with inflammatory irritation, is or is not attended with stomachic debility—and according as we decide these queries, we adopt the former or latter modes of treatment above mentioned.—*Lancet*, Nov. 20, 1847, p. 545.

In an interesting article in the November Number of the *New-York Journal of Medicine*, on the *Medical Topography of the City of Mexico*, by ROBERT NEWTON, Med. Staff Officer, U. S. A., we make room for the following extracts on the "Constitution and Diseases of Mexicans:"

Ed.

7. Constitution and Diseases of Mexicans.

In Stature the Mexicans are very inferior. The Indians are several inches below what is regarded as mediocrity with us. The inclination to degenerate insize, is remarkable in the Mexican horse, jackass, and their product, the mule; and the dog is smaller than the animal of the same species in our own country.

Both the Spanish and Indian Mexican is of a nervous temperament. The body and mind are highly sympathetic with each other. The sensibility of the nervous expansions is keen. The remark which has been made by some of our officers, that Mexicans entertain a great tolerance of pain, is, I am convinced, erroneous. Though the Indian from his taciturnity and reserve, makes an effort to restrain an exhibition of his sufferings, the involuntary expressions which I have witnessed, convince me that his feelings are very acute. Nervous complaints are exceeding common though more especially with the white portion of the community.

The Indian is distinguished for a capacious chest, and a small abdomen.

Among both sexes of the Creoles and among the females of the Indian race, the adipose tissue is apt to present a remarkable development.

The Indian exhibits a fine instance of muscular development. The strength he is capable of exerting is superior to that of an European laborer. His great power is particularly manifested in the carrying of burdens. A large chest, small belly and ample muscle, constitute the conformation of men who, without fatigue, can march in a day twice the distance the European is able to accomplish.

The natural intellect of the Creole is certainly good, that of the Indian very contracted. Humboldt thinks, that when the mind of the latter is educated, it manifests a tendency "to subtilize and seize the finest differences in the comparison of objects." He at the same time ascribes to him an almost utter destitution of imagination. Both races possess social feelings susceptible of great refinement. Though their semi-barbarous condition and the unprincipled character of some of their chiefs, have stained their national history with examples of savage ferocity, the real character of their people is gentle and compassionate. One of the greatest of their faults is *too little independence* of thought and feeling.

The *longevity* of the Spanish portion of the inhabitants is not great. It is quite rare to see a man of very advanced age. The Indians live longer. The

annual number of births in the capital, in the mean, for a term of one hundred years, was 5930, and that of deaths, 5050; so that, apart from the immigration, the population has had very little tendency to increase.

I now proceed to the consideration of the particular diseases as manifested among the inhabitants of this city.*

Intermittent Fever is by no means a fatal disease. The season of its greatest prevalence is that of April and May.†

Remittent Fever is almost totally unknown.

Typhoid Fever is very common in all seasons, but is most so in April and October. It has the pathognomonic characters which were first noticed by the pathologists of Paris. In the 3d, 9th, and 14th Regiments of Infantry, since their arrival in this city, the fever has prevailed to a very considerable extent.

I arrived here early in December, when I joined the regiment of mounted riflemen. Previously there were a number of cases in the regiment, and the disease had proved fatal to my zealous and indefatigable predecessor, Assistant-Surgeon Suter. But after him, no others in the regiment were affected until the weather became mild.

The cases of typhoid fever which I have seen in the Mexican hospital San Hipolito, and those which have come under my own charge, have been marked by pyrexia; a rose-colored lenticular eruption; tenderness of the bowels, and particularly in the region of the ilio-cæcal valve, with a gurgling sound when pressure is made in the latter region; looseness of the bowels; headache; injected conjunctiva; deafness; more or less stupor; a brown tongue, and a collection of sordes on the gum and teeth; with a belief of the patient that little or nothing was the matter with him. In the most severe cases, there were delirium, subsultus tendinum and involuntary evacuations. Some were accompanied with bronchitic or pleuritic complications. The treatment I have pursued, (and which, in the limited number of cases that have been under my charge, has always been successful,) has been, to shave the head and apply to it wet cloths, institute a mucilaginous diet, purge the bowels with castor oil once, or more frequently, (if there is much pain in the bowels and the excretions are vitiated,) apply leeches and poultices to the belly, and administer neutral mixture so long as there is preternatural heat and dryness of the skin. If the irritation of the alimentary canal have not been great, I have sometimes given the tartrate of antimony and potassa, with lemonade. When the pleura is affected, I have cupped repeatedly, and endeavored to bring the system under the influence of mercury. I have been studious to cause in the patient's rooms, a constant renewal of the atmosphere, and, during the day, have sometimes removed him to the open air. The shirt and bed-clothes have been frequently changed.

To the *Smallpox* is due the terrible reduction which the Indian population has experienced in some parts of this country since the first entrance of the Spaniards. The disease appears almost exclusively in the form of an epidemic, occurring at very marked intervals. Its ravages were awful in 1763, and still more so in 1779, when in the capital alone it destroyed more than 9000 persons. "A great part of the Mexican youths were cut down that year." In consequence

* For my information on this subject, I am most indebted to Dr. Galenzowsky, and Dr. Navarro. The former is one of the most distinguished of the civil practitioners in the city, and of very high professional attainments. The latter, Professor of Surgery in the Military Hospital of Instructions, is a young man of great zeal and talent. In practising with him, at the San Hipolito Hospital, surgical operations on the dead subjects and autopsic inspections, I have derived much benefit from his suggestions.

† It is remarkable that the garrison of Chapultepec were the greatest sufferers from intermittent of all our troops in the valley; at least the fact holds in the two months for which I have had the data necessary to make the comparison. Yet Chapultepec is by far the highest site occupied by our army in the valley. The explanation is, that to the north-east of that place, lies a large marsh, to wit, between it and the city; and that the prevailing winds are from the same direction.

chiefly of the introduction of variolous inoculation, the epidemic of 1779 was less destructive. The *vaccine* inoculation was first introduced in 1804, by Thomas Murphy, who brought the matter from the United States. It was readily submitted to by the inhabitants, who had previously been convinced of the value of an analogous process—inoculation with variolous matter. The introduction of vaccination was matured by a Spanish medical commission, which was dispatched by the Government to carry the process into Mexico and other Spanish colonies. Original matter has been obtained from the udders of cows in Atlixco and near Valladolid. Although the smallpox, now that its extension and violence are controlled, by vaccination and rational treatment, is divested of much of its fearfulness, in the *history* of Mexico it stands pre-eminent as the instrument of death. Terrible must have been its ravages to have given occasion to the statement of Motolinia, that in 1520 it carried off one half of the inhabitants.

Yellow Fever does not appear in the capital of the Republic, but the *Matlazahuatl*, a disease described as resembling the former, has raged here as an epidemic in times long past. It occurred in the years 1545, 1576, and 1736. Torquemada estimates the mortality caused by it in the first mentioned year at 800,000, and in 1576, at 2,000,000. It is remarkable for not attacking whites and those in whose veins is mingled the blood of the white man.

Diarrhœa and *Dysentery* are, when taken aggregately, charged with a greater mortality than any other disease. Indeed, this mortality amounts to nearly one-fifth of the whole.

That form of *hepatitis* which results in abscess is very common—these abscesses often discharge several pints of pus. It has been found best to make a *very free* external opening to the abscess; nor is it thought necessary to be solicitous about the admission of air: which is not thought to be injurious. At least, the advantage accruing from maintaining a passage constantly free for the issue of pus, more than counterbalances the injury from the air. In this way, I learn from Dr. Galenzowsky, more than one-half of the cases are cured.

Catarrh and *Bronchitis* are diseases of ordinary occurrence; but *Phthisis Pulmonalis* is very rare. *Vesicular Emphysema* is frequent.

Mexicans have already been stated to be subject to nervous affections. Among these may be specified cephalalgia, epilepsy, neuralgia, paralysis, and chorea.

Calculus is very seldom met with in the city of Mexico. In some other parts of the republic, it is more common.

In respect to *venereal diseases*, they may be stated to be one of the greatest banes of the country. There are few males of middle age in the city of Mexico, who have not been subjects of this disease. Nearly all of the lower class of females, also, have been affected. This is at once an effect and illustration of the immorality of the population. The chancre among the Mexicans is generally of the Hunterian character, and apt to be followed by secondary symptoms. I have frequently heard our medical officers affirm the difficulty of managing venereal affections in this place.

Dropsies are common. *Hemorrhages*, though the fact may seem strange in a country where the external pressure of the atmosphere is slight, are rare.

The inhabitants of this city are much subject to *Pericarditis* and *Endocarditis*, and also to *organic affections* of the *Heart*. Ossification in the vascular system, however, is said not to be unusually frequent.

But one of the most strikingly common diseases in this place, is *Senile Gangrene*. Being present recently at an amputation of the thigh, performed on account of this disease, I extracted the popliteal artery, which clearly demonstrated, in part, the pathology of the affection. In the cardiac portion of the artery is a coagulum of blood; but situated in the peripheral portion, and separated from the coagulum by a narrow section in a state of transition, is a cord of white fibrin. It is to be supposed, that the inner coat of the artery being inflamed caused a coagulum of blood, (as in the analogous case of endocarditis,) which was succeeded by fibrin.

The poor are much affected with *indolent ulcers*, generally situated on the legs. These ulcers are a frequent cause of death.

In walking the streets, one constantly meets persons with *affections of the Eye*. The most common are opacity of the cornea, fibrinous occlusion of the pupil, and inflamed and hypertrophied conjunctiva. Although many cases are exhibited for mendicant purposes, enough others are seen to convince a stranger that the number is extraordinarily large. Inflammations of the cornea and iris it is well known must be treated promptly and judiciously or the sight will almost certainly be lost by the exudation of coagulable lymph. Now there is probably no more improvident people in the world than the poor of this city. Need we wonder then at the opacities and occlusions? Besides, when we reflect that *ibritis* is frequently produced by syphilis, and that conjunctivitis, as well as inflammation of deeper tissues, is a result of gonorrhœal inoculation and of catarrhal influence, and moreover, that syphilis, gonorrhœa, and catarrh, are here exceedingly prevalent, we have a most satisfactory explanation.

Considering the fact that close study is not an ordinary phenomenon among Mexicans, *Myopia* is found in the cities of the table-land to an extraordinary extent.

To *diseases of the Skin*, the constitution of the Indian seems rather disinclined. There is, however, one horrible cutaneous affection which is frequently encountered. This is the *Elephantiasis Græca*. There are three forms. The first is characterised by tubercles generally varying from a quarter, to three quarters of an inch broad; disposed to squareness of shape; projecting, red or livid in the beginning, susceptible of change to a bronze hue; sometimes indolent, at other times more sensitive, and accompanied by a swelling of the subcutaneous cellular tissue; preceded by spots of a variable red; beginning usually at the root of the nose, and extending over the head and upper extremities, then affecting the lower, but seldom occupying the trunk. This is called the tuberculous form. The second, called the anæsthetic, is characterised by a want of sensibility in the extremities of the limbs, and by absorption of the bones. I have seen the auricular and annular fingers as completely removed by absorption as by amputation at the mecatarpo-phalangeal articulation, and without any semblance of a cicatrix. The third form was first described by Dr. Lucio, physician to the San Lazaro Hospital in this city, for the accommodation of patients of this disease. It consists principally in the production of red and painful discolorations, which commonly terminate in ulceration. The subjects of this form are called *lazarinos*. Two or more forms sometimes co-exist in the same individual. Dr. Lucio has discovered in the inspection of such as die from this disease, the frequency of certain alterations in the spleen. The first and second forms may be regarded as incurable. The patients, after having been affected many years without any material derangement of general health, usually die with diarrhœa; and in such cases the mesentery is found to contain deposits of tubercular matter. The San Lazaro Hospital was erected in the year 1811. In the year 1844, it had received in all, 82 women and 123 men. Elephantiasis affects only the poor, and particularly such as are exposed to strong heat and humidity. All the patients in this city come from the western side, though I am not aware of any explanation of this fact. The subjects are from 15 to 40 years old.

Looking for the local *causes* of disease, the mind immediately recurs to the large expanse of surface in the valley of Mexico, which is annually flooded by the rains and dried during the dry season. The portion subject to these changes is not less than one-tenth of the whole valley. Here is a good laboratory for the production of intermittent miasmata. These, however, do not appear to be evolved in the highest amount until the close of the dry season, when the heat is greatest, and the superimposed layer of water has been evaporated.

Another cause of disease is the humidity of the atmosphere during the wet season, when heavy rains occur every day, frequently flooding the streets of the city. The perspiration of the body not being duly evaporated, it becomes

necessary that the supply of heat should be diminished to prevent the temperature of the system rising above the normal standard. This supply is founded in the conversion of the carbon of the system into carbonic acid gas, and can be diminished in several different ways. It may be effected by the deposition of carbon in the form of fat, a process, however, which nature is frequently indisposed to adopt. Then the supply of carbon to the system may be curtailed by reducing the amount of food, which is the appropriate mode of overcoming the difficulty. Nature indicates this course by lessening the appetite in such cases. But when the constitution is disinclined to the formation of fat, and when the appetite is artificially stimulated as it is in Mexico by the constant and excessive abuse of alcoholic drinks, red pepper and garlic, then the superabundant carbon remains only to be eliminated by the liver as an element of the bile. The excitement of the liver necessary for this extraordinary labor, disposes it to inflammation. The increased vascularity of the intestines required for a supply of blood by the portal circle, sufficient for the extraordinary secretion of bile, renders the intestinal canal also predisposed to inflammation. This predisposition of the canal is increased by the irritation attending the reception of the unusual amount of bile and of fruits, here almost unlimited in variety, and some of which are not very healthful. Now if a man in the condition just described, with his system overheated and debilitated, go out doors into the rain and wet his feet with the water which has flooded the streets, the surface of the body is too rapidly cooled, and the liver or bowels, already excited, become almost unavoidably the seat of inflammation. We have thus no difficulty in understanding why hepatitis, dysentery and diarrhœa are here so frequent.

The very great difference in the temperature of day and night, and of sunshine and shade, with the thin mode of dressing, is a sufficient cause for the prevalence of catarrhs and bronchitis, as well as other internal inflammations. It is true that the contiguity of the lakes has a tendency to equalize the temperature of day and night; but there are other causes, which render it extremely unequal. These are, the clearness and stillness of the atmosphere; the want of trees and other projecting bodies; and finally, the rareness of the atmosphere, which, in consequence of this rareness, has less power to maintain an equilibrium, in the heat of the body, and leaves it more exclusively exposed to the influence of radiated caloric. The latter agency, in connection with the thickness of the walls of houses, renders the change of temperature very great in passing from the sun into the house. Drafts of air, however, have, for the same reason, less potent influences here than in a denser atmosphere.

To the rareness of the atmosphere, also, as may be reasonably imagined, should be attributed the prevalence of vesicular emphysema.

8.—*The question of Amputation in Gunshot Wounds.* By M. MALGAIGNE.

This question has formed the subject of a most interesting debate in the Academy of Medicine, Paris. MM. Roux and Baudens having concluded their communications.

M. Malgaigne observed, that the point towards which it was more especially his desire to call the attention of the Academy, was the treatment of fractures of the femur by war projectiles. It was a generally received opinion, more particularly promulgated by military surgeons, that such fractures required amputation. This was the practice of Ravaton, one of our great military authorities; Larrey, less exclusive than his predecessor, thought the extremity might be still preserved when the shot had only occasioned a simple fracture of the lower fourth or even third of the femur; but that all fractures occurring higher, in consequence of a gunshot wound, absolutely required amputation. M. Malgaigne then recalled the opinion of Ribes, who inclined towards considering all injuries of the femur, occasioned by musket balls, as cases for amputation.

M. Malgaigne himself had adopted at one time this mode of thinking, and had applied it upon a field of battle; but the results were most unsatisfactory and in the Polish campaign he had lost all the cases of amputation performed upon the thigh after gunshot wounds. On his return to France, M. Malgaigne had endeavoured to explain to himself this frightful mortality; and, on reading over the memoirs of M. Ribes, he was struck with an important fact, which that author honestly acknowledged:—"Out of 4000 invalided soldiers," said M. Ribes, "he had not found a single case of injury of the femur by shot. This was a proof, in his opinion, that all the men who had suffered from such wounds had died." But, on the other hand, amongst the 4000 cases, M. Ribes had not found a single case of amputation of the thigh—a fact which proved to M. Malgaigne that all these operations had been fatal, and that amputation did not afford more chances of preserving the lives of the wounded than the opposite practice.

On inquiring further into the details of the question, M. Malgaigne had arrived at this conclusion, that the same doubts might with justice be applied to all amputations performed in the treatment of injuries by war projectiles.

The question of immediate amputation might be said to be one of the most important points of modern surgery. In the ancient Academy of Surgery it was the object of a long debate; and Boucher had said that two-thirds of the cases of amputation terminated fatally.

After Fontenoy, out of 300 cases, Faure asserted that only thirty or forty were cured.

Bilguez states that, during the seven years' war, scarcely one or two cases had been saved out of numberless operations.

On the other hand, M. Malgaigne would lay before the Academy more recent statistics of a very different tendency.

Fercoq said that, of 60 primary amputations, only two cases did not recover, *i. e.*, 1 out of 30.

Percy was not quite so fortunate; of 92 amputations of the leg, thigh, and arm, 6 cases died, *i. e.*, 1 out of 15.

Guthrie, at New-Orleans, performed 45 immediate amputations; 7 cases terminated fatally—1 out of 7; at the battle of Toulouse, 47 amputations, 9 deaths—1 out of 5.

The English forces during the Spanish war presented 291 amputations; 24 deaths—1 out of 8.

Del Signore, at Navarino, 31 amputations—1 death.

The English surgeons at Abukir and Camfordomer reported 30 amputations; all were successful.

Larry, on the 27th and 29th Brumaire, 13 amputations—2 deaths.

M. Malgaigne had further drawn up a list of the amputations performed in the hospitals of Paris, for traumatic lesions, during a period of ten years—from 1836 to 1846. These statistics were instructive. 165 amputations had been performed upon men, and 17 upon women. The mortality had been 107 for the former, and 10 for the latter. How were these amputations subdivided? The following was their classification for the men:—

	Amputations.				Deaths.	
Thigh,	-	-	-	44	34	over 3-4
Leg,	-	-	-	67	42	near 2-3
Foot,	-	-	-	8	5	over 1-2
Shoulder	-	-	-	7	7	
Arm,	-	-	-	29	17	nearly 2-3
Forearm,	-	-	-	10	2	" 1-5

Thus, in Paris, in the best hospitals, under the care of the first surgeons in the world [!], the average mortality of primary amputations was equal to two-thirds of the cases.

These were certainly unexpected results, and must, doubtless, cast some doubts upon the numerous successes which had been previously enumerated.

Circumscribing the question to narrow limits, *i. e.*, to the results of amputation in fractures of the thigh or leg, M. Malgaigne could bring forward an equally important document. In 1830, Dupuytren had under his care 13 fractures of the thigh in which he did not operate; 5 cases cured, 7 died; another was operated at a later period, and proved fatal.

In fractures of the knee or leg, Dupuytren performed 5 primary amputations of the thigh, 3 patients died; 4 secondary amputations, 4 deaths.

For the other fractures of the leg, in which the same surgeon did not amputate, 14 were fractures of both bones, 8 died; 2 of the tibia, 1 death; 2 of the fibula, 1 death. He performed two primary amputations of the leg: both cases terminated fatally.

Such were [continued M. Malgaigne] the precise documents existing in science; they tended to prove that the opinion of military surgeons, relative to the advantageous results of primary amputations, did not rest upon a very solid basis.

The following general conclusion might be adopted, *viz.*, that, in attempting to preserve the limbs of the wounded, the surgeon did not cause them to incur any greater risks than if amputation were performed.

All these considerations had greatly modified M. Malgaigne's opinions on the subject, and had changed his practice in similar cases.

The events of June had furnished him with a melancholy occasion of verifying again the correctness of his newly adopted views; and this led him to expose before the Academy the results he had obtained at the Hôpital Saint Louis, in a service where an enormous number of wounded had been admitted.

He would acknowledge at once, that in some wounds all debate about the propriety of amputation should be set aside; thus in gunshot wounds of the hip or knee joints the operation was evidently unavoidable. These cases excluded, the following had been the result of the fractures, in which M. Malgaigne had refrained from amputation:—

5 fractures of the thigh:	2 recovered,	2 deaths, 1 secondary amput.
6 fractures of the leg:	2 doing very well,	4 deaths.
2 fractures of the tibia:	2 ditto.	
4 fractures of the fibula:	2 ditto.	2 deaths.
3 fractures of the arm:	1 recovered,	2 deaths.
5 fractures of the forearm:	5 ditto.	
2 fractures of the metacarp:	1 doing very well,	1 death.

27

15

11

1

M. Malgaigne had performed only one primary amputation, which he was almost ashamed to acknowledge, after the energetic reprobation of which it had been the object from M. Roux, an amputation of the elbow. It might be pleaded in extenuation that the patient had recovered. M. Malgaigne would acknowledge he did not understand M. Roux's motives for proscribing this operation.

Thus, out of 17 fractures of the thigh and leg treated without operation, 8 cures had been obtained. Dupuytren, out of 31, had obtained 13 recoveries. M. Malgaigne's results were, therefore, more satisfactory than those of M. Boucher, who considered that primary operations sacrificed the lives of two-thirds of the patients.

M. Gosselin, Dr. Malgaigne's colleague at St. Louis, had also abstained from operations. The following were his results:—

3 fractures of the thigh:	1 gives hope, 2 deaths.
3 fractures of the leg:	1 uncertain, 2 deaths.
4 fractures of the tibio-tarsal joint:	1 doing well, 1 death, 2 secondary amput.
2 fractures of the shoulder	2 ditto.
2 fractures of the arm:	2 ditto.
3 fractures of the elbow:	1 uncertain, 2 sec. amput., 2 deaths.
8 fractures of the forearm:	8 well.

25

16 cases of success.

The difference of the mortality amongst the insurgents and soldiers had been as follows :—

Out of 17 fractures of thigh and leg—

5 insurgents: 4 deaths, 1 recovery (fracture of the thigh).

12 soldiers: 4 deaths, 7 doing well, 1 amput. cured.

This considerable mortality amongst the insurgents depended, in some measure, of course, upon the moral depression consequent upon their defeat, but also upon the interrogations which those unfortunate men were submitted to, without the authorization of the surgeons to whose care they were confided.

The satisfactory results were due (said M. Malgaigne) to various causes, amongst which it was only fair to place the special treatment employed. He avoided, as much as possible, all scarifications and incisions, employed only the simplest dressings, and gave food to the patients as soon as any appetite was present. With regard to venesection, he scarcely ever employed it; being a pupil of Broussais, he formerly recommended the practice, but had been deterred from its use by the fatal effects he had witnessed. A document, which unfortunately had never been published, and which had been prepared by the orders of the administration of hospitals, was most peremptory in this respect. It was the statistical account of the mortality amongst the wounded admitted in 1814 into the hospitals of Paris. In these tables French, Prussian, Austrian and Russian subjects had been entered, together with a statement of the mode of treatment. All except the Russians were submitted to a severe regimen. The mild cases amongst the latter received what was called a portion of food; others received the half portion, and this *half* allowance consisted of—

Bread	-	-	-	-	-	1 lb.
Meat	-	-	-	-	-	$\frac{1}{2}$ lb.
Vegetables	-	-	-	-	-	$\frac{1}{2}$ lb.
Wine	-	-	-	-	-	12 oz.
Brandy	-	-	-	-	-	12 oz.

These figures might naturally astonish the meeting; but the tables of mortality would still further surprise the Academy. The mortality was—

For French soldiers	-	-	-	-	1 out of 7
— Prussian —	-	-	-	-	1 — 9
— Austrian —	-	-	-	-	1 — 11
— Russian —	-	-	-	-	1 — 27

These were eloquent ciphers; they had been sufficient to convert M. Malgaigne, who, it was true, did not allow brandy to his patients, but a reasonable amount of wine.—*Monthly Ret.*, Sept. 1848, from *Dr. M'Carthy's Report in Med. Times*, Aug. 19.

9.—Diseases affecting the System generally.

ANASARCA.—General.—Give digitalis in gradually increasing doses, until it acts decidedly on the system, watching the patient narrowly at least three times in the twenty-four hours. Begin with: (℞. Tinct. digital m xv.; potas acet. 3 i.; sp. juniper co, 3 ij.; decoc. scoparii co. 3 xiv. M.) twice a day: and increase the quantity of digitalis by five drops daily, until its effects are marked. Then give wine, ammonia, and camphor, with animal food. (Mr. M'Donald, p. 82.)

Give diosma (Buchu) thus: ℞. Infus. diosmæ 3 vj.; sodæ bicarb., potas. bicarb., potas. nitratis aa. ʒij.; syrup. aurant, 3 vj.; tinct. scyllæ 3 ij. M. 3j. ter die. (Anon. p. 32.)

ASCITES—Buchu is useful when given with such other remedies as the case may seem to require. (Mr. Hoskins, p. 33.)

ASIATIC CHOLERA.—This disease is probably “a congestive ague of quotidian type.” The collapse is not what is usually understood by that term; it arises from an active cause, from a *spasm of the capillary circulation*, and thus differs materially from the collapse produced by other and more usual causes.

The spasm puts a stop to the action of the capillary circulation, and drives the blood into the great interior veins—hence the pain about the precordia, and oppression of the heart, with congestion of the spleen, stomach, &c. Hence the treatment is to relax this spasm of the capillary circulation by *early blood-letting*. If this is done at the commencement of the congestion, it not only relieves the internal veins, but relaxes the spasm. Bleeding, however, must be adopted *early*, and not when the pulse is recovering, nor in the last stage; this would produce fatal collapse. It may seem contrary to all sound practice to bleed thus when the patient is pulseless, but in doing it in the early stage of congestion and collapse, consists the whole success. The medicine most successful is the following: Quinæ disulph. gr. xij. to ℥i.; ferri sulph. gr. ix. acid sulph. dil. m xl. aquæ Oiss.; a wine glass full every quarter of an hour or hour. Do not give diffusible stimulants unless in combination with bleeding, or at the very outset; not after congestion is fully established. Employ cold applications rather than hot ones. (Dr. C. W. Bell, p. 95.)

Use neither internal nor external stimulants, nor artificial heat; but give calomel, and let the patient drink cold water. The first appearance of bile in the ejections from the stomach and bowels, is evidence of recovery. (Mr. M'Coy, p. 111.)—(Dr. King p. 108.)

“When the regular symptoms peculiar to the severe form of cholera had set in, medicine, I repeat, had very little influence upon it.” (Dr. Watson, p. 112.)

Rub the patient well with a preparation of capsicum, over the abdomen, heart, and calves of the legs, several times a day, or at any time when there is coldness of the surface, or spasm; continuing the rubbing each time, till the patient feels the heat intolerable. The advantage of capsicum is that it does not blister. The following preparations may be used: Extract of capsicum made with alcohol, reduced to the consistence of jelly, ʒ ij.; purified lard, ʒ vi.; or concentrated tincture of capsicum, viz., capsicum pods, ʒ iv., rectified spirit, ʒ xij., macerate for a week and strain; two or four grains of delphinia or veratria may be added to this tincture, if thought needful. Or, as a household remedy, boil four ounces of capsicum washed to free it from chloride of sodium, in a pint of olive oil for six hours, and strain; rub with this as above. (Dr. Turnbull, p. 112.)

Reaction takes place from the stage of collapse, by absorption of water into the blood vessels, and by vomiting. The best plan of treatment is to let the patient drink cold water *ad libitum*. (Mr. French, p. 107.)

If the disease commences like common bilious cholera, give acetate of lead and opium, or chalk mixture with opium and aromatic confection; give effervescing draughts with laudanum to allay the vomiting, keep the patient warm in bed, and the next day give a large emollient injection. But if the more severe and characteristic symptoms come on, or if there is great debility from the first, and cramps or coldness of the skin are observed, bleed largely, if the pulse will allow, and give a scrupel of calomel with a grain of opium; after twenty minutes, give ten grains of calomel and half a grain of opium. When the calomel has acted well on the bowels, and large quantities of bile have been passed by stool, opium alone may be given, but not till then. If the disease is very far advanced when the patient is first seen, or if the attack has consisted in sudden collapse, almost without cramps, vomiting, or purging, apply stimulants, liniments, sinapisms, &c., to the legs and stomach, rub hot terpentine along the spine, or apply a blister to the nape; give turpentine injections or mustard emetics, and bleed; giving also internal stimulants, punch, brandy, or carbonate of ammonia. (Dr. Massy, p. 108.)

FEVER.—*Typhus*.—In the low forms of fever, camphor, in doses of 3 or 5 grains, is a very valuable remedy, even where there is such cerebral excitement as to indicate local depletion, or when subacute inflammation in the alimentary cavities prevents the use of ordinary stimulants. Give Sir J. Murray's solution of camphor in his fluid magnesia, an ounce of which contains

three grains of camphor. Use the same preparation as a lotion to prevent sloughing sores. (Sir Jas. Murray, p. 18.)

Camphor is an excellent remedy, and in low fever it may be given up to fifteen grains three or four times a day, combined with calomel and opium. (Dr. Copland, p. 19.)

When the system is worn out for want of sleep, and when there is much delirium, let chloroform be inhaled to induce sleep. Commence with 10 minims on a little sponge or pocket-handkerchief, and, if necessary, repeat it in a few hours, increasing the dose according to circumstances. (Dr. A. Fairbrother, p. 20.)

GOUT.—After cleansing out the bowels with proper aperients, give ten grains of phosphate of ammonia every eight hours, either in simple water, or in infusion of serpentaria with spirit of nitre. Order perfect rest and simple diet; cover the parts with fleecy hosiery and oiled silk; and give an aperient of pil hydrarg. with pil. rhei. comp. every other night. (Dr. Edwards, p. 26.)

RHEUMATISM.—Give vinegar in rheumatic and arthritic diseases, under the following circumstances; when the colchico-alkaline treatment fails; when the patient is of middle age and of injured constitution; when there is epigastric pain after eating, foul breath, and fetid eructations; and when the urine is not cloudy; in short, where the disease is owing to a deficiency in the assimilating process and imperfect secretions, give a drachm of acetic acid, with tincture of Jalap twenty minims, tincture of orange-peel a drachm, and a little camphor mixture, twice or thrice a day. (Mr. J. C. Atkinson, p. 29.)

Acute.—When there is no cardiac affection, give sulphate of quinine, from fifteen to thirty grains daily, and continue its use for a few days after relief is obtained. (M. Trousseau, p. 28.)

After subduing the more inflammatory symptoms of antiphlogistic treatment, give ten grains of phosphate of ammonia every eight hours. This medicine acts beneficially by decomposing the uric acid or urate of soda, which is formed in excess in gout and rheumatism. Instead of urate of soda, a very insoluble, we have thus formed phosphate of soda, a most soluble, salt. (Dr. Edwards, p. 26.)

Chronic.—There is an excess of albuminous compounds in the system, and a diminished quantity of oily compounds: the direct addition of the latter, therefore, seems the most rational method of treatment. Cod-liver oil is to be preferred to other oils, because experience has proved its greater efficacy. (Dr. Hughes Bennett, p. 285.)

To relieve the lameness, employ electro-galvanism. (Mr. Christophers, p. 288.)

SCROFULA.—Give proteine (prepared by Garden, Oxford-street, London), five or ten grains once or twice a day. (Mr. Tuson, p. 298.)

SCURVY.—Arises probably from a deficiency of potash in the blood, and is to be prevented or cured by the exhibition of some of the salts of potash, or those articles of diet in which these substances are found, as almost all fruit and fresh vegetables, milk and fresh meat. If medicine is to be prescribed, give 10 or 15 grains of the phosphate, chloride, or tartrate of potash twice or three times a day either in water or with the food. (Dr. A. B. Garrod, p. 23.)

Braithwaite's Retrospect, 1848.

Part Fourth.

AMERICAN MEDICAL INTELLIGENCE.

I.—*Treatment of Cholera, by Blood-letting and Emetics of salt and water.* By BENJAMIN H. MAY, M. D. of Petersburg, Va.

The following communication was received a few days since, by the Honorable A. D. Crossman, President of the Board of Health, from Dr. May of Virginia, suggesting a mode of treating Cholera, which although not new, to the older members of the profession, yet we think sufficiently interesting to give it to the public.—Some of the views advanced by Dr. May coincide completely with our own, and the source whence they emanate is so respectable, that we make no apology for giving them a place in the Medical Journal—*Ed.*

To the President of the Board of Health, New Orleans.

SIR—Perceiving your City to be suffering under Asiatic Cholera, I have thought any suggestion in the treatment of that disease, which has proved successful, might not be unacceptable. My opportunities of observing the symptoms of this epidemic in the years 1832–33 were ample. I visited the epidemic in a neighbouring city six weeks before it made its appearance in Petersburg, where I had a full practice, during its continuance; the stimulating treatment which was the practice of all the Physicians at first, proving entirely unsuccessful, and upon mature investigation, I came to the conclusion that the disease was one of congestion and not of collapse; the similarity which it bore to a malignant scarlatina, which had preceded it, further confirmed me in this opinion and the cold stage of *that disease*, is admitted by all Practitioners to be one of Congestion. Proceeding upon this opinion, the first case of Cholera, to which I was called, was hastening rapidly into fatal collapse. Tongue, skin and breath cold, great prostration of strength, loss of voice, and perturbation about the *heart*. I had a vein immediately opened, but no blood would run; tumbler after tumbler of warm salt and water was given, until it acted as an emetic, at the same time the arm rubbed, and after getting out about a gill and a half of black coagulated tarred blood, the red blood made its appearance; the pulse which had been at first weak, rose, and became of such strength, that it was necessary to take a pint and a half to reduce it; the breath, tongue and skin became immediately warm—the strength of the man revived and he returned me many thanks for having taken the load off his chest, which yesterday equalled three 56 lb. weight; moderate doses of camphor, opium and calomel were ordered every three hours, nothing else but ice to be taken in the stomach, to remain perfectly

quiet, to be covered with blankets, with warm bricks to be kept under them, to keep up perspiration; in four hours I called again—slight symptoms of collapse were making their appearance; I opened the same vein, when about an ounce of this dark coagulated blood again preceded the red blood, a pint of which was taken away, which produced the same effects of the first bleeding; the medicine, ice and quiet were continued during the night; in the morning the kidneys had recommenced secreting, the bilious passages had succeeded the rice water ones, and the patient pronounced out of danger. This practice was prescribed in every case of collapse to which I was called, the results were, that when a patient was seen in time to get red blood from the arm, relief almost invariably followed, but in many cases the collapse was too far advanced to enable you to get blood, either from the arm or the jugular vein. The practice was so decidedly beneficial and so apparent to the observer, that orders were issued, that in every case of collapse, if medical aid could not be immediately obtained, to have a vein opened and vomit the patient with warm salt and water, until the physician had arrived. There is at this time, living in this place, a most respectable and intelligent manufacturer who had many hands in his employ, who did not bleed less than 50 cases with relief, before I could arrive at his factory. It is necessary to state that the warm salt and water emetic was invariably used, whether the case required blood-letting or not; and notwithstanding, the patient might be vomiting for an hour or two, experience showed, that the contents of the stomach were not evacuated until the salt-water was used, and it generally prevented any further vomiting; perfect stillness, not even moving the extremities, or getting up on any occasion, was strictly enjoined, for in no disease is *motion* so injurious as in Cholera; this shows the necessity of treating the patient where he lies, no matter how disadvantageous that situation might be. The effort to take one to the hospital would put him in a situation that blood cannot be abstracted, because, by that time, the veins will be too much congested to bleed; the only other treatment on which I relied was the use of 10 grains calomel and 3 grains camphor and one grain of opium, to be repeated every three or four hours until the diarrhœa was checked, when the doses of calomel and opium were reduced one half, the camphor continued; all were stopped when the kidneys and liver commenced secreting, when nourishment for the first time was allowed, a blister to the epigastrium, when the sick stomach was troublesome. I will cite a single case as an illustration of my treatment.

Sept. 15, 1832.—A. G., a girl of robust health, aged 22 years, dined upon peas at 2 o'clock the day previous, at night went to a dancing party, eat sweet things and drank lemonade, returned to bed at 1 o'clock, rested well and got up at 9 o'clock, and took tea and toast; no previous diarrhœa or sick stomach, was crossing the street at 10 o'clock to pay a visit, when she was seized with violent cramps, giddiness and loss of strength; she was just able to throw herself in the door, when by her face her friends saw that she was attacked with Cholera. I saw her in 15 or 20 minutes, she was having evacuations with rice water, from the stomach and bowels; skin, breath and tongue cool, voice weak and the cramps were more severe, than I ever witnessed,

with a complete prostration of strength. A vein was immediately opened and a salt-water emetic given, three or four ounces of dark coagulated blood were got out of the arm with some little difficulty, followed by a bold stream of red blood; the pulse rose so, that it was necessary to take a quart of blood to reduce it to the proper standard, the action of the emetic brought up the peas unaltered. 10 grains of calomel, 3 of camphor and one of opium were given in powder and another ordered in three hours; blankets with warm bricks under them, and ice to allay thirst, were directed. In three hours and a half I saw her again, the cold stage was rapidly advancing, the vein was again opened, an ounce or two of coagulated blood was again succeeded by a full stream of red blood, the same effects followed as in the first bleeding, the diarrhœa having ceased, 5 grains calomel, $\frac{1}{2}$ grain of opium and 3 of camphor were ordered every three hours. I visited the patient again in six hours, I found the cold stage again making its appearance; a pint and a half of blood was again taken, the medicine, ice, warmth and rest, were directed to be continued during the night; in the morning all symptoms were favorable, the kidneys and liver commenced healthy secretion, the medicine was stopped, light nourishment was ordered—not even a purgative was found necessary to work off the calomel and she recovered without a sore mouth. She is now alive and well. This patient required a larger abstraction of blood than any I met with; the cramps and suffering were also greater than usual; I had all the blood saved during the day, and invited my brother practitioners to call and witness the effect of the treatment. It was believed by all, that saw her, that but for the loss of the large quantity of blood, she must have died in from 10 to 12 hours. Not being acquainted with the Faculty of New Orleans, I must refer you to Messrs. Brander and McKenny, Jas. and W. Gasket, as to my standing as a practitioner.

Yours respectfully,

BENJAMIN H. MAY, M. D.

Petersburg, Va., Dec. 30th, 1848.

New Iberia, 22d December, 1848.

SITTING OF THE ATTAKAPAS MEDICAL SOCIETY.

The Society met pursuant to adjournment and was called to order by Dr. C. Dungan, President.

The following members answered to the roll: Drs. J. B. Dungan, A. Duperier, J. B. Hacker, Jerome Mudd, J. T. Alexander, R. C. Hilliard, A. J. Saunders, C. R. Fassitt, W. G. Mills, J. W. Lyman.

The following gentlemen were elected members of the society:—Drs. D. E. Meade and E. F. Beauchamp.

From the committee for the incorporation of the Attakapas Medical Society, Dr. J. Mudd read a report which was filed among the archives of the society; said committee was discharged.

Report from the treasurer was read and accepted.

The following resolutions were adopted:

Resolved, That a committee of three be appointed to address the

Legislature on the subject of amending the law, regulating the practice of Physic in the State.

The president appointed Drs. W. G. Mills, G. W. Scranton and R. C. Hilliard.

Resolved, That a Committee of three be appointed, to correspond with the Physico-Medical Society of New Orleans, on the propriety of holding a State Medical Convention in the City of New Orleans, on the first Tuesday in March next.

The President appointed Drs. A. Duperier, J. B. Hacker and C. R. Fassitt.

Resolved, That the same Committee be authorized to have printed and distributed among the physicians of the State, notifying them of this convention.

Resolved further, That the President appoint a Committee of five to attend the State Medical Convention.

The President appointed Drs. D. E. Meade, A. Duperier, C. R. Fassitt, J. M. Lyman and J. Mudd.

Resolved, That on account of necessary absence of Dr. J. B. Hacker, his essay be accepted, to be read at the next meeting.

Dr. J. M. Lyman exhibited to the Society a specimen of worm, which was vomited up by a negro girl belonging to a planter near Franklin, in the summer of 1847.—The worm was thrown up in detached pieces, of various lengths—and when put together, measured 280 inches. (twenty-three feet and four inches;) it is of the size of a large swan's quill—the head measures six lines from eye to eye, and ten or eleven lines in length; the eyes are of the size of a large marrowfat pea.

It was deposited in the Museum of the Society.

The following officers were elected for the ensuing year:

Drs. J. M. Lyman, President; W. G. Mills, Vice-President; J. B. Hacker, Rec. Secretary; D. E. Meade, Cor. Secretary; A. Duperier, Treasurer.

Censor for the Parish	Lafayette	- -	Dr. G. W. Scranton
"	"	"	" E. F. Beauchamp
"	"	" Vermillion	- - " J. T. Alexander
"	"	" St. Martin	- - " R. C. Hilliard
"	"	"	- - " J. A. Smith
"	"	" St. Mary	- - " C. R. Fassitt
"	"	"	- - " J. T. Smith.

Drs. E. Sheil and J. A. Smith were appointed to prepare essays for the next meeting of the Society.

(A true copy of the proceedings) J. B. HACKER, M. D.,
New Iberia, 22d December, 1848. *Secretary.*

STATE MEDICAL CONVENTION.

Action of the Physico-Medical Society.

At a regular meeting of the "Physico-Medical Society of New Orleans," held on the 6th January, 1849, a communication from A. Duperier, M. D., on behalf of the "Attakapas Medical Society," was read, and the following resolutions unanimously adopted:

1st. *Resolved*, That the "Physico-Medical Society" have heard with pleasure, that a "State Medical Convention" is about to assemble on the second Tuesday of the ensuing month of March.

2nd. *Resolved*, That we will cheerfully cooperate with our brethren throughout the State in their laudable object in meeting.

3d. *Resolved*, That the Physico-Medical Society" do hereby tender to the "State Medical Convention" the use of their hall.

4th. *Resolved*, That the Corresponding Secretary of this Society be instructed to correspond with the "Attakapas Medical Society" on the subject of a "State Medical Society," and to transmit to that body a copy of the foregoing resolutions.

5th *Resolved*, That the foregoing resolutions be published in the New Orleans Medical and Surgical Journal.

THOS. HUNT, M. D., *President*,

B. H. MOSS, *Secretary*.

NEW ORLEANS, JANUARY 1, 1849.

WITHDRAWAL OF DR. J. HARRISON.

We regret to announce to the readers of the Journal, that our able and talented colleague, Dr. Harrison, has withdrawn from the Journal. His professional duties, and other engagements, have so occupied his time, that he could not give any attention to editorial matters. He has however written enough to place his name among the first medical Philosophers of the day, and we hope he may yet live to extend his fame, and enrich, by his pen, the science of Medicine.

HEALTH OF THE CITY.

January, 1849.

Up to the time of the appearance of cholera in our city, the sanitary condition of New Orleans was most excellent, notwithstanding the rapid and great accession to our population, since the commencement of fall. No peculiar type of disease prevailed; it was too late for autumnal fevers, and yet rather early for winter diseases, such as typhus, typhoid and thoracic affections.—It is worthy of remark, that up to the present time, we have been less afflicted with the exanthemata, such as measles, scarlatina and small-pox, than usual during the winter.

It does not become us, however, to speak too confidently on this point, as we are but yet in the middle of the winter-solstice.

Until reports of cases of cholera were disseminated by some of the prints of the day, our city gave promise of a brilliant season to those devoted to pleasure and amusement, and to the man of business, a rich reward for his toils and his enterprize.

About the time the cholera began to decline as an epidemic, enteric affections, remarkably obstinate in their character, began to appear. During the months of December and January, gastro-enteric diseases became quite common, and, in some cases, unmanageable, showing a manifest tendency for disease to locate itself upon the mucous surfaces.

Since the 20th of Dec., we have seen several cases of small-pox, and heard of a number of others,—all very violent in their character,

notwithstanding previous vaccination had been practised in several instances.

Specific diseases of whatever nature, seem to have been marked with more than their ordinary obstinacy, and can this be explained without appealing to an epidemic constitution of the atmosphere, according to Sydenham? or is it attributable merely to an epidemic influence, which pervades the atmosphere, operating injuriously upon all classes of the community?

We are unable, with our present knowledge, to answer these questions.

About the 22d of December, 1848, the Board of Health declared the cholera to be epidemic.

On the 6th of January, the same body declared the cholera no longer epidemic. It then follows, that, from the 22d of December 1848, to the 6th of January, 1849, the cholera raged in this city as an epidemic. Thus, then, it prevailed as an epidemic in our midst for fourteen days, beginning about the 12th of December, and continuing up to the present date, (23d of January), the deaths from the disease, daily, at this time, being about an average of twelve.

The greatest number of deaths for the 24 hours, reached 114, of which 92 were of cholera.

This was about the 28th of Dec., and soon after this date, it began slowly to decline, in accordance with the laws of epidemics, and we trust, that ere long, not a mark of this singular pestilence will be seen in our city or country.

It will be seen, that we are much behind our time, in the publication of the work. The epidemic must be our apology, since it frightened even the printer's *devil* from his post, and but few could be found to carry on the Journal.

The delay then was unavoidable, and we beg the kind indulgence of our friends for this seeming negligence.

We could not foresee and therefore provide for such an accident; for the future, we shall strive to be punctual. The fault is not with the editor.

CHOLERA IN NEW-ORLEANS.

About the middle of December, the public mind was greatly excited and much alarmed, in consequence of the arrival of the ship *Swanton* at our port with a large number of immigrants on board, some of whom, it was reported, had died of Asiatic Cholera, during the passage. Our esteemed and ever vigilant Mayor, Hon. A. D. Crossman, Esq., caused the vessel to be boarded, and carefully examined, and the result proved that between fifteen and sixteen, out of 280 persons, had died during the passage, some with ship-dysentery, or diarrhœa, and some other affections, such as are common among a large crowd of steerage passengers, on a long voyage, confined to a small space, and breathing an impure—a contaminatad atmosphere. One of the passengers, a female, who was labouring under some affection of the bowels, on the arrival of the vessel, was taken to the Charity Hospital, and expired in a few hours, with all the symptoms of *Cholera Asphyxia*. The day after the

vessel anchored, a German, aged about 25, was attacked with vomiting, purging, cramps &c., and died at the Hospital, whither he had been carried, in less than 24 hours, from the onset of the disease. Thus ended the cases from the Swanton. The German, whose case has already been mentioned, left the ship on the 12th December,—a cold, damp and rainy day, and exposed himself, as he acknowledged, to the rain, until he became quite wet,—ate fruit abundantly and retired for the night. During the night, he was attacked with vomiting and purging; also with cramps in stomach and muscles of the lower extremities. About 9 o'clock, the next morning, he was admitted in the Hospital, in a *collapsed* condition and died during the day. From this time, cases began to occur daily in different parts of the city, remote from the vessel supposed to have imported the fell disease in our midst. No connection, even by the warmest advocates for importation and infection, between the two cases from the Swanton, from Havre, and the subsequent attacks, could be traced; yet it was again and again asserted, that this packet had been the starting point—the *focus* from which the disease had been propagated. The arrival of the ship from Havre, where not a single case of *Cholera* had made its appearance, and the developement of the disease in our midst, at the same time must be regarded merely as a coincidence, not as a consequence—not as cause and effect; otherwise, the disease must have appeared first in the vicinity of the vessel, and not, as is well known to be the case, in a part of the city, more than a mile from the anchorage of the Havre packet. Let these facts be remembered when we attempt to account for the origin of the disease.

Considerable difference of opinion prevailed, not only among the professional but also non-professional public, in regard to the real nature of this singular disease. Some will have it that it is the real *bona-fide Asiatic Cholera*,—others, an aggravated form of *Cholera-Morbus*,—others again, designate it *Cholera-Maligna*, &c., &c. That the reader may form his own opinion of the disease, we shall attempt to point out some of the most distinctive features and symptoms of the disease and leave the rest to the profession to determine.

Imprimis;—this disease proves fatal, in some instances, in less than six hours, from the first attack, although it is usually preceded by some disturbance of the stomach and bowels, which manifests itself in the form of a diarrhœa, the discharge becoming gradually more thin and serous, with or without griping, and if not checked at the end of twelve or twenty-four hours, a state of *collapse* supervenes and death takes place in from 4 to 24 hours, in spite of the best directed efforts of the physician. The forming stage of the disease is usually, as has already been observed, characterized by diarrhœa, and for our treatment to prove effectual, it must be interposed before the patient reaches the second or *adynamic* stage, from which the most powerful stimulants and revulsives can not extricate him.

Causes of the disease. It has been attempted to prove, that the disease, called cholera, which prevails in this city is of *asiatic* origin, and was imported into this city; but if we can point out the causes that gave rise to the disease, its foreign origin must be abandoned. For why should we resort to importation, in the very teeth of the most positive facts to the contrary, to account for the developement of a disease,

when domestic, local causes, adequate to the production of almost any form of endemic malady, can be found in our midst. For some days prior to the appearance of the first case of cholera, the rain fell almost daily—the atmosphere was humid, murky, close and oppressive,—the streets, gutters &c. were surcharged with filth and filth of every kind—abundant sources—numerous *foci* for the generation and spread of a species of malaria or miasmata which, operating upon persons, already debilitated and relaxed, by the warm, damp weather, soon manifested its poisonous influence upon the system by acting upon an irritable gastro-enteric mucous surface, producing diarrhœa, vomiting, cramps, collapse and death. All this time the wind was from the South; at intervals during the day, an almost tropical sun beamed upon us,—moist and murky vapor, born of the stagnant pools and filthy sewers that surround us, and charged with pestilential matters, enveloped the city and hung like a funeral pall about us. The thermometer, although in the middle of December, rose to 75 and even as high as 84, in the shade. It may be well to remark, that this state of things was possibly aggravated, by the exposure of a large quantity of mud and dirt, to the action of the sun &c., caused by excavating the foundation for our new custom house, near the levee. Suffice it to say, that the state of the streets, yards, alleys, gutters, &c., notoriously in a filthy condition, and now made worse by the fall of a large quantity of rain—all added strength and virulence to the exciting cause of the disease.

Hence, the origin of the disease, as we humbly conceive; hence too the fatality among the poor and exposed part of our population.

In our preceding number we ventured to invite the attention of our city authorities to the condition of the streets &c.,—predicted the speedy appearance of the cholera among us, and urged the immediate adoption of judicious sanitary measures, to mitigate, if they could not stay, the progress of the scourge. It is bootless to say that nothing was done,—the evil seemed too remote,—too contingent, to attract attention or excite alarm. It has been shrewdly, but not correctly remarked by a physician, that the cholera begins where other diseases end,—in death. The premonitory symptoms over, death begins his work, and in a few hours all is over. The stage of collapse begins in some cases almost from the moment of attack; it is characterized by great prostration, a feeble, wavering and filiform pulse,—panting respiration in some instance, cold and shrivelled surface,—blueish tinge about the hands and feet—the lips and face generally present the same color. The tongue is moist,—of a delicate pink hue along its borders—either natural on the dorsum, or of yellowish hue. The eye is sometimes injected as in yellow fever, and the tunica albuginea of an icterode tinge. Sometimes they present the appearance of one in a state of profound intoxication. The intellect is generally, though not invariably, clear. Often they complain of a sense of weight, of tightness about the præcordial region, and through the lower part of the chest. If reaction should be brought about, by means of stimulants, revulsives &c., the tongue becomes dry and red or brown—singultus sometimes supervenes,—the skin becomes red and warm,—the pulse rises, both in fullness and strength,—the carotides throb—the head is painful and thirst unquenchable—and too often the physician deems his patient out of danger. Far from it—another and a fatal col-

lapse suddenly seizes the patient, and he is no more. In some instances, they perish during the stage of reaction—with a burning though perspirable skin—and other evidences of great arterial excitement. We consider this one of the anomalies of the present prevailing disease. If they survive the first collapse, they run into a form of congestive typhus, which speedily ends in cerebral congestion, coma and death.

The disease confined itself chiefly to the intemperate, the reckless and such as were exposed, badly clothed, fed and lodged. We noted a few exceptions to this general rule. The blacks also suffered throughout the progress of the epidemic; this may be explained from the fact that slaves are notoriously improvident, and habitually neglect, unless questioned on the subject, to report the first symptoms of the disease.

From our recollection of the cholera of '32 we are persuaded that the epidemic of '48—9 differed in some respects from that of '32; the discharges were not so frequent and profuse—the cramps less constant and obstinate and a much smaller number were rescued, after entering the stage of collapse, in the last than in the first epidemic.

Old residents, and persons thoroughly acclimated, were less liable to attacks of cholera than strangers, newly arrived in the city.

We have not yet learned that a single case of cholera occurred among those who passed through the epidemic of '32, in this city; there may be exceptions to this proposition, but they have not come to our knowledge. The cholera attacked and carried off a number who happened to be labouring under chronic diarrhœa; this was particularly noted in the Charity Hospital, where a large number of cases of this disease is always on hand.

It spared neither the infant in its mother's arms, nor the octogenarian, leaning upon his staff for support, who happened to be predisposed to the disease, from previous attacks of diarrhœa. It must be repeated however, that very few of our citizens who could command all the comforts of life, were attacked with the disease; yet all were more or less under the epidemic influence.

Few escaped some disturbance of the alimentary canal,—a general tendency to excessive evacuations from the bowels, was prevalent. All complained of uncomfortable sensations in the stomach and abdomen, either with or without purging; perhaps the imagination—that fruitful source for good or evil—had some agency in the matter.

During the prevalence of the cholera, many suffered from a mild form of Influenza; in some instances, it was attended with catarrhal symptoms, and severe bronchial irritation accompanied with febrile disturbance, pains and soreness through the chest and muscular system generally. It rarely proved fatal, without the most reckless exposure and culpable neglect on the part of the patient.

We observed that those who suffered from the Influenza, escaped an attack of cholera; hence we are disposed to regard both diseases as produced by the same causes, but operating upon different parts of the system, according to peculiarities of constitution &c.

We are not prepared to enter into any speculation upon the pathology of cholera; it is acknowledged to be a disease of the fluids of the body. The cause, whatever that may be, of the disease, operates upon the blood, producing some serious change in its constituent elements, by

which the fibrine and some of the salts of this fluid are poured out in large quantities into the stomach and bowels, and discharged in the form of serous and rice-water evacuations. Were the discharges composed simply of such fluids as are brought away by saline cathartics, or such as take place in an ordinary diarrhœa, collapse could not occur so early in the disease; nor will the quantity of the evacuations by any means explain that utter prostration—that extinction of both the vital and dynamic forces which we witness in cholera, a few hours after the disease fixes its inexorable grasp upon its victim.

In a word, the discharges are often too trifling to account for the prostration and death of the patient. We venture the opinion, that the force of the disease expends itself upon the great ganglia—the great centers of the sympathetic nerve—destroying the equilibrium between the circulation, innervation, and consequently, secretion,—hence the phenomena of cholera. We moreover believe that the impression made upon the nervous system of organic life, superinduces a state of congestion, from which it is so difficult to arouse the system.

We regard the serous evacuations as *symptomatic*, and not as the *prima causa* of the disease—not invariably the cause of death—because, many expire with all the phenomena of cholera, without any rice-water discharges. In *paucis verbis*, congestion kills the patient, therefore let us regard it as an acute form of *algid*—of *congestive* fever, and nothing more,—of course our treatment of the disease will be made to correspond with the views already developed as to its essential nature.

We give below an interesting report of the *post-mortem* examination of eighteen cases of cholera carefully drawn up by Vincent Boagni, a very talented resident student of the charity hospital. We witnessed several autopsies of this disease, and the subjoined notes, we believe, contain all the important cadaveric changes, revealed by the scalpel:

Autopsy of 18 Cases of Epidemic Cholera.

In the thorax, the lungs were of a pink color, always collapsed, with the exception of three cases, where existed morbid changes anterior to the death by cholera. The blood found in them was very dark. In six cases, there was a good deal of mucus in the larger bronchi. The chambers of the heart were invariably dilated and filled with blood, the right auricle remarkably so. The vena cava likewise. In three cases examined 3 hours after death there were on the external surface of the heart several reddish spots somewhat resembling ecchymosis.

In the abdomen, the stomach was in five cases intensely inflamed, the mucous coat was soft, and had *le couleur de tie de vin*, and there was a quantity of fluid resembling the black vomit of yellow fever. In twelve cases the traces of inflammation were not so intense, and the fluids appeared of a yellowish-green color. The mucous tissue of the intestines was pulpy and thickened. In four cases there was evidence of violent inflammation, extending the whole length of the smaller intestines, and involving the ilio-cœcal valve. In nine cases this appearance was less evident. In five the mucous tissue was bleached, anæmic, strongly contrasting with the condition of the others. The glands of Brunner were plainly visible—those of Peyer were salient, pulpy, red and softened. The solitary glands were larger than natural and easily distinguishable in

the whole of the intestinal tube. In it there was to be found at times a turbid, at times, a transparent whitish fluid, with shreds floating in it, whiter than the fluid itself. This secretion was tested, and albumen was always found in it.

The Liver was in a few cases much engorged, in the majority however it was not so, and its appearance was healthy withal.

The Gall Bladder was always found distended with bile and its ducts pervious to the air blown from the fundus of the bladder into the intestines.

In the Kidneys and Spleen there was no apparent change.

The Bladder was very much contracted. In three cases there was in it a few drachms of liquid closely resembling that found in the intestines.

The veins within the abdomen were distended with dark blood.

The Brain appeared healthy—to the touch it seemed firm, even more so than usual. Its serous membranes felt very dry, pasty—it was even so in the thorax; it was so in the abdomen. The veins were in some instances much distended, sometimes not. In two cases there was some effusion in the lateral ventricles.

The spinal cord as examined in sixteen of the cases offered no remarkable change. However, in eight, air blown in the sub-arachnoidean space showed several distinct adhesions between the cord and its serous envelope. There was less sub-arachnoidean fluid than usual, in some cases it was almost wanting. In two cases there seemed to be a partial softening of the medulla, on a point corresponding with the 9th Dorsal Vertebra.

CHARITY HOSPITAL.

For the month of October, 1848.

Admissions :	Males,	-	-	-	-	857	} 1015
"	Females,	-	-	-	-	158	
Discharges :	Males,	-	-	-	-	759	} 908
"	Females,	-	-	-	-	149	
Deaths :	Males,	-	-	-	-	138	} 150
"	Females,	-	-	-	-	12	

For the month of November.

Admissions :	Males,	-	-	-	-	818	} 948
"	Females,	-	-	-	-	138	
Discharges :	Males,	-	-	-	-	644	} 743
"	Females,	-	-	-	-	99	
Deaths :	Males,	-	-	-	-	95	} 109
"	Females,	-	-	-	-	14	

For the month of December.

Admissions :	Males,	-	-	-	-	1224	} 1448
"	Females,	-	-	-	-	224	
Discharges :	Males,	-	-	-	-	729	} 856
"	Females,	-	-	-	-	127	
Deaths :	Males,	-	-	-	-	483	} 500
"	Females,	-	-	-	-	17	

ANNUAL REPORT FOR 1848.

Admitted :	Males,	-	-	-	-	9733	} 11945
"	Females,	-	-	-	-	2214	
Discharged :	Males,	-	-	-	-	8129	} 10010
"	Females,	-	-	-	-	1881	
Died :	Males,	-	-	-	-	1375	} 1575
"	Females,	-	-	-	-	200	

DR. BRICKELL'S HOSPITAL REPORTS.

Mr. Editor,

SIR—I proffer you a few hasty remarks on some interesting cases of disease which have come under my observation in my wards at the Charity Hospital during the past month.—

The first of these is a case of “Traumatic Tetanus”; and I know of no better mode of reporting so highly interesting an instance of this dread malady than that of submitting my “notes” taken at the bedside of the patient. The result in this instance has been most happy, and, even were there no other point of interest connected with the case, we should regard *recovery* as having fully entitled it to a place in the “Hospital Reports” of your journal. But a double degree of interest has been excited in me by the mode or *modes* of treatment adopted; in part, on account of the novelty (in New Orleans) of one unsuccessful plan; but more especially, on account of what I conceive to be the complete *triumph* of a remedial agent which, while it commands a proud array of champions well prepared for its defence, has, at the same time, more enemies, perhaps, than any other article of the “Materia Medica.” I refer to that much used, but still more *abused* medicine, Quinine; and while I claim for it the faith of the profession in this instance, I by no means lay claim to any *originality* in its administration in cases similar to the one under consideration. One thing, however, I *do* lay claim to—viz: That I have so administered the medicine as not only to effect the desired end, but, by reporting the case, to afford skeptics a fair opportunity for observing and knowing that in certain conditions of the system many of the evil results attending the administration of “large doses” of Quinine are altogether *imaginary*; and, that the agent is capable of producing other than the “contra-stimulant” effects imputed to it by those, who, whilst they grant it this, together with a “tonic virtue,” deny its capability of checking the continued fevers, &c, through the means of another, and what we consider to be a far *more* important inherent power; and this, merely because they have not *seen* such results. But I did not take up my pen for the purpose of discussing Quinine. I propose merely to cast before the world facts; believing that the time is not far distant, when a mind more comprehensive, better qualified in every way, will, with a sufficient number of such facts, lay the foundation of one *great* and *immutable* fact in Medicine—viz:—that our *Materia Medica* can boast of agents which, when properly administered, are far more potent and more to be relied on by the physician than the imaginary “*Vis Mediatricis Naturæ*.”

“*Case of Traumatic Tetanus.*”

March 16th 1848:—Albert Fisher, German, aged 20 years, quite ro-

bust, proprietor of a "coffee house" in the 3d Municipality, entered ward 11, Charity Hospital. Says he ran a nail into the external portion of his right foot, near the junction of the "metatarsal" and first phalangeal bones of the "little toe;" about five weeks ago; that the wound caused him considerable pain at the time, but soon healed without any difficulty, that, at the expiration of three weeks from the time of the reception of the injury he was seized with violent tetanic spasms, together with slight pain in the original seat of injury, that, on the following day a physician laid open the old cicatrix, and extracted a portion of the nail which had been imbedded there during all this time, that the spasms were now somewhat mitigated, but he was confined to his bed with constant rigidity of the back, jaws and hip joints, together with frequent lancinating pains in the lumbar region.

In this condition I find him this morning. He complains bitterly of insomnia, begs to have something which will induce sleep, every three or four hours he has an aggravation of the symptoms amounting to almost complete "opisthotonos," appetite good, bowels in good condition, perhaps a little torpid, skin perfectly natural, countenance wearing the distressed air so peculiar to these cases, complains much of the lancinating pain in his back, says they recur every three minutes.

"*Treat.*"—Ol. Ricin. f. $\frac{3}{4}$ i.—Tinct. Opii. gtt. xxx. every hour, after the free operation of the oil, until sleep is induced; low diet, Emplast. Visicat. to back.

17th. Says he feels better, but is drowsy and dull, oil operated freely, commenced taking laudanum at 12 M. yesterday, and repeated, as ordered, every hour until 12 at night, when he fell asleep and slept soundly until 6 this morning, pain in back not so intense, rigidity same.

"*Treat.*"—: Moderate dose of Magnesia and Rheubarb, Tinct. Opii. gtt. lx, to be taken at 8 P. M. half diet.

18th: Not so well, slept very little last night, complains of the stupefying effects of the laudanum, pain in the back returned.

"*Treat.*"—I now resolved to try the *internal* administration of Chloroform, and ordered, accordingly, gtt. xxx to be given at once in one ounce of mucilage, and to be repeated every two hours, with gtt. x additional at each dose, until sleep is induced; blister, also, to be reapplied, it having taken but slight effect before.

19th. Took four doses of chloroform as ordered, and fell asleep about an hour after the first dose, slept soundly until 1 o'clock this morning, says he thinks he would have slept before, but for the irritation kept up by the blister; looks bright, feels much refreshed, back much relaxed, hip joints nearly entirely so, no unpleasant result from taking the chloroform.

"*Treat.*"—Chloroform repeated as before, beginning with gtt. lx. half diet.

20th. Slept several hours yesterday, and soundly all night, feels much better, rigidity almost the same.

"*Treat.*"—Chloroform gtt. c. at night, Rheubarb and Magnesia.

21st.—Had several spasms yesterday before taking the chloroform, complains much of the pain consequent, countenance more distressed, had several spasms this morning, rigidity the same.

"*Treat.*"—Chloroform gtts. c, to be taken at 12 M., and repeated at 4 and 8 P. M., if sleep is not induced.

22nd.—Took two doses of chloroform as directed, and slept from 4½ P. M., to 12 or 1 this morning; spasms quite frequent this morning, and amounting to almost decided "opisthotonos," was aroused from his sleep by these spasms several times last night, much pain, is evidently growing worse.

"*Treat.*"—Quin. Sulph. grs. xxx
Tinct. Opii. gtts. lx
Mucil. Acac. f. ʒ i. M.
S. Take at 10 A. M.

"5 P. M.—Much better, but four or five spasms during the day, and these much mitigated, pain much relieved, countenance more cheerful, no "tinitus aurium," or other disagreeable effect from the use of Quinine.

"*Treat.*"—Repeated the Quinine, &c.

"23d."—Very much improved, had but one or two slight spasms since I last saw him, scarcely any pain, slept well last night, is delighted with the remedy.

"*Treat.*"—Same dose as before to be taken at 10 A. M., and 3 and 9 P. M.

"5 P. M.—" Has taken two doses as ordered, feels better every hour, spasms scarcely perceptible, no pain, has slept well during the day, no unpleasant symptom from Quinine, feels "a little drunk."

"24th.—" Still improving, rigidity of back very slight, no pain, slept well all night, looks cheerful. "*Treat.*"—Same dose Quinine at 12 M. and 8 P. M.

"25th —" Marked improvement, no more spasms, no pain, rigidity very slight, slept well, looks bright, says he is nearly well.

"*Treat.*"—Same as before.

"26th.—" Patient is walking about the ward, improving rapidly.

"*Treat.*"—Same continued.

"27th.—" Suspended the Quinine, and ordered good diet, small dose of oil.

"30th.—" Discharged him, *well*.

"*Observations.*"

This is the first instance, I believe, wherein chloroform has been *internally* administered in the City of New Orleans, and notwithstanding its failure to effect the desired end, it may not be uninteresting to the profession to know the results of its exhibition. It evidently possesses the power of producing anesthesia when thus administered, and my limited observation leads me to believe that the process of inhalation may ere long be superseded by the method under consideration. The patient experienced no disagreeable effect whatever from its use; there were none of the distressing symptoms which I have so often seen consequent on the inhalation of the agent, viz.—wild delirium, convulsive action of the muscles of the extremities, and astonishingly increased activity of the circulation. The effect appeared to be more gradual, the patient passing off into a sweet and refreshing sleep without being subjected to previous excitement.

The dose administered was much larger than in any previous instance

that I know of, and, I think, may have been considerably increased without incurring any risk of unpleasant consequence.

In relation to the administration of Quinine in this case, I cannot refrain from calling the particular attention of the skeptical reader to a few facts extracted from the above "Notes."

1st. The patient was, at the time of the adoption of the Quinine treatment, evidently growing worse. 2nd. That marked improvement followed the first dose. 3d. That, so far from any disagreeable symptoms having followed said dose, the patient actually expressed himself as "happy" whilst under its influence, and begged to be *kept constantly* in this condition. 4th. That, an hour and a half after having taken two thirty grain doses, at an interval of five hours, he said he felt even better than before, no other disagreeable sensation than that of being "a little drunk." 5th. That, on the morning after having taken ninety grains within ten hours he expressed himself as much improved in every way, and *was so*. 6th. That, after this he took sixty grains daily, in two doses, for three days, without experiencing a *single one* of the much dreaded effects of the medicine, and was not only relieved of the tetanic symptoms, but improved most wonderfully in appearance, slept well, had a fine appetite, and digested the strongest food with ease.

"Potass, Hydriod, in Secondary Syphilis, &c.

I will now beg leave to mention two cases of Secondary Syphilis, wherein Potass. Hydriod. has been freely used, and with the very best results. I do not offer notes in full on these cases, but merely present their characteristics, together with the treatment and results.

In both instances the patient was a robust man of middle age. Each had been afflicted with primary syphilis several years ago, were suffering, when they entered the ward, with severe pain in the head and limbs, more intense at night, had nodes. One of them had a slight cutaneous eruption, evidently of syphilitic nature. I put them both on the use of a combination of Potass. Hydriod. and Vin. Colchic., beginning with grs. xxx of the former and gtts. xc. of the latter daily, in three doses, these to be increased daily grs. x and gtts. x. Gave them, also, good diet. When they reached grs. xc, improvement commenced, and by the time they reached 3 ii was *decided*. I did not increase the dose farther, but repeated every day, and at the end of about three weeks an entire cure was the result, that is, so far as we are enabled to judge from an entire absence of all the symptoms which characterize Secondary Syphilis, as well as the *presence* of all the phenomena which indicate *health*. I have administered this medicine in like manner in several other cases, and with the very best results. There is now a case in one of my wards which goes farther to illustrate the virtue of this remedy than any other I have yet seen. The patient had been a martyr to chronic rheumatism for four years past, had resorted to every agent recommended for the relief of this disease (amongst them the very remedy in question), but without even temporary relief. I found him with a haggard countenance, pale and thin, legs permanently flexed at the knees, the latter much "puffed," suffering intense agony night and day. By questioning him I learned that he had used Potass. Hydriod. during a long time, but had never exceeded 15 to 25 grs. daily. Under such cir-

cumstances I resolved to give it a fair trial; and, accordingly, ordered as in the cases reported above. Before he had reached grs. c, he was entirely free of pain, could extend the legs, slept well, began to fatten: and now, although there still exists some "puffiness" of the knee joints, he looks and feels as well as any one; walks about the ward without his crutches; and is "as happy as a lord." He has been taking grs. c for a week past, and, being anxious to effect an absolute cure, I shall continue the remedy.

Before closing I would beg leave to mention that a genuine case of "Yellow Fever" made its appearance in Ward 13 on the——ult. Patient was in almost a hopeless condition when he entered the ward, had been sick two weeks, but is now convalescent.

D. W. B.

New Orleans, Dec. 1st, 1848.

ABSTRACT OF A METEOROLOGICAL JOURNAL FOR 1848 & 1849.

By D. T. LILLIE, AT THE CITY OF NEW ORLEANS.

Latitude, 29 deg. 57 min.; Longitude, 90 deg. 07 min. west of Greenwich.

WEEKLY. — 1848.	THERMOMETER.			BAROMETER.			COURSE OF WIND.	FORCE OF WIND, Ratio 1 to 10.	Rainy Days.	Quan- tity of Rain. — Inches.
	Max.	Min.	Range.	Max.	Min.	Range.				
Nov. - 4	77.0	46.0	31.0	30.50	29.94	0.56	N.W.	3	2	5.450
" - 11	73.0	48.0	25.0	30.57	30.22	0.35	N.W.	3 $\frac{1}{2}$	1	0.585
" - 18	73.0	42.0	31.0	30.48	30.18	0.30	N.W.	3 $\frac{1}{2}$	1	1.230
" - 25	63.0	43.0	20.0	30.58	29.98	0.60	N.W.	3 $\frac{1}{4}$	1	1.320
Dec. - 2	67.0	35.5	31.5	30.48	29.75	0.73	W.	3 $\frac{1}{2}$	2	1.275
" - 9	76.5	52.0	24.5	30.24	29.96	0.28	S.	3 $\frac{1}{2}$	4	5.050
" - 16	69.5	43.0	26.5	30.16	29.96	0.20	N.E.	3 $\frac{1}{2}$	2	0.825
" - 23	79.0	58.0	21.0	30.29	30.10	0.19	S.	3 $\frac{1}{2}$	1	0.225
" - 30	74.0	42.5	32.5	30.35	29.92	0.43	N.	3	3	3.725
1849.										
Jan. - 6	65.0	41.5	23.5	30.38	30.01	0.37	N.W.	3 $\frac{1}{4}$	0	0.000
" - 13	71.7	38.0	33.7	30.45	29.98	0.47	E.	3 $\frac{3}{4}$	1	0.325
" - 20	78.5	50.5	28.0	30.52	30.18	0.34	S.E.	3	1	0.075

REMARKS.—The Thermometer used for these observations is not attached to the Barometer, but is a self-registering one, and is placed in a fair exposure. Regular hours of observation, 8 A.M., 2 P.M. and 8 P.M.

The Barometer is located at an elevation of 19 feet above the level of the ocean, and is suspended clear of the wall of the building.

The Rain Gauge is graduated to the thousandth part of an inch, and the receiver is elevated 40 feet from the ground.

THEORY OF THE EARTH

The theory of the earth is a branch of geology which deals with the origin and development of the earth and its various parts. It is a science which seeks to explain the processes which have shaped the earth and its features. The theory of the earth is based on the study of the earth's structure and its history. It is a science which is constantly developing and changing as new discoveries are made.

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TO READERS AND CORRESPONDENTS.

Our correspondents will please send in their communications for the *May number* of the Journal as early as practicable. We again request them to *condense* as much as possible ; short and practical-papers are preferable to communications, extended to a tedious length. Our aim is to be brief and to the point, and to this end, we solicit the aid and co-operation of our friends. Ed.

The following Books &c., have been received.

1. *Southern Medical Surgical Journal.* February '49
2. *Charleston Medical Journal and Review.* " "
3. *A case of Typhus or Ship Fever*, with remarks, by W. INGALLS, M. D., (From Author).
4. *Report on the Medical Department of the University of Pa.* for 1848. (From the Faculty).
5. *The Medical Examiner.* For February 1849. (Editors, Drs. SMITH & TUCK., Philad.
6. *Western Journal of Medicine and Surgery.* (Edited by Professor YANDELL). February, 1849.
7. *St. Louis Medical and Surgical Journal*, for February, 1849.
8. *Report of the Standing Sanatory Committee of the Board of Health of the City of New York on the subject of Asiatic Cholera.* (From the Committee).
9. *The Annalist, a record of Practical Medicine.* New York. Febr, 1849.
10. *The Western Lancet and Hospital Reporter.* January and February' 1849.
11. *American Journal of Science and Arts.* January, 1849.
12. *North-Western Medical and Surgical Journal.* January, 1849.
13. *History of a Sarcomatous Tumor, removed from the Womb.* By Professor BEDFORD. (From the author.)
14. *Ohio Medical and Surgical Journal.* January, 1849.
15. *Buffalo Medical Journal and Monthly Review.* January, 1849.
16. *Boston Medical and Surgical Journal.* February, 1849.
17. *Summary of the Transactions of the College of Physicians of Philad.* Jan, 1849.
18. *An Introductory Lecture*, before the Albany Medical College. Oct, 3d, 1848. By Professor T. HUN. (From Author.)
19. *An Introductory Lecture to a course of Medical Lectures at Peoria. Ill.*
20. *Braithwaite's Retrospect.* Last number.
21. *Letter from the General Board of Health of Great Britain respecting the spread of Cholera in England and the inutility of Quarantine Regulations, for preventing its introduction.* (From Mayor CROSSMAN).
22. *Case of Lithotomy, in which 117 Calculi were successfully removed.* By Professor P. F. EVE. (From the Author.) Published in March No South. and Surgical Journal.
23. *American Journal of Medical Sciences.* Last number.
24. *New York Journal of Medicine.* January, 1849.
25. *Ranking's Abstract*, for January 1849.
26. *Manual of Physiology.* By KIRKES & PAGOT, '49.

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THE NEW ORLEANS MEDICAL AND SURGICAL JOURNAL.

MARCH, 1849.

Part First.

ORIGINAL COMMUNICATIONS.

- I.—*History of a mild Yellow Fever which prevailed epidemically in the City of Natchez, during a part of the summer and the fall of 1848, with observations respecting its character and natural mode of cure.*
By C. H. STONE, M. D., of Natchez, Mississippi.

The almost total absence of intermittent, remittent, and other fevers in the City of Natchez, during the summer of 1848, especially for several weeks preceding the second week of August, afforded a rare and valuable opportunity to those conversant with the peculiarities of Yellow Fever, to witness the latter, in its mild form, make its appearance and prevail extensively, uncomplicated with others of a *periodical* and totally opposite character.

In calling the Epidemic *Yellow Fever*, it is proper, at once, to state the rather curious fact, that the physicians differed; *even* the people did not agree respecting it.

The quarantine had been established long enough to satisfy nearly every one of its futility—its false security—and I think it will never find an advocate here again.—But having faith in it, at first, it was ground for belief, with some, that the disease was not Yellow-Fever, which they had been so *easily* taught to believe never had—never *could* have, an origin in Natchez.—Again, with others, the Yellow Fever they had seen and heard of was a dangerous, a fatal disease—they had no idea it could be *mild*—that an elephant had ever been an infant, so little informed were they.

At the commencement of the Epidemic the sick recovered quickly—there was no danger—they got well—the fever was gone before there was time to overdose themselves. It was attended with pains, *severe*

pains in all parts of the body, sometimes even to the hands and fingers and with various *eruptions* on the skin—the same had been observed in Dengue, and some, *therefore*, suggested that this was Dengue—they were encouraged so to consider it, and altho', those who had had Dengue insisted that this was not that disease, yet Dengue became the popular idea and continued with "this fever", to be the name of what was also called "the prevailing disease" till too many *convincing fatal* facts forced an unwilling conviction of its true character on the minds of *all*, I believe I may say rather than nearly all persons.

The people did not, and could not be expected to know that Rush and Drysdale had described these *pains* and *eruptions* in the Yellow Fevers of Philadelphia and Baltimore in 1793, 94 etc., so exactly that their descriptions answer for this, that Dr. Lewis had said that, in the Yellow Fever of Mobile in 1847, the *entire muscular* system was affected with *pains*.

Yet it was still *called* Dengue, through habit by some, through *determination* by others, and perhaps from conviction by a few—and tho' Dengue in life, it was generally admitted to have been Yellow Fever when death took place—in Dengue to-day—they were *unexpectedly* in the death struggle of Yellow-Fever to-morrow, and thus *sporadic* cases of the latter were made by some of the people.

By the Physicians the Epidemic was called by various names—Dengue, nervous fever, rheumatic fever etc.,—by one it was admitted to have nearly all the symptoms of mild Yellow Fever—one, a certain appearance of the countenance being alone wanting—but he, shrewdly, did not *name* it, he was satisfied to say it was *not* Yellow Fever.—Dr. Jones pronounced that Yellow Fever was Epidemic and that Dengue was also prevalent.—Dr. H. Lyle announced the opinion that it was, solely, Yellow Fever and was the first, so to call it publicly, and the writer of this history, entertaining the same conviction, gave the same opinion. These two, not desiring, never received many thanks for this, but they had not a *few hard* names, the usual reward for telling unpleasant truths, however imperative the duty.—Let a description be given of whatever else prevailed. Will it be done?

It will be seen that the Physicians differed more than the people and some of the latter insisted upon their right to disagree on this very account. Yet there were points upon which there was *unanimity* with Doctors and people.

The unusual quantity of bile passed by the Bowels or Kidneys* and indicated by the *bitter* taste, the *yellow skin* etc., in all cases mild and tending to health, and the total absence of bile by vomiting or purging at the commencement in the worst cases, or later in those threatened with death—and they would not have agreed on these, could they have avoided it—the proof was too palpable.

This difference of opinion so common may be viewed as one of the distinguishing marks of Yellow Fever. Neither physicians or people disagree about other fevers of the climate, but always of this.

The only exceptions to one form of disease, and that of Yellow Fever, that occurred to me were a few cases of mumps, some complicated, others not, with the Epidemic, two of pure intermittent from the River

*The Kidneys eliminate some of the elements of the bile only.—Ed.

or swamp—one case of mucous diarrhœa in an infant—one of convulsions in a child from gastro-intestinal irritation, developing the sure signs of the Yellow Fever 18 hours after the attack, and a few other trifling ailments as a common catarrh without fever.—All others were of one and the same disease which by a minute and faithful history I hope to satisfy every one was Yellow Fever and nothing else.

To decide what the disease was, is a consideration too local and unimportant for the pages of a Medical Journal, except that as a similar disease has and will again prevail elsewhere and here, it assumes a consequence to describe it. And this is enhanced by the fact that, if it was Yellow Fever, it has *in its very mildness*, spoken *loudly* to the profession, though gently not dangerously to the sufferers, what functional derangements *mainly* constitute that disease and, more than all, in importance, by what processes it *cures itself*, which, at its beginning, I assured the people it would do, *if* it remained in its then mild form, and which I was enabled to say with confidence, from having seen it thus mild and *self-curing* in many instances, when *allowed*, at Woodville in 1844.

Quarantine was ordered about the 2d September. The disease began under the Hill some weeks before that time and while many citizens were calling for the Quarantine to keep *out* the Yellow Fever, it was already *in*, by means of a local cause, of domestic origin, as always before and hence. It was *rife* about the 10th to the 20th August, tho' many cases had occurred at an earlier period, as I can well prove.

It continued till some two weeks after two frosts and a freeze occurring between the 2d and 5th Nov., the number of cases had become very few for several days previously, but they increased at this time for a few days, in consequence of the sudden cold exciting the disease. As the season advanced, the poison impressed the system more profoundly, which was shown more by the greater debility and the greater danger from imprudencies of any kind, during and succeeding an attack, than by any very apparent *violence* of the symptoms. It was mild from beginning to end, with more, though still few, exceptions as the season proceeded,—and mild, it remained a self-curing disease with still few exceptions.—These exceptions included those recently from the North, Scotland and Ireland and others from conditions, the result of chronic diseases, of habits etc. This is intended to apply to original attacks, and such as was not interfered with, had a fair opportunity by those means, included under the head of *good nursing*, as foot baths, warm beds, recumbent posture, suitable drinks and avoidance of solid food, and even some without these aids—these *remedies*.

The disease attacked all ages, colors, sexes, conditions, even those old Yellow Fever subjects, who perhaps thought themselves safe, though generally sparing this class till a later period, affecting about half or two thirds as I suppose, and uniformly with less severity, which remark is applicable to those of long residence here, or in the South.

Some few genuine *second* attacks no doubt occurred and they have been invariably represented as *alike*. Some persons of a family were said to have Dengue, while the others were admitted to have Yellow Fever, and while whole families having had Dengue were (thus?) exempt from, and did not have Yellow Fever, those having had the latter, were

exempt from the former, though the former cases again were said to *run into* the latter, by what process will be seen. Is Dengue a preventive of Yellow-Fever, or are they the same disease? How certainly not—and how conclusive these facts are that one and the same disease was prevalent and constituted the Epidemic.

The deaths were reported as of various diseases, or *effects* of disease, as specimens—of “debility”, “mental affliction”, “exhaustion resulting from fever”, “inflammation of the stomach and bowels”, “epilepsy” shown by yellow and livid skin and black liquid blood, running from the bowels after death, “disease of the heart” with yellow skin after death etc.—and these are only a few, the most striking peculiarities of form, that Yellow Fever *will* sometimes assume.

The number of cases has been, I believe, within bounds computed at 3000, and the population at 4000 to 4500 ; the number of deaths were forty-two ; and surely it is correct and more modest to call that a *self-curing* disease, which shows this mortality, and considering the variety of modes of treatment required for the variety of diseases, if *names* indicate such, and the fact that very many recovered without medicine and under varieties of domestic remedies.

Two children, perhaps others, were born before they were *due*, while the mothers were under attacks and were found *yellow* from the moment they were seen by daylight, (two or three hours.) I saw one on the sixth day, as yellow as gold and prostrate almost to death, but quickly restored by beef tea and the recumbent posture. (At Woodville in 1844, I was present at a birth ; the child was born with the disease, and went through an attack of great severity, with red tongue, convulsions, &c., to recovery. Mr. P. H. Zoor is cognisant of the fact.) Several persons were so slightly affected as to attend to their business, suffering pains, and their skin and eyes being more or less yellow or thickened ; others were confined for a day or two with slight fevers and pains, &c. A few were affected with *the* pains every four days for weeks, and in one instance, J. Stetheimer, the system labored most severely for the greater part of two months without regular development of fever, but with severe and indubitable marks of the influence of the poison.

The cases ranged from the simplest and the mildest character of the old fashioned “bilious state of the system,” to a mere indisposition, yielding perfectly to a mild remedy,—a mercurial or subsiding *naturally*, though *generally* not perfectly ; they ranged through all grades to a few attacks of great, of mortal severity.

Not the least curious part of the disease was the reason that *each* thought himself attacked, as well as of what the disease was. They often forgot the danger ; they supposed it was a cold in the nose, a sore throat ; an old pain in the back suddenly becomes worse, and he would *call* again in a few days and *sending* in an hour. And some had neuralgia or rheumatism, &c., while few would have been sick *at all* if one had not gone in the country where it was so dusty—the dust in the city not being quite half-knee deep—if another had not had his hair cut two days before, and if others had not done this or that trifle.

As it was believed by too many that yellow fever or other dangerous disease could not have an origin in the place of *their* residence, so, few

individuals had "this fever;" they had something else. Perhaps a *local* patriotism had its counterpart in a self-love not more blind and more dangerous in its consequences. When the fever and pains moderated or subsided, the sick often could not be persuaded that they were still sick—they felt as well as ever, only a little weak; and, to get strong, they must eat (without appetite), and they must walk about, sometimes without ability to raise the head. When could they get up and eat, was the almost universal inquiry; and they would get up and eat unless prevented, and often died from so doing; and indeed every variety of conceit entered the minds of some at dangerous periods, showing a deranged state of the mind, while to a careless observer they might be considered quite sane. By some these may be considered trifles, but not so of anything which tends to mark and distinguish this disease from others, and these very points are almost peculiar to yellow fever.

Catarrhal symptoms, more frequent and marked towards the middle and end of the epidemic, were very common—as a rough voice, pain in the throat, without swelling or much redness, a hawking of great quantities of mucous, a free secretion from the nostrils and eyes, and a cough without expectoration, but sometimes shrill, as in croup—even with a boy twelve years old. I know a child to have been treated for croup, so similar was this symptom, and this had happened to me in 1844, has occurred to many, and will again to many more.

The mucous surfaces in all parts of the body become implicated, as will be noticed of some portions again; that of the uterus showed it by its secretion coming on almost invariably.

A few were able to sleep throughout the attack as in health, but generally they were sleepless, even after the fever and pains had subsided. Dreams were a constant source of great disturbance, and in one case of severity, so affrighted was the patient by their horrid nature that he told me he was afraid to sleep. An abundant extrication of gas in the stomach and bowels was not only almost universal, but distressing.

Sighing was observed in cases of severity, or at the termination in death, of those which had been dengue at their onset, and hiccough was seldom absent at the same stages.

An odor which I cannot describe, but which was *the* odor that had been indelibly impressed on me in 1844, was strongly perceptible in many of the simplest cases towards the latter part of the epidemic, but which I did not notice for a month after its commencement.

In the severer cases vomiting was repeated with violence; in others the stomach was chiefly affected with nausea, or sense of oppression, or fullness after a day or two's continuance of the fever; and if vomiting occurred in these, the mild cases, it was of bile in profusion and afforded relief; but if, in the others, the more severe ones, it was of mucous or the fluid drank and gave no relief; and again, if it occurred in this class towards the conclusion, or if in the milder ones, become worse, it was mucous, or *brown*, then *black vomit*.

Some cases terminated in death without this vomit, but if so, they bled from various parts before or after death. It attended the death of a child ten years old and was *gulped* up. The gums bled and the lips became incrustated with black scabs; incisions in the skin and blistered

surfaces bled as the case was becoming more serious ; the stools were of green mucous or became glutinous, foetid or inodorous, and variously colored or black from dissolved blood, which in numerous instances ran from the bowels, nose and mouth, after death.

As one of its characteristics, the disease had *one* febrile paroxysm, generally with sweating, though sometimes with dry skin, and of from a few hours to two or three, in some *few* extending to four or five days duration, during which no true remission was ever noticed. Where a temporary abatement occurred, it was invariably the effect of means employed, and in this way it was often seen, the fever alternately moderating and increasing, but not subsiding fully until its term, *the period natural to each case*, had expired ; as the case was tending to health, the fever gradually subsided, with a continuance of the perspiration for many days, though with abatement in offensiveness and generally also in quantity, and the pulse gradually came to the *beat of health*, though always more slowly than in similar stages of other fevers ; but if the tendency was to death, the fever subsided less gradually, the skin continued dry, or was in partial liquid sweat, then it became yellow or this color increased ; the pulse more suddenly became slow, fell to fifty-five, sixty, or sixty-five, round, distinct and regular, but *not* with a healthful beat—the patient was in the calm. After the regular febrile stage had passed, a fever sometimes supervened, but this second, or it might be, third fever, *never* occurred except from *imprudences*, as taking medicine or food at a wrong time or kind, muscular or mental effort, or exposure of a perspiring skin to the air ; one or any of these taking place, the effect was a renewal of the fever, and often gastro-duodenal inflammation, which, until subdued, prevented the termination of the fever, causing it to last many days. But this effect, this return of fever, was confined, so far as my observation extended, or I could learn, to the *milder* class of attacks ; in those more severe, the effect of the more gross errors was the more fatal signs, vomiting, red tongue, slow pulse, &c., and *not* fever.

If such errors were committed during the regular febrile period in the milder or in the more grave, the effects were the same, to increase the fever in the former and not to increase it in the latter, but to hasten the precursors of death.

One febrile period, therefore, alone belonged to the disease, and while it could be *moderated* with unerring certainty, I never saw it *lessened in time*, and some mild attacks had as long a period for the fever as others of much greater severity. Some complained of a burning at the stomach just before or at the decline of the fever.

After the period indicated for the fever, nearly all distress vanished ; yet in some milder cases the pains, though less, still remained, first here, then there ; generally the pulse, tongue and skin being natural or nearly so, and the patient, feeling well, could not be induced to remain in bed, waiting patiently for a *true* restoration of health and strength. In the early part of the season they could indulge this wayward humor, but in the latter part, however sure the recovery seemed and really was, a slight imprudence seldom failed to cause a sad reverse and too often death ; this really was the period of true danger—*when the fever subsided*.

The debility induced by the attack, however slight, was not merely muscular or dependant simply upon nervous impairment; the blood had undergone a great alteration, great loss of proper constitution, so excessive was the debility.

It was apparently greater than usual in more severe forms of yellow fever, great as that was in the Woodville epidemic and as it is represented generally to be. And while with *debility* and approaching *faintness* from bleeding in all other fevers, the pulse is *frequent*, in this disease, under these circumstances, it was *slow*, and in other fevers, this condition present, the respiration is quick, frequent and hurried, while in this disease, it was calm—*slow, during the fever—slow up to, and in the article of death.*

These *two grand characteristics* of yellow fever were present in this disease.

The stomach was enfeebled and could digest none but well prepared liquid food in small quantity, at the very time that the blood required still further depuration of *morbid*, and simultaneously a replenishing with *healthful*, materials.

But this was not all; there was a great *tendency* at every period to gastro-duodenal inflammation, which once setting in suspended these processes of *depuration* and *replenishment*, and which must soon end in death if not quickly arrested by ice, mucilages, local bleedings, or by the power of the constitution; the latter for *this* condition so little to be relied upon, and so *entirely failing*, as did all means, other than those named.

When a case was about to terminate in death, which happened to some so unexpectedly, besides the slow pulse and "natural" looking tongue, giving so much satisfaction to the uninitiated, and filling the minds of others with *horror and affright*, there was return of vomiting, the tongue became red and moist, red and dry, or cracked and bleeding, and vomitings were of mucous and of *brown*, sometimes preceding the *black* vomit. Generally with calm minds unconscious of danger, there was in one case said to be stupor, in another wild delirium, falsely and slanderously, though not ignorantly suggested to be delirium tremens, and several died in convulsions.

After death the skin became more yellow than in life; from lemon to orange; livid spots of various dimensions soon appeared on face, neck, &c.; sometimes so black and extensive as almost to obscure the yellow color, and they bled from leech bites, blistered surfaces, from the mouth, the nose and anus profusely in many instances.

Thus ended in death cases even of *mild* character at their beginning, and in life admitted to be of "the epidemic," "this fever," whether called dengue or by other name. Not all showed the whole of the preceding symptoms or effects, but none was devoid of the yellow color, or failed to have sanguineous discharges, or ecchymoses, one or both, *after death*, and but one had a chalk white skin in life and attended with black vomit.

This might suffice, but desiring to exhibit the disease, more practically useful, I shall proceed to a more minute account of the symptoms and conditions, and while showing what were *curative* and what *morbid*, I hope to be allowed the endeavor to trace them to their source without

being charged with presumption; but whether so or not, I shall still make the effort to throw a ray of light upon a disease, about which there is so little unanimity; about which nearly every thing written is discrepant, contradictory, uncertain.

An attack usually began with sudden pains, and generally without previous indisposition, though with some there were marked indications of the system suffering from the poison for days, even weeks, as bitter taste, pains, yellow eyes, &c. Many, especially children, vomited; one had a sudden and extreme weakness with very slight pain to usher in an attack; another a whirling sensation in the head, with profuse perspiration on the face; a chilly sensation was common, and one had an *extreme* sensation of coolness which lasted many hours.

Pains.—The pains in various parts of the body, chiefly in head, back and knees, soon attracted all the patient's attention; they were not always fixed, often leaving or lessening in the head or back or other parts to return or increase again. If in the head it was chiefly in the forepart, often on the top; in the center alone in one case, and in which there was no other pain; and in the occiput; almost invariably in the balls of the eyes, deep in the orbits, and in one fatal case intense between the eyes, and sometimes the whole head was one mass of suffering.

When the back suffered, the pains often extended from the dorsal region to the stomach, and from the lumbar across the hips to the lower part of the abdomen, down the thighs and legs to the feet, more fixed in the calves and in the knees; a pain or soreness or aching in the lower part of the abdomen was often complained of, and was the last pain to subside; while in one it was very severe and the first symptom of the attack, and lasted forty hours.

Many had a feeling of oppression or pain at the epigastrium, and in each side, as if bound by a cord.

In a few cases the pain in the stomach and duodenum only appeared *after* the fever, and it disappeared to return again several times. In another again it presented the aspect of *bilious colic*, but soon the peculiar characters of the epidemic were present.

In some, the pains assumed or changed into the form of cramps in the extremities, to the fingers and toes, and with others in the neck, and in one case in the diaphragm; but generally these succeeded the more common forms of pain.

A general soreness of the muscles or parts which had been in actual pain, often, almost invariably succeeded; but sometimes only a sense of weariness, at others in the most cases an extreme sensitiveness of the skin; an itching over the whole body was an attendant on many at the beginning and for days.

In one case, a difficulty of deglutition was so great that fluids passed the nostrils, and a dreadful sense of constriction, impeding respiration, and giving a sense of impending death, occurred in another; and several cases of this description were observed, though to less extent, and in those the muscles of the neck were also generally implicated in pain or cramp.

A soreness of the throat, *unattended* with swelling, and with very slight redness was sometimes much complained of. There was with some a

sense of oppression within the chest, towards the latter part of the epidemic, and more distressing at the conclusion of the febrile stage, being then often the only uneasiness complained of. It differed from the pain or soreness in the muscles of the thorax. It was seated in the *lungs*, and was very distressing to some persons.

All the joints, large and small, suffered, and sometimes the bones exquisitely, also the teeth; in one case, *one* tooth of a sound sett and *not* chargeable to *calomel*.

As the case was more severe, the pains were confined to the head, back and knees, were less complained of, and sooner, finally, disappeared than when more general; an intolerable restlessness with little pain was an index of more severity. General pains, however severe, never indicated danger, and these latter varied from a mere uneasiness to intense suffering. They always moderated, sometimes disappeared entirely at, or even before, the end of the fever; the relief being in proportion to the discharges of bile from the bowels or by the kidneys. They were clearly dependant upon the absorption of bile, (*morbid* bile,) and the delay of its elimination, as they invariably lessened at a time, corresponding with the proper commencement or increase of this. In two cases pain began only after there was evidence of secretion and absorption of bile. Their return also depended upon this absorption, the person suffering from continued exposure to the poison, or the liver not *fully* relieved. These returns of pain were very common, even to a very late period in the season. With one a sense of strangling came on several times as late as six weeks after an attack, and a purge of calomel invariably relieved it.

Yet in the calm, they ceased finally while the absorption of a still more acrid bile, if any at all, though in smaller quantity, was going on; but at this period the pain was shown in a still more dangerous form and tissue—an exquisite sensitiveness in the skin, especially over the epigastric, corresponding to that of the gastro-duodenal mucous tissue and the medium of aggravating the latter as shown by friction, baths, etc., instantly causing vomiting or efforts to vomit, as I witnessed twice. During the calm, the nervous system is so obtunded—the various parts so little sympathise, as the heart with the gastric irritations—that it is only surprising this sensitiveness was formed, and the similarity between the skin and mucous tissue may explain this exception.

Color of Skin.—With a general bright redness in many persons, the skin had this color only on the face, neck, chest and hands in a great number, varying in intensity; it was a bright suffusion, and on the cheeks often a deep red, almost purple in some, and the face bloated as in drunkenness. If the skin was generally pale or of a dirty yellow, the cheeks were nevertheless red and face swollen with few exceptions, and these might be very simple cases. Although a yellow tinge was seldom absent, at some period, deep with some, slight it might be with others, there were some whose skin, were devoid of this color during the disease and showing a tinge after recovery. In one fatal case the skin was chalky, deadly white, during black-vomit.

When the yellow color appeared or increased towards, or after the period of calm, a dark yellow was not an index of as much danger, as a light lemon; indeed, the latter never appeared before this time, and

only in the severer cases, whereas a deep, dirty yellow was sometimes present from the commencement, and was of no material consequence as, in these cases, the skin was in free *glutinous* sweat, the kidneys in full secretion of *yellow* urine and large discharges of bile were readily induced from the bowels.

Eruptions.—With some, a rash extended over the body, in others it was confined to face and neck; others had red blotches and pimples on these parts and on the hands, and again the skin presented the appearance of measles.

I saw none of these eruptions after a few days continuance of the disease, though I heard of their continuing longer and have seen many who, after the *disease* was considered cured, had biles and pimples in great numbers. In one instance, the red having been succeeded by a pale yellow skin, on 4th day, the fore-head became covered with red spots like mosquito bites and they extended to the face and chest.

Cellular Tissue.—During the early part of the febrile period, besides the fullness of face, swollen even when not much reddened, the hands and fingers were seldom not distended with fluids, indeed, the whole body was puffed in a great many, and in very manageable cases. In one case, a finger, after a severe pain on the second day, gradually became *dark* red, swollen and still more painful; and in an other, the same kind of swelling (an exudation of blood into this tissue) almost purple and beginning with pain on 3d day, occurred around an ear, extended to both eyes, entirely closing them for several days, and treated to involve the scalp; they ended after many days of intense suffering, without suppuration, but with extensive desquamation, which also took place after the eruptions and other affections of the skin.

Perspiration.—The skin was never very hot and its least variable condition was moisture—the perspiration always offensive, often foetid, was at the beginning of the attack generally free or easily rendered so by a foot bath, and gradually becoming less copious and equally free from all parts of the body; it was more *sticky*, glutinous and disagreeable, or having this character from the commencement, it retained it, till the system was evidently fully relieved, or as death approached, it became liquid and then often partial. The skin moist with this glutinous perspiration, the recovery was more easy, and if dry, as it was sometimes through the fever, the difficulties of the case and the distress of the patient were greater.

In one case, fatal in three days, I was told, the skin was cool and bathed in a liquid sweat (a mere exudation), and the tongue flabby; in another, rendered dangerous by an error of food on the second day, and also with flabby pale tongue, it was cool and dry.

If after the peculiarities of the disease as indicated by the pulse etc. had been removed, a fever supervened, periodical, as happened in two instances, or irregular (from imprudences), the perspiration no longer had this offensive, glutinous character and evidently aided to terminate the fever.

The action of the skin producing this glutinous sweat was clearly an important, a restorative process, eliminating some morbid material and affording relief to the patient, yet *not* causing the termination of the febrile period, as in other fevers, or rather I should say, in fevers,—but

often beginning with it, continuing afterwards, and dangerous to be checked, persisting longer if any aggravation of the disease occurred, and returning in cases of a *true* relapse, always the same *glutinous* secretion, till the system was relieved—a curative sign, next only to copious secretion of dark yellow urine, and a continued discharge of bile, yellow bile, by the bowels.

The Eyes.—With a hot burning sensation in the lids, the eyes were red and yellow and suffused with tears at the onset—the red predominating as the fever was more intense and the skin clear and dry—the yellow more perceptible as the skin was more yellowish, and the former yielding to the latter in a day or two.

I observed them brilliant and *fiery* red in only one case, sometimes they were neither much red and not yellow, and this was the case in some *very* mild cases and in a few of great intensity. If absent in the original attack, the yellow was sure to be developed at subsequent indisposition. The yellowness of eyes was less uniformly perceived at the beginning, and more seldom absent as the epidemic advanced and the same was true of the skin.

The Tongue.—Was of natural appearance for the first 6 to 24 hours, but soon became pasty, on its base, sometimes much more, at others much less, red than natural; when pale it was generally much coated with a thick slimy fur and very moist—or it was a light red color with little or much fur. The margins often became red, though sometimes an unnatural paleness extended over the whole organ—sometimes it was dangerously pale and flabby without fur—then unduly red, dry or moist and not furred, and a slight coat appearing as the case improved—the most trivial causes quickly changed its appearance—sitting up, or improper food caused a tongue of proper color to become very red. It was sometimes red and disposed to dryness on the second or third day,—in one at the tip, within 4 hours—in four others over the whole surface, generally a redness and dryness, diffused or confined to the tip, or extending a broad, dry streak through the centre. As the case became worse, it was deep red, cracked, dry and bleeding or “natural” and then of fearful portent—if often trembled in cases of severity which was also observed to continue with some after convalescence.

It was of interest and of *practical* importance to observe how the condition of the skin corresponded with that of the gastro-duodenal mucous tissue in the different states of *action* and *inaction*. The tendency to gastro-duodenal irritations in the latter, as indicated by the tongue, was greater in those with florid skins, and its occurrence more to be guarded against at the time the greater redness existed in the skin, whether that was early or late in the febrile stage, or when, afterwards, the skin was still red, or, as witnessed often, for the first time became so. It was common, almost invariable, even after the fever, to find a beautiful florid skin with persons, previously and soon again to assume their natural pale or sallow complexion; and how sure, during this cutaneous excitation to witness the injurious, the too often fatal, effects of *solid* food, or of irritating medicines, more certainly than during the febrile stage, and, this danger was owing also, no doubt, to the impairment of tone in the *muscular* coat, greater at this period of the disease, whether proceeding to recovery or death. Yet this was a

healthful excitement of the capillaries of the skin and mucous tissue—the tongue had lost its fur or undue redness, was like a coin just from the mint. But in reverse circumstances, the case not improving, what was formed? Torpor, or lessened vitality or action and stagnation of fluids in the capillaries of the skin, and correspondingly the same in those of the mucous surface—shown in the former by the skin being dry, cool and rough or bathed in a liquid—an exudation—or giving out blood or becoming yellow; and proven in the mucous tissue by the tongue becoming *deep* red, black or “natural”, and by the exudations of blood from the tongue, stomach and bowels—all proportioned to the diminished action or vitality of the solids and to the blackness and fluidity of the blood. A glance at or feel of the skin, told of the condition of the gastric surface with an unerring certainty.

Secretions of the Mouth.—In catarrhal cases these were free, and with a hawking of mucous from the throat, were always favorable, and whether liquid or pasty, were always most disagreeable to the patient and continued so for many days after recovery. The sense of taste and of smell were impaired and with many remained so for many days after recovery, yet it was seldom, that the *bitter* taste was not distinguished and seldom was it absent—a mouth without much secretion, and the tongue not furred, had still the bitter taste, with two exceptions,—one person informed me it preceded the attack 5 days—and many declared that even ice and water were bitter—few drinks could please them, so vitiated were the secretions. The thin, white pellicle to which Dr. de Valetti refers, was formed on the gums in all instances, where the secretions were free in the mouth, but in those with decided deficiency, the gums were dry, pale, hard and totally free from it. Thirst was often absent on the first day, and with some continued throughout the attack, though it seldom failed to become excessive on the second day and continued during the fever, and into the calm,—they called often for drink yet were satisfied with moderate quantities. A *true* desire or relish for food was slow to return, but in two *chronic* cases it continued voracious, while the system was under great disturbance, and only subsided with an improvement of the health.

The Breath—was generally quite offensive, and in one instance the patient expressed the disgust it gave him, and in this, a fatal one, the skin was dry, the urine like brandy and small in quantity. It was far more so with dry than moist skin, and with deficient than copious yellow urine and not observed at all during the first month of the prevalence of the disease, and it became, after that time, seldom absent in the mildest cases.

This was the odor, the yellow fever odor which had so forcibly impressed me four years before. The lungs, the source of this odor, evidently eliminated in this form some portion of a morbid material.

The Pulse—Generally during the early part of the fever not more than twenty to thirty beats above the natural standard; was expanded and firm without hardness; in a few cases vigorous, thrilling, and in many *tumultuous*, and in only very few, tense. But as the first violence of the fever moderated, and the calm not coming on, the pulse with diminished volume and frequency exhibited a peculiar beat, which had been only obscured by the *tumult* in the system; *obscured*, because a

local epigastric bleeding, if only temporarily, developed it; developed a firm, steady, measured beat; quick, not frequent; neither hard nor soft; with distinct outline and still expanded volume. It was an approach to, without the slowness or weakness of, the pulse in the calm; ten or fifteen beats above the natural number; it gradually came even below it when the disease had fully yielded. And thus it often continued until the strength had been somewhat regained. It was not always observed at the onset; it was more than once nearly the only sign of disease, except sallow skin. It was present with the skin, either dry or moist with the glutinous sweat; it was absent when the perspiration became liquid on the case tending to death, or the peculiarity of the disease being gone, exacerbations of fever occurred from extraneous causes, returning in true relapse. It was absent in two cases of pressing danger in which the skin was dry, cool, and husky, and free from yellow tinge; the eyes not tinged, urine clear and free, no bitter taste, flabby tongue in one, and in neither any evidence of biliary secretion and absorption for many days. In one of these cases it was small, frequent, and soft; in the other small and sharp. I have never failed to find such a pulse as I have endeavored to describe during the febrile stage of yellow fever cases presenting evidence of full secretion and *absorption* of bile; nor have I ever found it in other fevers, except once in 1846 in a case of *jaundice with fever*. It was evidently the pulse of the disease in its milder and more curable form. I found the same pulse at Woodville after the bleedings, general or local, had subdued the extreme violence of the fever, and I believe it depends upon *absorption* of *morbid bile*. The pulse of the calm appears to be the same in a more extreme condition; slower, with less volume and strength, though still not weak, until towards death. In the calm, it came to fifty in one case, and in another, a negro, down to fifty-five without any other ill symptom, and in this was followed by quick recovery on copious bilious stools occurring.

Urine.—We now come to note the secretion of the most important emunctory—that of the kidneys. If the attack was sharp, the urine soon became deep reddish yellow; more red and scanty as the case was severe, and with dry skin; more orange or gamboge-yellow and free as the fever declined, and the case also improved, and often again red and scanty if it became aggravated; and the yellow color was more deep in cases attended from the commencement, with yellow suffusion of skin and eyes.

It was seldom indeed that the urine did not exhibit a yellow color, increasing at the period of improvement, or only taking place at this time. Some cases were so slight that it did not deviate from the healthy appearance except at this period; and in others again, of great intensity, it was not only not yellow until this period, but almost colorless; and in a few cases this was most marked. In one, the most dangerous I attended, the urine in full quantity was for many days very pale, and at times colorless, while the skin was clear red on the face, dry and cool all over the body; the pulse frequent and weak; tongue clean, pale and flabby, frequent vomiting of mucous; and all evidence of biliary secretion and absorption absent; these threatening symptoms began to decline, but were preceded, *first*, by a *bitter* taste, then the urine gradually became

loaded with yellow matter, irritated the urethra and deposited as the improvement proceeded, a thick, gray, thin, yellow substance, $\frac{1}{2}$ oz. to the pint. In this case, as in another, an oily fluid floated on the surface during two days; a full discharge of yellow urine so surely promised favorably that in the two cases of Long and N. Newman, I formed an expectation of recovery upon it alone under the most discouraging circumstances. At the approach of death the urine was suppressed or became scanty and like porter; though, thus represented, in one patient, it became pure yellow and in *very* small quantity, ($\frac{1}{2}$ oz.) simultaneously with a full *glutinous* sweat, six hours *after* black vomit, which was arrested during thirty-six hours, by ice, &c., and by the efforts of the system. This was the case of Mr. Vanhoosen, in which the kidneys made an effort, but failed to relive, by failing in quantity. This yellow urine was discharged for days after convalescence began, and was clearly the chief mode by which the natural cure was effected, and the last remnant of bile was eliminated; for this continued while the skin was still dry and the breath had lost its odor. The urine was only sufficiently acrid to irritate the urethra in a small proportion of the cases, and its acrimony was clearly dependent upon its containing morbid biliary matter.

Bile.—The most important of all the secretions was that of the liver. By bile, I mean a yellow secretion, varying from a pale straw to a deep orange hue; not intestinal secretions, black or green mucous *called* black or green bile; nor others with which bile may or may not be mingled. The yellow color can be developed in these by dilution or division on a white surface, and if not developed, I venture to say, it is safer to presume there is no bile present. Bile was generally secreted copiously, and either absorbed in whole or in part; removed from the blood by the kidneys and other emunctories, or excreted in part and removed by vomiting, or passed through the bowels by the aid of enemata or of medicines, and but twice that I heard of, by their natural action,—by a diarrhoea. In the simplest cases its secretion and absorption in quantity was far greater than is usual in any other fever of this climate, and was most evident in the color of the skin, eyes, urine, by the natural channels, or by the *bitter* taste; not by all in every case, yet by some one of these at some period generally, a *deep* yellow, as vomited or passed by the bowels, it was sometimes more pale than natural; in one case a pale straw color from the beginning of the attack to the period of calm when it suddenly ceased to be excreted; in others it also varied in color during the different periods of the attack, dark, then pale, to be again dark-yellow. If the color was *pale* the case was not proceeding as well as if *dark*-yellow, when indeed it was invariably in rapid improvement, and a deep yellow urine also gave more relief to the disease than a pale, a gamboge yellow, and each in greater proportion to their copiousness.

Yet there were a few cases which at their commencement and for some days showed a total failure of this secretion, as indicated by the signs referred to and generally admitted to be evidences of it, and these were dangerous ones invariably; the first evidence of its secretion gave a well-founded ground for a favorable opinion, and, as remarked before, a *bitter* taste for three days absent, was the first that the secre-

tion and absorption had taken place, and unimportant as it may seem to others, upon this alone I based and expressed an opinion that an improvement would soon take place, and the next proof was the urine described under that head; and again in another case in which, like the preceding, the vomitings were of mucous, the first evidence was a few deep yellow specks thrown up.

So important was this secretion and its free elimination, that even without an excretion, I felt that I had grounds in the severest cases to look for a favorable issue, and with excretion after the first few days, they could not die of the disease, for *at this period* the danger from the gastric inflammation was gone; death could then only occur from errors, the blood was improving, not deteriorating from the moment the bilious secretion had its proper outlet. As death approached, bile was deficient in the urine and totally absent in the matters vomited or purged. These facts are not found in any other fever, and they are unquestionably witnessed in this.

The bowels were almost uniformly constipated; they were nevertheless made to act readily by medicines, and continued to respond to an enema of tepid water, once daily in all cases where, at the onset, a proper sedative impression had been made on the liver, and the gall-ducts opened, their *occlusion* overcome by calomel. This done, large quantities of dark yellow matters were discharged; the first, after a mercurial and sometimes other medicines, were dark, blackish, intestinal secretion, with deep yellow bile. This intestinal secretion, if induced by calomel soon ceased, leaving the evacuations pure yellow, bilious matter; and to secure the continuance of this excretion, thus induced, it was only necessary to guard against, to anticipate, or subdue the gastro-duodenal inflammation by local bleeding, ice, &c., and, being successful in this, the case went on to cure with unerring certainty; *and in this is comprised the whole treatment.* A general bleeding was practised but once, and then on the second day, for a pulse of excessive vigor and tumult, by which the life was surely saved. Local bleeding sufficed to subdue the gastric inflammation in the others, the more severe cases. It was conceived that loss of blood could have no power over the disease, other than to save the organs from being overwhelmed by the violence of the febrile action, and to relieve those sanguineous gastro-duodenal irritations, which, endangering life directly, more surely led to death by impeding and arresting the excretory and the eliminating processes, upon whose continuance or restoration the recovery essentially depended. To return.

If an excretion of bile failed to take place, the bowels did not act after the first unloading, or the evacuations were thin and vitiated, muddy or watery, with or without flakes of mucous, and the case further advanced, they were of green, bileless, mucous or like thick dirty gum-water, grayish or transparent, foetid or inodorous, and finally of dissolved blood, or the excretion ceasing, after being established, the stools assumed the characters above specified, if the case terminated fatally.

In a few cases occurring towards the close of the epidemic and early in each, blood was freely discharged from the bowels, either without or with very little medicine having been taken, and was no doubt serviceable; as far as I can learn where medicines were not given, the

bowels remained constipated through the disease, though I am assured of two cases, where on the second and third days, copious stools of dark, black and yellow matter were discharged to the great relief of the patients,—sufferers from accumulations of fœtid secretions in the bowels, disposed to constipation as a morbid symptom or condition of the disease.

If the first purgative was such as to expend its force on the intestinal canal, and not to make a proper impression on the liver, an inflammatory irritation was thereby often induced, and if a repetition was made at later periods of the case or the epidemic, it often sealed the fate of the patient, and never failed to aggravate the case, though some were so mild at an early period as with difficulty to be harmed. To such cases the physicians were often called, and they had to combat these irritations often without the hope of success, but with the certainty of a *count* if they failed. Intestinal secretion had but little concern in affording relief to the disease, at late periods none, and a revulsive action on the bowels was incompatible with a just view of the disease and with the fact of the strong tendency to*—with the presence of inflammation or engorgement in the mucous tissue, and of course, with the safety of the patient; therefore, the object was merely to procure a discharge of biliary secretion as it might accumulate in the bowels by the mildest measures, by enemata. But these often failed for days and days, even while the accumulation was evident, and in the absence of the signs of gastric inflammations, it was often proper and safe to evacuate the bowels by a *sure* purgative; a mercurial often the best, but others proper and safe, by which large masses of feculencies were brought away, to the great relief of the patient.

An inflammatory irritation positively forbid a purge; mere torpor of the bowels did not.

The bowels were generally very easily acted upon by medicines at any period of the case, and this very facility, this excitability of the mucous tissue, was the source of nearly all the danger, and great indeed was the damage from medicines of this class, other than mercurial, especially after the season had advanced, and if administered at late periods of the case. But calomel, by making or continuing the proper impression on the liver and on the gall-ducts, causing them to *emulge*, (that is the old and expressive word;) Calomel, by doing this, *never* irritated the mucous surface to a harmful extent; it was often a sedative to its excited condition and did not purge, as in other fevers. There is no disease in which the inflammation can be so readily overcome as in this, *if* the liver is in full secretion *and* excretion. Calomel given at an early period, and proportioned to the case, never failed to effect this, and when given later, even after the inflammation had set in, but not intense, or when temporarily allayed by local bleeding and ice, it was still the remedy—the *divine* remedy in this yellow fever. In that at Woodville I usually gave 10grs., too often less, and sometimes 5 to 10grs. blue mass instead, and always to regret it; at others I gave 20 and to very few 30 grs. In two cases in which I gave 10 and 15 grs., I deeply regretted they had not been 30 and 60 respectively. One proper dose at the onset always rendered repetitions unnecessary, but with so many mild attacks, the proper time for the full sedative dose, required

* This sentence is, to us, unintelligible.—Ed.

in the intense ones passed, as will always happen in mild epidemics before its necessity was indicated.

Calomel may do harm; it may—it will kill; so may, and *does*, quinine in *periodical* fevers more often than people have been aware of. What then? Let us learn when it aids, when it prevents recovery; use it then, avoid it now; and let us not fail to do the same with calomel. The vast majority of the cases were such as were able to go through the cure *naturally* by the processes referred to; that is without *medicines*; though few failed to have the aids of ice, mustard pediluvia, &c., and a *little* medicine taken by themselves to start on; but with these only they dragged heavily, but still struggled on to a relief, often imperfect, and through much more suffering and risk than when properly aided, and yet better when the curative processes were *interfered* with. These curative processes could be easily induced and easily checked.

The bearings of the following case of Mr. Soher, which occurred during the more inflammatory form of yellow fever at Woodville in 1844, and was published in the New Orleans Journal for September, 1845, will be apparent to those who read it, and compare it with the preceding history and with the two cases which succeed—

Mr. Soher, aged 27, was attacked September 14, 1844, and was treated during the first few days by Dr. Brown, his preceptor, by bleeding, cupping, two doses of calomel of 25 grs. each, blue pills, &c. On the sixth day, he had a pulse, full, firm and about 120; no pain, flushed face, skin slightly tinged, tongue red and dry at the point, with dark, thick fur at the root; his stools had ceased for twenty-four hours, having been only slightly tinged yellow from the beginning. He was taking 5 gr. doses of calomel; I advised the discontinuance of all medicine, to depend upon local bleedings freely from the spine and epigastrium, which was adopted and continued for twenty-four hours, when he refused to lose more blood. During the next twenty-four hours, I declined to advise; he took three doses of charcoal and magnesia, had a blister to the epigastrium, vomited, had hiccough, the tongue more dry and less furred, and pulse still strong. In this aggravated condition I again urged leeching to the epigastrium, full and frequently repeated, and no medicine. During four days he was leeches two or three times every twenty-four hours; the first night with seventy-five American leeches at three leechings. His stomach would retain only a tea-spoonful of gum-water, solution of bicarb. soda, &c. Mild injections were frequently used and came away untinged.

His skin became deep orange, reddish brown at some parts. His urine was copiously secreted, two gallons were once reported to me in twenty-four hours; he could at almost any visit pass nearly a pint for my inspection, of a dark reddish yellow color, leaving a thick coat of yellow on the vessel in which it remained six hours. The perspiration was slightly yellow.

At the end of four days his tongue had become pale and moist, pulse still of good strength and much slower. I advised calomel at this moment; he took 5 gr. doses every two or three hours and one 10 gr. dose, until he had 25 grs.; he was leeches once during this time; a tepid water injection now repassed, tinged yellow, with old fecal matter, four or five small evacuations, becoming a brighter yellow, followed in twelve

hours, making his recovery certain. He continued to improve; he began of his own will and without my knowledge, to drink strong coffee, when the tongue reddened and became dry, and the evacuations ceased. He was leeches repeatedly and without benefit, when Dr. B. told me that he had been drinking coffee for three days. He was placed under my exclusive care, had no more coffee, and the leeching now removed the redness and dryness of the tongue; he took 10 grs. calomel; yellow stools recommenced, and his convalescence again began, and went on without intermission slowly to health.

During seven days he had no discharge from the bowels; no bile was thrown into them, but it is not so clear that it was not secreted in great quantities, absorbed into the blood, taken up and expelled by the kidneys. It is more probable that these organs will take from the blood bile ready formed, than that they can combine its constituents.

I think his case could not have been brought to a happy issue but for the performance of this office by the kidneys. The action of the kidneys, so truly vicarious, must be regarded as of the same conservative character as a similar action of the skin preserving the brain from a fatal coma in suppression of urine.

Mr. J. H. Long, aged about 26 years, and the first summer in the South, was seized at 2 o'clock, 24th September, 1848, with a whirling sensation of the brain, and profuse perspiration on the face; pulse frequent and the mind under great excitement. A mustard pediluvium, 15 grs. calomel, and in eight hours $\frac{1}{2}$ oz. oil were directed; the oil was rejected and no alvine discharge until on the 25th, an enema produced a copious one, a mere unloading of the bowels.

Early on this day, the 25th, the tongue became red and dry at the tip and a little furred; the eyes fiery red, face flushed and cheeks deep red; pain in forehead and back intense and absent elsewhere, and subsequently, during the fever, the epigastric distress and restlessness was excessive; occasional vomiting of mucus or the drinks; skin hot and dry; pulse hard, sharp, but little expanded, and from 90 to 110 during the febrile period, which lasted about 72 hours.

The skin retained its clear red color until near the end of this stage, when the redness of the eyes, having previously yielded to a yellow, and the redness of hands and chest declined, that of the face and forehead became tinged yellow, and extending gradually to the chest, it involved the whole body in the same light color, and subsequently the forehead, neck and chest were covered with spots like those of the bites of mosquitoes; his dreams became so horrid that he assured me he was *afraid* to sleep.

During the febrile stage I gave no medicine except 10 grs. blue mass, which was rejected. I relied upon three full cuppings at the epigastrium, ice, bathing the skin and upon water enemata, which always failed to produce a discharge, as in fact the intestinal secretions and biliary excretion were entirely arrested.

The local bleedings invariably softened and reduced the vigor of the pulse, rendered the tongue moist and pale, and by subduing the excessive throbbing, gave great relief.

The period of calm approached, and a blister was applied to the

stomach and right side, in hopes to subdue the irritation and insure the excretion of bile at the critical moment near at hand.

The pulse came down to sixty; the skin was cool and exquisitely sensitive over the abdomen and thighs, and the pains gone, while the general distress was greatly lessened; the calm was fully present; an enema was returned colorless as before, and with thin flakes of *green mucus*, and my fears were confirmed—I had failed.

I now joined Dr. Lyle in the treatment of the case. He found the tongue moist, pale and still furred,—proofs that he had gone into the calm in as good condition as was possible for any one. His organs had been saved from being overwhelmed by the violence of the febrile action, but the excretion of bile had not been effected. The time within which to attain this important end, or to produce an action on the liver, were conducive to healthful secretion and being limited, we gave 15 grs. calomel at the intervals of four and eight hours, because we knew in the continuance of these *foundations* of the disease, that the tongue would soon become dry and red, or fatally “natural;” that the gastro-duodenal inflammations would supervene. On the next day, the 5th of the disease, this redness and dryness of the tongue with pulse slightly excited, was present in slight, though alarming degree—it was evidence that we had failed.

The time for medicines was passed, and ten Swedish leeches were applied to the gastro-duodenal regions; before they had fully drawn, he expressed relief from a painful sense of constriction over that region, and after the leeching was completed, he said all distress was gone—he could expand his chest with ease.

The tongue became fully moist, lost the redness and, with pulse improved, remained so during the night with great amelioration of his condition.

During the early part of the fever, the urine had been red and scanty, and as this period declined, it gradually became less red and more yellow and frequent, though not full, until now, it was full and deposited a yellow pellicle on the vessel, and thus continued, until Monday, the 8th day.

The leech-bites discharged a red blood; moderately, during Friday night and on Saturday until Sunday, becoming gradually more free, more liquid and black.

On Saturday night he had camphor water; on Sunday his strength was failing; the actual cautery was applied to each bite, the bleeding instantly ceased and never returned.

Champaigne with pounded ice was given freely, ($\frac{1}{2}$ pint in two hours,) with prompt good effect, and being continued through the night, though in much less quantity, the *secretions* of the mouth and throat became free to such an extent that he hawked and spat; at that stage and in this disease an important proof of an improvement.

During forty-eight hours preceding this, there had been only two small alvine discharges of thick green, though bileless mucus; but during this night, at 2 o'clock, there was one of a dark, thin, gruelly substance, surely from the *upper* portion of the bowels; the secretions of important parts had begun.

Yet he had passed a restless night, and on the next morning it was evident that the wine had not been sufficiently *permanent* in its effect.

He was under great mental excitement, an idea of approaching death was impressed upon his mind, and he seemed a wreck more from this than from physical prostration, and the event proved that it was partly from the evanescent nature of the stimulant, though more from disappointment that *he* was not yet well, while a certain person, he had been informed, had been taken sick and was already recovered under homeopathy.

To decide upon a change of his medical attendants, and upon so radical a change of treatment was to decide the question of his life or his death and might well create the great nervous agitation under which he labored. Without knowing he had this idea of change we gave hot brandy and water in a stiff dose at 7 o'clock and directed it repeated, which being done, he was found at 10 o'clock with moist tongue, skin uniformly soft and warm and pulse 120, necessarily frequent from the mental effort of the preceding 3 or 4 hours, and his wishes not yet effected—and far better in this disease to be frequent than slow—and more important than all, a *bright yellow* discharge had taken place from his bowels—small and thin—it was yet *bile*, and the object of all our solicitude had been attained—he was *safe*. At my invitation, Dr. Jones had, fortunately, seen him the day before, and I sent for him to witness his condition which he pronounced to be *decidedly* better, and Mr. T. Henderson who had been with him all the night before, also said he was better—and under these circumstances he was placed under Dr. F. A. W. Davis and Homœopathy.

To decide the question of wine and brandy had been to pronounce upon his life or death—the wine, guarded in its first exciting effect on the stomach by ice, had pervaded his system—thrilled every fibre—had given new life to the blood and the secretion and excretion of bile was the consequence. It was the appropriate remedy, to be followed by brandy when it proved, as we knew, and as was suggested, it might prove, too evanescent.

They were adapted to his condition, they gave him the *certainty* of life and yet he died.—Why?

The brandy was reduced to a *tea-spoonful* in a saucer of arrow root, and on this he was kept during the subsequent treatment—at one time he asked for something to eat, saying he was *hungry* and he was told, he might have as much arrow-root, *thus seasoned*, as he wished—on this and Homœopathic *nothings* was his life staked—on arrow root—on nothing—was his blood left to be replenished, blood which required what is almost its equivalent—strong essence of beef—could *not* be replenished by arrow root, the least nutritious of all farinaceous articles.

At Mr. Long's request I visited him on Thursday; his tongue was then of an anæmic paleness, and dry and furred; pulse feeble and 108, and skin cool. Again I visited him at his request, on Friday; the pulse was still at 108 and more feeble; tongue moist at edges and pale; his mind much enfeebled. He had just before had a large pultaceous bright *yellow* discharge from the bowels which I saw and which I called upon Dr. Jones to witness; he had another on Sunday, while the urine, I was informed, continued full and yellow through the whole time, and he

died on Monday of *utter prostration*, of inanition—literally of *inanition*, for his stomach was found empty at the post mortem examination, a portion of yellow mucus-looking substance, about two inches long, was all that it contained.

Immediately after witnessing the post mortem examination, I made note of the appearances as follows.

External appearance of skin light yellow, bowels on serous surface dark red, on internal surface dark red in the large intestines and lighter red as the openings were made higher up, and in the stomach clear red, and as rubbed with the sponge losing much of the redness—no ulcerations seen or pointed out in bowels; contents of bowels, a pul-taceous matter of dirty yellow appearance tinging the operator's hand yellow, and tinging the water yellow in which the sponge was washed; gall-bladder full and of greenish yellow appearance externally and not opened; the liver firmer than natural, not having the clear dark red of health, but a dirty light greenish yellow mixed with the red—no cause for death seen in this organ.

This is a meagre post mortem it is true, but with the history of the case, the treatment under Dr. Lyle and myself and the treatment by *tea* spoonful of *brandy* by Dr. Davis, is amply sufficient to show the utter impossibility of his disease being, or of his dying of "Inflammation of the stomach and bowels" as Dr. Davis reported to the sexton—but had the report been, died from "Exhaustion resulting from fever" or of "Debility" and this not remedied by stimulants and nourishment as were two others, then indeed, would it have been truthful.

The *redness*, so *diffused* over the whole mucous surface from stomach to the lowest part opened was *no* evidence of inflammation, was proof of the contrary, was produced after death by the impoverished blood, in vessels previously debilitated, and liable to this engorgement from these causes.

In speaking of the condition called inflammation, produced by the liquidity of the blood, Magendie says at page 169, London Lancet, Vol. 1, 38, 39 "*They* limited its action to living organs; *I* have extended it to the tissues when they have ceased to *live*. Many and many a time have I proved by experiment, that its most terrible *symptoms* develop themselves in parts *wholly inanimate*"—that is, such symptoms as the redness, externally and internally, of the bowels found in Long's case, which will more fully appear as we proceed, in further observations from Magendie.

And then the character of the discharges on Friday and Sunday, and the contents of the bowels after death, totally disprove the *inflammation*; but it is painful to prove the cause of this noble young man's death, of him who said to me "I am the last prop of my father; if you cure me I will consider you as my father; and to whom I replied, *when* I cure you we will be as brothers"—Yet a duty to myself and to humanity imperiously demands it.

Nelson Newman, a delicate boy 12 years old, was attacked Oct. 15th, with violent pain in the head, back and duodenal region; the skin was hot, dry and bright red, face swollen and deep red; vomiting during two days of mucus or the fluids drank; so distressed and delirious as to be uncontrolable during four days, and sleepless for six days and nights—

the least doze being broken by frightful dreams—on the second day he had a hoarse, croupy cough, lasting 4 days and subsiding without expectoration.

The tongue, previously bright red, and frequently dry, became *dark red*, though very moist with a watery fluid, twelve hours before the calm, and the thirst excessive for many days afterwards.

The alvine evacuations, small, frequent and painful, were, from the commencement, a pale straw colored fluid of thin arrow-root consistence; they lost the yellow color at the access of the calm, which took place after a febrile stage of three days.

The treatment of this period was by 10 grs. calomel, $\frac{1}{2}$ oz. castor oil, foot baths, ice, two cuppings, one leeching to the abdominal region, and a blister to the liver and duodenum at its termination.

In the calm, the skin was dry, husky and cool, and having lost the red color became more yellow; occupied with various fancies, he frequently started from a momentary quiet to get up, or tossed from side to side, with much strength; and the peculiar odor of the breath, present before, increased to a most offensive degree.

The stools small and perfectly inodorous, had the appearance of a thick gummy substance, at first transparent, then greyish, and were passed every 4 or 5 hours; during 4 days they continued thus, becoming less frequent, and the two last at long intervals, and darker colored and odorous.

The urine slightly colored for several days, on the fourth, became more yellow, and finally deep orange, and in full quantity, coating the vessel with a deep yellow deposit, more and more, as the case proceeded.

In 12 hours after the calm, the tongue lost the deep red color and *then* three portions of calomel of 6 grs each were given at intervals of 6 and 12 hours and applied hot salt water frictions to the skin repeatedly. For two days and a half the tongue threatened to become "natural", the pulse became feeble, and not frequent, the skin remained steadily dry and husky with yellowness increasing—blood oozed from and formed black crusts on the lips, the blistered surface oozed a black blood, the teeth were covered with a dark sordes, and on the 6th day the tongue was covered with a sticky foetid dark substance, evidently, in part, blood, and which *perhaps* came from the lips; or was it exuded from the tongue? His gums were sound.

Under these unfavorable circumstances, gradually forming, the only encouraging symptom was the *deep yellow urine*, in good quantity, and I constantly relied upon it, gave encouragement, and waited for the time when it would be proper to stimulate. This had perhaps, though I believe not, more than arrived on the morning of the 6th day, and he was placed on the full use of champagne with ice freely.

In a few hours the tongue was free of the foetid substance, was moist, but became dry in the evening when the wine was discontinued, and resumed in 6 hours.

The pulse quickly filled out, was firmer; the skin gradually softened on the forehead and neck with the *glutinous* sweat, the urine became even more free and yellow, the odor of the breath gradually lessened; he began to sleep by half hours and hours, continuing to discharge

deep orange colored urine, taking wine, ice and gruel,—he improved steadily, and on Monday, the 8th day, had an alvine discharge, of a *dirty, yellow, consistent mass*—thens trong chicken water and beef tea were given with less wine.

The odor was gone, the skin was, in a few days, in full perspiration, no longer glutinous, the bowels acted daily more and more naturally, and the urine becoming, by degrees, less yellow, was of healthy appearance soon after a week's time.

His convalescence perhaps began with the use of the wine, but could only be depended upon when it was certain the biliary secretion had its *natural* exit from the liver; and until then, the kidneys acting, may I say *vicariously*, but if not, *certainly*, as in the cases of Soher and Long, *saving* the system, saving the blood from destruction, until the disease *ran its course* aided or not,—impeded or not by the means employed, as each person may believe according to his knowledge, his fancies or his wishes.

What *name* for this long array of symptoms and conditions of body and mind, found in a disease of such general prevalence, occurring during the summer and fall in a City subject to Yellow Fever? What were *essential*, what *incidental*? what *curative* and to be aided, what *morbid* and to be repressed? and finally what derangements of the system do they announce?

No one can doubt that *Yellow Fever* is the name by which to call the disease—one, the most reliable of all single signs of this fever, or rather *disease*, in its severe form, was present—the *slow* pulse, the *calm*, and found in cases *mild* in their commencement; we have the next, singly, the most pathognomonic, the *slow respiration*, present *during* the most *tumultuous* excitement of the circulation, and in the article of death; then the *one* febrile paroxysm; the debility excessive and totally disproportionate, giving the early necessity to replenish the liquid, black blood with liquid-blood-like-nourishment, *solid* food, after “breaking the fever,” in other fevers, being generally *health* giving, and at the same stage of this, *death-dealing*; the tongue of healthful appearance for a certain period, assuming the characters pointed out, not indicating danger to the *uninitiated*, when to others a sure sign of death; often the same false inferences by the same persons, from the state of the skin, the countenance, the feelings, and the conversation of the patient; the peculiar odor of the breath; the black and liquid blood in the more severe grades or advanced stages of the *very mildest*, rendered worse from *trivial* causes, or from medicines, true *remedies* in similar stages of other fevers;—the yellow skin, so seldom absent—but *once* here at the fatal termination and then deadly white during black vomit, and *never* absent after death;—this skin might be omitted, except that being *combined* with black vomit, or with exudations of blood from other parts during life or after death, proving the *same* condition of solids and fluids as that productive of black vomit, *thus connected*. The yellow skin occurs in no other fever, and is therefore distinctive of this; the black vomit might also be omitted, but for the same connection with the yellow or with the *deadly*, chalky white skin becoming yellow after death. *Singly*, the color of the skin and black vomit are not to be relied upon—*combined*, they are unerring; yet the *peculiar* mode of vomiting, *some-*

times seen, ought to indicate Yellow Fever, for I believe, that mode, aptly compared by Lewis, of Mobile, to a *pumping*—never occurs in any other disease, and it was present, here, with some;—and then we had the pains—the unimportant pains—those of the head, back, knees and calves of the legs, generally present in Yellow Fever though not essential—absent in intense grades elsewhere, as also here, in the *same*. The general pains, often present in *mild* Yellow Fever, are certainly, and in the same manner, found in another disease, and quite unimportant in this; and the *eruptions*, equally a mere nothing, and seen in other diseases—a mere evidence in this, as in those, that the blood is *vitiated*;—and finally, though not least in importance, indeed, being, as I believe, the origin—the *fountain* of nearly, if not, *all* the preceding, we have those *never-failing* conditions of the liver, and its secretion in Yellow Fever, shown; either, first, in the evidences of bile, in more or less profuse *quantity* and *vitiation*, excreted or not, and absorbed, and freely eliminated in the milder, and not excreted, and not freely eliminated, but still *secreted* and absorbed in more severe forms; or second, in the usually recognized signs of the total abolishment of the secretion, or what may rather be, its still greater *vitiation* and *reduction* in quantity—a more *concentrated poison*—rapidly causing death in the intense grades. The *first* always *self-curing*—the *second* requiring well devised aids, and the *latter*, perhaps beyond human power to relieve.*

I had seen Yellow Fever *mild* for thirty or forty days of its prevalence at Woodville in 1844. I had the necessity and fortunately, the frequent opportunity of studying it from its very foundation. I saw, as I thought, the corner stone laid before the superstructure was erected to obscure this important point. I had a sad experience, for, besides the loss of many patients, I saw one of my own children die with *white, bileless* stools, and black *arterial* blood, and another recover without medicine through three or four daily discharges of dark yellow bile and mucus,—at first I was more attracted by this mode of natural cure, but I soon became aware of others—of that by the kidneys, which was so well exhibited in the case of Soher. And I am now assured that the skin in its *glutinous*, not liquid perspiration, and the lungs in the *odor* of the breath, even the mucous surfaces of the chief portion of the alimentary canal—all eliminate some parts of the acrid bile, or other material, if others there be, in the blood. And that the discharge of bile by the bowels, only now and then occurring naturally, can justly be likened to a “grand cut off,” a veritable “Bon Dieu,” diverting a *flood* of morbid bile from the blood, avoiding the impediments it may meet—the dangers it may *cause*, while winding its devious way through a long channel full of danger, perhaps without an outlet in the emunctories, and *then*, with the certainty of the liver becoming brittle—indurated, yellow, dry, and impervious to blood; saving the system from these dangers, and rendering the arrival to health sure and more speedy and with less suffering.

When in the Fall of '44, I spoke to Dr. W. Stone, of New Orleans, of the attack and natural mode of cure of my child, he replied “such must be the manner in which many children are sick and get well in New Orleans, and who, sallow and feverish for some days, recover

* The author's meaning in this paragraph is a little obscure.—Ed.

without much, if any medicine"—and true enough, no doubt many children thus have yellow fever and acquire that immunity which is attributed to their being *Creoles—born in the country!* whereas, the fact is, they may be, and sometimes are, born *in the disease*, or have it under the appearance of Croup, of a cold, hoarseness or biliousness, and getting well naturally, or under treatment, the case attracts no attention either as to the character of the disease, or its natural mode of cure.

Severe or malignant forms of the disease are taken as models in the various descriptions—mild Yellow Fever is not described—it is said to get well if let alone, but its *mode* of getting well—its *natural cure*—is never pointed out, and in these errors lie much, if not all the uncertainty and contradiction respecting the disease. The consequence is, not only uncertainty, but many medical men scarcely know that Yellow Fever *can* be mild—they must see black vomit and death, and even then there is uncertainty, and well there may be, for these are erring; there are more certain signs of the disease appearing early—indicating the disease, while yet something may be done and much avoided and long before this black vomit, which indeed may never appear. A poor workman present at the building of *very* many, might tell how the Pyramids were, at least, begun to be built, but once erected, towering in all their majesty, every thing is uncertainty, confusion and contradiction with the most scientific.

It is certain that the mild is the grade in which the disease can only be studied with advantage, and then, by comparison with the more intense, something to be depended upon may be elicited.

I have not feared to speak of, to dwell upon, the *liver*, the *bile*, *calomel*, hazardous though it be now they are so out of fashion. I have long since believed that the liver and the bile played but a secondary part in our fevers, even in *bilious* fever, but in Yellow Fever it has appeared to be the *first* organ deviating from health, and that to its fractional derangement—in its increased, *vitiating* secretion and absorption, or perhaps in its total abolishment, can be found the chief—the *never* absent, symptoms and conditions of the disease—while to an altered state of the blood, even if from another and more occult cause, is due others, equally *never* absent in the more violent, at the fatal termination of the milder, and perhaps at the commencement of all grades.

The discrepancies respecting the liver and its secretion are evidently more apparent than real. The facts of its being affected and of its not being affected, as shown by dissections, are both true facts, and those of immense quantities of *bile* and *no bile* are also true facts—each set depending upon the degree of violence with which the poison has assailed the *function* of the organ, causing mild or intense attacks; the liver affected in one and not evincing it in the other—bile in the former, and no bile in the latter grade, that is, none *excreted*, but secreted, sometimes profusely and absorbed and eliminated or not, as recovery or death takes place, but *always* bile, except in a few cases, rare indeed, and seldom *not* mortal. And this exception proving the truth of the importance, attributed to the liver, for death is owing to this very fact of the abolishment of its secretion.

It is also called the most insidious of all diseases and yet in the de-

scriptions by those conversant with it, it is positively asserted that such symptoms lead certainly to recovery, while another *set* as surely portend death. The basis of these *certainities* of life and death, in a most *insidious* disease! what is it? *bile* in one, and *no bile* in another,—two opposite conditions rest upon the liver and its secretion.—Bile or no bile, in this disease, are synonymous with life or death, as will be seen.

It is not long since that much dispute existed whether this or that form of the fever which prevailed in this or that Island of the West Indies was Yellow Fever in one, and not Yellow Fever in another, so various were the descriptions of the *same* Fever.—All true, and yet apparently so different.

Dr. Good, than whom no man was equally capable in examining these discrepancies and extracting the truth, made the following remarks, the justness of which in their main points will not be disputed.

At p. 377. vol. 1. "Study of Medicine," he says, "in the midst however of so much discrepance, there is still much that is concurrent, and quite enough to establish the identity of the two diseases, if an abundance of other evidence to the same purpose were not at hand.—The Fever of Dr. Pym, specifically characterized by black vomit, is represented as being peculiarly dangerous and fatal; in that of Dr. Musgrave, this symptom only occurred in the most perilous cases, according to the latter the severest and most deadly attacks were among the newcomers; the mildest among the natives, or those whose constitutions were assimilated to the climate. — The yellow hue of the former (and I have already endeavoured to account for this) was of a *deep orange*; that of the latter a *lemon color*.—Dr. Pym describes three species of fever as common to warm climates, but which differ from each other in their mode of origin and diagnostic character.—In that of least danger, the colour of the surface, he tells us, is of "*a very deep yellow*"; in that of higher danger, it is of "*a deep yellow*" and in the disease before us, which is by far the most fatal, where there is any yellow at all, it is of "*a very pale lemon colour*;" which is in effect the very hue ascribed to the severest cases by Dr. Musgrave, as the "*very deep yellow*" or "*orange*" is to the mildest.—So that examined by their external livery as well as by their internal disorganization there can be no doubt that the two diseases are the same.

Dr. Pym appeals peculiarly as a distinctive character of the Bulam Fever, to the deadly and chlorotic paleness exhibited by the countenance in its latest stage or *most fatal incursion*.—But even this only shows, that in such case the disease makes a mortal attack upon the larger viscera, and especially the liver, *from the first*; and demonstrates the proposition I have ventured to lay down, that, in proportion as this organ is severely affected, is its inability to secrete propre bile, or indeed bile of any kind; and consequently that if the irritation only reaches a certain point, its secernments will be stimulated to emulge a larger quantity and of a deeper hue; a considerable portion of which will be absorbed into the sanguiferous system and produce the orange tinge, which, in the description of both these writers, peculiarly marks the disease before us in its less fatal attacks: while, if the febrile incursion be so violent as totally to derange the function, and still more the structure of the liver, no bile will be secreted at all, or, if secreted,

less in quantity, and consequently less diffusive in colour; and hence conveying a chlorotic, or livid tinge to the face, which at the same time exhibits a bloated fulness from effusion or debility of vascular action.

In confirmation of this remark, Dr. Jackson's earlier cases of practice furnish numerous examples; "examples indeed", to adopt his own words "of that form of disease when there is a considerable degree of vascular excitement in the early stage, *terminating* commonly by deranging the functions of an organ of importance, most frequently the liver or stomach."

Nothing but the improbability of an exposition of any part of this disease coming from a person never having seen it, or the most determined theory, can account for such approach to truth being overlooked or neglected. Had Dr. Good seen the disease begin in its extreme mildness, and proceeded through whatever causes to death, or had the same been presented to that close observer, Dr. Jackson, who acknowledged his total ignorance of the disease from his readings, and the necessity of examining it for himself on his arrival in Jamaica in 1774, they certainly would have perceived, that the liver was the *first* organ affected—that the poison not the disease deranged the function; that the derangement was from an excitation or abolishment *purely nervous*, which Good so well knew was alone sufficient to *vitate* to any extent, *greatly increase* in quantity, or *entirely suspend* the secretion, upon which, and its absorption or upon its "total" failure, he lays so much stress as proving identity, as distinguishing, yes, *constituting*! the milder and the mortal grades; also that "the febrile incursion"—"a considerable degree of vascular excitement" was the *effect*, not the *cause*, of the secretion and absorption, and a consideration of the facts that "no fever ever makes its appearance" * that the "pulse is slow and waveing", † in the mortal attacks "in the walking", "the congestive cases", would have shown them that the "total" derangement—the *abolishment* of the function was the *cause* of this condition of "the deadly and chlorotic *palleness* exhibited by the countenance in its latest stage, or *most fatal incursion*", and which, indeed, Dr. Good would have expressed, had he said that the *poison*, the *cause* of the disease, rather than "the *disease*, makes a mortal attack upon the larger viscera and especially the *liver, from the first*."

In stating the various facts it will often be necessary to include the opinions of those recording them, both being, sometimes, intimately interwoven—and it will sometimes be difficult to place facts in the position which they may have been intended to occupy with respect to the stage or to the mildness or severity of the case, as favourable or unfavourable in its termination, as often, too often, they are mere statements of facts; but I hope to succeed in placing some of them in their *true* position.

Doct. Lewis, in the New Orleans Med. Jour., July, '48, p. 40, states that no sophistry or authority could induce a creole or nurse in Mobile to the belief, that a man labouring under Yellow Fever either vomited or purged bile."

Doct. Harrison, in the same Journal for Nov., 45, p. 324, has said that "as, for the liver, the symptoms of the disease throughout its whole

* Harrison. † Lewis.

course, as well as *post mortem* examinations, show that it is not particularly affected, the passage of bilious stools, is as common an occurrence as we meet with, though not a grain of any mercurial has been taken." And Doct. Rush declares that "he was surprised to find so few marks of Hepatic affection"—p. 57. Med. Enq. vol. 2.

Many other Authors might be cited to similar opinions and facts, but these will suffice as general positions and somewhat as texts on which to proceed.

It is evident that Doct. Rush founds his opinion chiefly upon the fact that he "met with but two cases in which the patient could lie only on the right side"—and from the absence of soreness to the touch in the pit of the stomach"—pain, swelling, soreness, inflammation &c, were in his day considered almost essential in primary affections of the liver.

In another part of his writings, he speaks of it as being sympathetically affected—and his whole work is full of proof of its *functional* derangement, as will be seen as we proceed. It was of *organic* or *inflammatory* affection of which he found so few marks.

Doct. Gillkrest's dissections of upwards of 200 cases, and his observations in the Gibraltar Epidemic of 1828, established the fact of absence of organic lesions of the liver, and "he and his brethren came to the conclusion that it was the *function* of the liver that was deranged in this malady." "The liver in ordinary cases was the organ most frequently presenting well marked morbid phenomena." And the evidence of this was chiefly found in the color of the organ, usually a *greenish yellow* or *pale olive color* and *dry*. "The organ was rarely enlarged in volume, no adhesions, abscesses or signs of inflammation were visible." "In cases of *extraordinary malignity* and *rapid mortality* this change of color was seldom well marked—but in a considerable proportion of these cases (200) it was sufficiently so to show that the organ was *passing into that state* which evinced the phenomena above mentioned. Here the engorgement appeared to be the principal character, though it was not invariably present."*

Of equal opportunity with Doct. Gillkrest I present the observations of Doct. Harrison, as found in the N. O. Jour., Sept. '45. He says "he has never seen any lesion in this organ (the liver) which could be attributed to the effects of the disease, the appearance, being at times very dark; in other cases, presenting a *pale yellow aspect*." "It is *paler* and *drier* than usual, at other times, however, it is *engorged* with blood." "In the worst cases, those of a *congestive* character, no lesions occur." This is spoken generally and includes the liver. "Chronic affections are met with, but also an equal number of cases at other seasons, and they have nothing to do with Yellow Fever, either in cause or effect."

How exactly these descriptions accord, and Doct. Nott, in speaking of the yellow colored, dry liver, says "it is not more constant than other lesions, and it is worthy of notice that there is a *marked difference* in the *frequency* of this lesion in different epidemics here." To what can this be owing but to intensity of attack? The disease is the same in other respects.

Doct. Pascalis, of New York, witnessing the Yellow Fever there (50

*See Med. Review. 1830, and Cyclopaedia of Practical Medicine., Art. Y. Fever.

years ago) so common, and of comparatively mild grade, said "it is more than probable that the liver, *always* found by dissection to be of a *Nankeen* color, is the *first of the viscera deprived of its excitability*."*

Doct. Frost, observing the Yellow Fever of the most intense grade in Guiana, in 1803 and '4, wrote in Med. Rep., Vol, 12 and 13, that "in most of the subjects that I dissected the external membrane of the viscus was either in an inflammatory or gangrenous state, which sometimes penetrate into the substance of the liver, more especially its posterior convex portion. It was sometimes found preternaturally enlarged, of a deep purple or livid complexion, and again remarkably *indurated*." The *inflammatory* and *gangrenous* states will receive at *this* day their more just explanation in the fluid condition of the blood, and that this was the cause of the appearances he called inflammatory and gangrenous, is evident, as he says the lungs were, in a few, "in an inflammatory state" without adhesions "and the same is stated of the *stomach, bowels* and *urinary bladder*, while the blood formed in the heart, arteries and veins, was almost always unnaturally dark and fluid. Doct. Gillkrest would say of the *indurated* liver that it "*was passing into that state*" yellow and dry; or will not others rather suppose, not only of Frost's indurated liver, but of all those, yellow and dry, in Gillkrest's cases that the function of the liver had not been so completely overwhelmed by the poison at the onset as not to allow *some* secretion and absorption and deposit in the organ?

Dr. Frost says, "the blood drawn was in some dense and almost black, and if allowed to stand a little while, was as compact and solid as the substance of liver, and frequently without any appearance of serum on its surface; when in other instances it would be thin, and dissolved, and of an unusual *yellow* cast, and sometimes appeared greasy as though oil had been mixed with it." Of frightful grade, surely, was this yellow fever at Starbrock, Dutch Guiana; but still the proof of secretion and absorption of bile, not only in the blood, but in the skin, as he describes elsewhere; so rare is it for a "*total*" *suspension* of secretion and, still more rare, so *impossible of absorption* in *all* instances *with fever*, and yet so *common* for the power of *execution* to be abolished.

From Dr. Lewis, I quote that O'Halloran, in sixty cases examined, found the liver to be in all "of light straw color, atrophied, dry, brittle and gritty;" and it is impossible to suppose that the disease at Barcelona was of as malignant grade as at Guiana, or as sometimes in Mobile, or New Orleans, or as at Gibraltar.

There is to be found no evidence of *sanguineous* irritation, no swelling, no suppuration, no adhesions, no lesions, recognized *at present as organic*, as cause or effect, during life or after death by yellow fever. The affection results from, and is proof of, a purely *functional* derangement. Yet surely it is not exactly correct to say the liver presents no evidence of organic lesion as an effect, when in its yellow aspect state it is found to be so firm, tough—imparting the idea of wet sole leather; even cartilaginous, brittle, gritty, always dry and bloodless; in one of

*Med. Rep. Vol: 3.—Nankeen, like the liver being out of fashion, it may be well to say to the rising generation, that *India* Nankeen was more *yellow* than any they will see now, and it was to that kind he compared it.

all these conditions. It is not difficult to see that, with the color, these states are produced by its secretion, more abundant than usual, being necessarily delayed sometimes in the organ, and while the more liquid parts are absorbed, some portion imbibed; the more solid ones are deposited in the substance of the organ and it becomes dry, bloodless, because this deposit has impeded and totally obstructed the capillary circulation, from which condition of the liver, and a consideration of the origin of the portal circulation, may be deduced another reason; besides the liquidity of blood, for the profuse discharges of that fluid, from the gastro-intestinal mucous surface, and also one for the general hopeless character of a case at that stage.

But to decide this question fully the microscope is required, yet that the liquid parts, and such as would strike the eye, are absorbed to the last drop from the liver, is well shown by Dr. Gillkrest, who by such means as pounding, boiling, &c., failed to obtain bile from it, notwithstanding this fluid was present in the gall bladder in very many cases. This proof of increased power of absorption remaining after that of secretion was lost, is important, because the fact of the increased power is highly so, to elucidate the reason of so little distress in the region of the liver, and of the general suffusion of the system, seldom absent in life, and perhaps never after death, even in cases with scanty, perhaps no secretion after the attack. To the last drop it is absorbed from the liver, even after it has lost its secretory power, but has yet left a remnant of the "excitability," of which Dr. Pascalis has said it "is the first of the viscera to be deprived." *The first to be impressed by the poison, it is the first to be overpowered.*

The post-mortem proofs of functional derangement, and what is considered evidence of absence of organic lesion, being thus sufficiently shown, let us return to the question of *bile* and *no bile*, to see from as many authors as may be at command, or necessary, that there is both *bile* in abundance, or *no bile*, according to the mildness or severity of the attack, and that we have in life evidence, or not, of bile in proportion as it may be excreted, or as, being absorbed in whole or in part, we find it eliminated by the kidneys, skin, mucous surfaces and lungs, and deposited in the tissues; and in death bile or no bile, according to violence of attack, &c.

Of the color of the skin and other tissues it may, merely, be said that it gives the name to the disease, and if not invariable in life, it is perhaps never absent after death. It is admitted to be *general*, *partial* and *recurrent*, of *sudden* and of *slow* access.

An interesting case of this sudden coloring occurred during the late epidemic, showing the manner of its production and also the permanency of the yellow serum after an attack, the person remaining exposed to the influence of the poison. Mrs. V. had suffered from an attack of the fever a month previously; her health was restored, when, not having been well, from exhaustion and distress, but with skin quite free from yellow tinge, perfectly clear, she suddenly fell to the floor in a state of insensibility. I saw her within ten minutes; a light lemon color was diffused over all the exposed parts of her skin, which I was informed was observed instantly after she fell, (instant torpor of the capillaries and imbibition began,) the pulse was full, slow and laboring

with, now and then, a deep inspiration ; she remained insensible for 12 hours and had convulsions ; soon action began in the capillaries of the face, it became red, and the yellow color was observed to appear again and remain for days during her convalescence.

Dr. Rush attributed the color to bile when it is general and lasts for some time, (weeks,) but when transitory or local he thought it wholly owing to a peculiar action of the blood vessels, (capillaries,) and it was by others before, and is still by some, high in the profession, denied to be from bile in the blood ; it is insisted to be from some other condition of that fluid, to the depraved state of the *red* globules or other change of the serum, or from a peculiar *action* of the capillaries, and the fact of its occasional partial character is used as one of the proofs. It is said by Desmoulin that there is "a fluxinary movement" to the skin, an "elaboration" of this color is said by him to be made from the blood, but the serum of the blood must be previously yellow, and it has been shown to be so, even before an attack, before the "fluxinary movement" could take place. It was proven by Dr. Potter, of Baltimore, to be yellow before an attack, and he who had no superior in his day, considered it produced by bile. He daily bled, in very small quantity, a healthy young man just arrived in the city during the prevalence of the yellow fever. The serum was found daily to become more and more yellow, and soon the attack was developed. That the yellow serum is present during the disease is of universal admission and fact, and denied by few to be from bile absorbed.

The process of imbibition and exhibition taking place when action in the capillaries lessens, or ceases when the capillary circulation is impeded, explains the manner in which the color is developed, as Magendie has so well shown. It will not be thought strange that torpor of the cutaneous vessels may be partial, when it cannot be denied that a normal and abnormal action may exist and produce perspiration, healthful or vitiated, yellow or blue. A yellow and a blue perspiration, as well as a yellow skin, has been partial ; the two former from action, the latter from inaction, of the particular parts. Imbibition begins as soon as the capillary circulation is impaired, whether primarily through the state of the solids or fluids, torpor of one or fluidity of the other ; it begins first with the more liquid part, the serum, and the skin is yellow in proportion to the quantity and deepness of color of the bile in it, than with the more solid parts, the red or black, and the reddish brown, the ginger-bread color of some yellow fever skins is made. The skin may continue clear, or become white, chalky, deadly, white during life and soon after death it is yellow, pale yellow, and livid spots appear—the same process of imbibition has been at work.

The black and fluid blood not found in cases of jaundice resulting from other causes than the poison of yellow fever, accounts for the difference in color met with often, not always, in the two diseases. If the color is not orange or green, a person is not said to have jaundice, he is merely bilious ; an effect of many conditions, organic and functional, is sometimes dignified into a general disease and called jaundice, at others not ; the light lemon color of yellow fever is not often seen in jaundice, which may receive its explanation in the peculiarity and greater intensity of the irritation, producing bile, variously colored and in quantity

more or less acrid, according to its degree; and we find the fact to be in the most dangerous cases, in which there is found any secretion at all, that *lemon* is the color either before or after death.

It is admitted by all that the color of skin, eyes, mucous surface of the tongue and mouth, the urine, &c., in jaundice, are solely produced by bile, and it is no more allowed to deny to the liver the capacity to be impressed in different modes, to receive irritations, each varying in character, intensity, and duration, as also in the bland, or acrid or other quantity of the secretion, according to the peculiarity of the "poison" or cause, and its "dose," than to refuse the same to the skin, to the mucous surfaces or other parts.

As we find much similarity between that Asiatic scourge, cholera, and our own disease of the same name, so we perceive much between yellow fever in its milder grades and jaundice, without *organic* lesion, and *combined* with a *fever*; a great *difference*, yet a great *similarity* is found in each, and we cannot refuse the application of recognized principles and facts to this disease.

Baillie and others have shown that in jaundice, "there is often a *deficiency* in the quantity of bile, without any disease in the structure of the liver. Sometimes the bile is of a dark color, nearly as black as ink, and the liver quite sound in its structure." It is more frequently mere functional than organic disease. The secretion usually abundant, and, though generally, is not always, wholly absorbed; its excretion is full in some cases, in others it is totally obstructed and no organic disease, no tumor found to account for it. In both diseases, the ducts are often found shut *in life*; without organic cause; in one *always*, and in the other occasionally, open *in death*.

After yellow fever the orange yellow skin, not to call it the jaundice skin, continues occasionally for a long time, even where calomel had not been used in the treatment.

During the attack, the yellow color begins in the same parts in the two diseases, and other points of similarity are found in the slowness of pulse and in the itching sensitive skin, in the constipation of the bowels, and the extrication of gas, the character of the stools often alike for awhile; the same of the urine during a longer time, the absence of pain in the liver when unattended with inflammation or organic disease, the pains in the eye-balls, in the knees, (Charles Bell speaks of "pains in the knees preceding a *fit* of the bile,") and in the calves of the legs, the latter often attracting all attention to such a degree from its intensity and absence of other feelings of disease, that a patient having "jaundice with a fever," in the Natchez Hospital, in 1846, begged me to "cure that pain, nothing else was the matter with him," and the nearly similar starts of mind are to be considered.

Horackzeck's treatise on yellow atrophy of the liver, and Budd's work on this organ, afford valuable and incontestible proof of the intimate relation of disorder of the liver, as cause in the production of many of the symptoms and conditions of yellow fever.

In disorder of the liver, I have seen two cases of painful irritation and purulent urethral discharge, exactly similar to that found in yellow fever, solely attributable to this cause, as the skin, the eyes and the urine were yellow, and the stools pale, and as one case was a child

three years old and the other an irreproachable man ; this irritation of the urethra must be attributed to the acrimony imparted by the acrid bile ; they were soon relieved by a few portions of calomel and by iodide potassa. I have known many yellow fever patients only begin to complain of this pain when the urine showed the elimination had just began.

The perspiration has been observed to be yellow, partial and general, preceding, during, and succeeding an attack, and the mucous from the nostrils and lungs, and the saliva, have also been yellow, as remarked by Rush, Drysdale, Pringle and others ; and Dr. Rush gave the name of *Bilious* to the disease, so struck was he with proofs of functional derangement of the liver, and observing these for weeks before an attack.

He recorded at p. 60, and seq., Med. Enq., vol. 2, the following : "In some cases there was a constipation of the liver, if I may be allowed that expression, or a total obstruction of secretion and excretion, but more frequently a preternatural secretion or excretion took place.

"On the first and second days of the disease, many patients puked from half-a-pint to nearly a quart of green and yellow bile, and the quantity of stools produced by a single purge was in many cases very great, they could be accounted for only by calling in the constant and rapid formation of them by the preternatural effusions of bile into the bowels."

"I attended one person and heard of two others in whom the stools were as white as in jaundice. I suspected in these cases the liver to be so constipated or paralyzed by the disease as to be unable to secrete or excrete bile to color the fæces." And Dr. Coxe attended a child, 17 months old, who had white stools for several days, "which turned black before it died." It was a happy circumstance when morbid bilious matter came away in the beginning of the disease, and upon this point of bilious vomiting or purgings early or late, all writers in all parts of the world agree that they are favorable ; favorable to certainty of recovery when occurring late. It is evident that Dr. Rush's observations prove bile to exist in the mild, and none in the violent grades or dangerous stages.

He says "a total deficiency of urine took place in many people for a day or two"; "it generally accompanied or portended great danger"; "a young man discharged several quarts of *limpid* urine just before he died" but the urine was in some plentiful, of a high color—at times clear, at others turbid—at 4th or 5th day assuming a dark color, it resembled strong coffee and continued of this color, in one case, for several days, after recovery ;" p. 41 "we observed the effects of the same cause (concentration of the disease on the vital parts) in a *natural* state of the skin and in a *natural* appearance of the urine in the *most malignant* forms of the fever".

Dr. Drysdale says in the Yellow Fever of Balt. of 1794, Med. Museum, Vol. 1. „The urine was very *high colored* and *yellow* except in two cases, in which it was *limpid*, it deposited a *copious sediment* even in the exacerbations of fever ; in many instances there was a suppression of urine from the first hour of the disease to its generally fatal close."

Dr. Frost found the urine deep *red* and *small in quantity* in the *ma-*

lignant fever at *Guiana*, and the urinary bladder contracted to a very small size.

No bile in the most malignant, and bile was abundant in the milder as exhibited by the urine and evidently dependant upon grades, which affords a full explanation of the following accounts apparently so discrepant.

After stating that his experience did not bear out the statement of others, as to the *bilious* appearances of the matters vomited, Dr. Gillkrest says "bile is also usually absent on an inspection of the stools and urine" while others of West India experience say the latter was *saffron* colored and the former, the stools, under favorable circumstances, were bilious, and the Seville physicians in 1819 found the "urine yellow and paper dipped into it was tinged the same color." Dr. Lewis, in the *N. O. Jour.*, Sep. '47., says "the urine in Jaundice is colored by bile, but it is not so in Yellow Fever. To be sure Jaundice sometimes supervenes in Yellow Fever, but this is rare, and when such is the case the urine is tinged."

These statements are only apparently discrepant. It is certain that they are solely attributable to the observations being made in cases of greater or less intensity, or at different stages in which bile was found excreted or not, eliminated or not, according to these circumstances and which some of the authors state and others do not.

In the epidemic at Woodville in 1844, the secretion was profuse beyond all example in my observation of fevers during 18 years, this was evidenced by vomiting, by the alvine discharges and by the urine. In the favorable conditions the two latter continued full and free—in those becoming grave the excretion failed, but still as recovery was promised, the urine was deeply charged with bile, and recovery took place alone in one case (Soher's) by this elimination, and in many by this and an excretion induced by calomel at late periods.

As death approached, this elimination failed as well as the excretion and either pale, bileless stool or black bloody ones were observed.

These points have been shown in the most conclusive manner in the late epidemic at Natchez. It was called by various names, but all agreed in the fact of profuse secretion of bile; and yet no one ever saw this secretion excreted or eliminated while death was portending—the only instance of elimination by the kidneys at this period that I saw was the 3 ss quantities in Mr. V.'s case.

These were the only two facts upon which the people and the physicians agreed without cavil and one physician, an old yellow fever veteran, witnessing the *intense* grades during twenty five years, reported a death from the prevailing *bilious* fever, which assumed a malignant type from previous derangement of the liver.

Of the Opelousas epidemic Dr. Cooke says (*N. O. Jour.*, July 1846) "An arrest of the secretion of bile was the only never failing (symptom)" and that "the secretion of bile was a certain sign of returning health. In the fatal cases it never reappeared; hence the most painful anxiety was experienced until the presence of bile in the evacuations showed the restoration of the hepatic function." Most correct observation, and it is apparent he means that the *excretion* of bile did not take place, and was the only never failing symptom from the onset of the attack to the

termination, which was fatal if no bile appeared. His general description of these epidemics shows them to have been of high grade—and of this grade, in all countries and at all times it has been the same.

Of Charleston Yellow Fever Dr. Wardeman, Amer. Jour., Jan. '45, remarks that "the most pathognomonic symptom is a total cessation, or the much diminished and vitiated secretion of bile;" also if ejected at the beginning he considered it to have been secreted before the attack and that "the restoration of the function of the liver is the *most favorable* symptom etc."

Dr. Wardeman plainly is speaking of the severe form as he throws out ten mild cases in his calculation, and three-fourths of his patients were addicted to the *free* use of ardent spirits.

Dr. Nott observes that "another *striking peculiarity* of Yellow Fever too is the entire absence of bilious vomiting *after* the paroxysm of fever has passed, if perchance you see a blue, green or yellow tinge in the clear fluid vomited, you may hail it as the harbinger of *safety*; the prognosis is almost certain". Dr. Nott again says "the secretion (the excretion?) of bile in this disease is almost invariably suppressed early, in severe cases it is *rarely* vomited after the second day and I believe I have never seen it after the third day, *when they were fatal*, except in one or two protracted cases." The latter observation was published in 1845 in Philadelphia, and the former in 1848 in New Orleans, and although of similar tenor, they attract less attention, being separated, but connected they forcibly teach, if not the foundation, at least, certainly the object always to be had in view "that the liver is the organ by and through which the disease can be most successfully (surely) overcome." No doubt Dr. Lewis means in such cases and in similar stages as Nott describes, when he says that "a man in Yellow Fever either vomited or purged bile would not be believed by a creole or nurse in Mobile" and that this is Dr. Lewis's true meaning is also apparent from other parts of his own writings, and it is important to remark that nearly all his descriptions, good as they are, are of "grave and malignant Yellow Fever" and this is always true when he speaks of the *bile* and *urine*. At p. 297, New Orl. Jour., Jan., '45 he says in speaking of some cases of long febrile paroxysms that they were attended among other symptoms with occasional bilious vomitings", at p. 299 "If bile is ejected, it may be set down, not only as an exception but a *favorable* indication" and at p. 423, March Number, "The evacuations from this cathartic (mercurial in the first stage) are usually bilious, but as the disease progresses, (to death, or unfavourably of course,) the stools become gummy and inodorous" and it is this kind of stool, or ashy or dove colored—all showing absence of bile at late periods, that he solely refers to. At p. 159, Sept. No. 47, he says "If bile is vomited in the second stage (that of calm) it constitutes good reasons for *questioning the character* of the disease." Nothing could be said more conclusive, than this from one so versed in the peculiar signs of the disease, to indicate the predominating importance of the liver. It cannot be said of the bile in any other disease.

With all these evidences from himself and others, and the total absence of bile in the stomach and intestines *after* death, soon to be referred to—it is with surprise I find Dr. Lewis, in speaking of the

case of Dr. Fletcher, at p. 426, say: "This was the *only* instance which has come under my observation where *green bile* was thrown up with black vomit, a table spoon-full of each was ejected at intervals of 4 or 5 hours." This case has such important bearings—the fact of green bile and black vomit formed together—the only one to be found on record—that I hope to be indulged in suggesting that as some of the blood *from the gums* most probably entered the stomach, and meeting with the acids, was ejected, of an appearance impossible to be distinguished from genuine black vomit, or that made from *blood exuded from the stomach*, and was followed by *green bile* in the vomitings. I believe Dr. L. endorses Dr. Nott's conclusion that "no one can tell the *artificial* from the genuine black vomit." I make the above suggestion with the more hope of its correctness as Dr. Nott, in the American Journal, p. 281, says that the aqueous portion (of genuine black vomit) thus filtered, differed in color * * *; in *one* of a light *green* color like dilute bile with an acid added, &c." This was the only one of this color and these experiments were made with the assistance of Doctor Lewis—and it must be this case that Dr. N. again refers to at p. 284, thus: A moderate quantity of bile may exist in the black vomit without being *perceived*; this I proved by adding bile to the artificial black vomit, and by filtering the genuine black vomit, the aqueous part of which in *one* case was *green*, and this color I *presume*, was attributable to a small admixture of bile."

The case of Dr. Fletcher, with this and the preceding one, seems to have been the same, and the *appearance* of bile more justly attributable to some *complication*, or to be viewed as an exception *so rare* as not to disprove the rule of absence of bile with black vomit, even had he died of Yellow Fever as he did not, but recovering after six days with black vomit and discharges of blood from the bowels and urinary bladder, died ten days subsequently of Cholera Morbus. And with Dr. Lewis I should rather question the *character* of the disease,—at all events, it is a case of much interest and shows the skill of its management.

Dr. Harrison* says the stools are sometimes "exceedingly large and offensive; at others watery and slightly tinged with coloring matter, then of a clayey, *ash colored* hue." "Sometimes they seem to be made up of water and bile, which (the bile) in certain cases is secreted in *immense* quantity." "At others, they are composed of dark, tarry, matter," and that these two latter kind are not "caused by the action of calomel" and that "the passage of *bilious* stools is as *common* an occurrence as we meet with, though not a grain of any mercurial had been taken."†

Dr. Lewis gives the case of a clergyman, who after a 24 hours fever, "continued for 5 days without fever, pain, nausea, or thirst, still growing weaker, more restless and anxious, with *scanty* and *colorless* secretion of urine, and thick dove colored putty-like discharges, which were forced away by by enemas," and this case affords a striking illustration of the absence of bile—no one can doubt that this was the foundation of his dangerous condition, and if the secretion had not soon come on, black vomit would.

Dr. Harrison observes that the gall-bladder in most cases contains

* Sept. No. 45, p. 136.

† Nov. 18, 18, 45, p. 324.

its usual quantity of bile, which is to all appearance healthy—sometimes it is *greatly inspissated*; in others the bile is more mixed with mucous than usual. I have sometimes found the gall-bladder containing only a little *glairy mucous*; these cases are rare. The mucous coat of the organ is sometimes, like other mucous tissues, injected or spotted with *blood, &c.*”—more valuable proof of the two opposite conditions and the intermediate grades, could not be found. Dr. Gillkrest gives the same. He says “the gall-bladder containing bile *highly inspissated*, and sometimes dark tar-like appearance (blood?) or *altogether empty*.” These observers agree perfectly on this point as they did on the state of the liver.

And Dr. Nott says of the gall-bladder—“This was found in all the sixteen cases except one, to contain bile varying in quantity from $\frac{3}{4}$ ss to $\frac{3}{4}$ iv; color from pale green to olive and even black; consistence from water to tar—in one case the cyst contained about $\frac{3}{4}$ iv. of colorless fluid resembling gum water or *mucus*.†

Now, Dr. Frost's account of the contents of the gall-bladder will show them in the *most violent* grade. “The gall-bladder was found *empty in some* and in a *few* contained a dark green, ropy bile, but *generally* was distended with black thick matter, resembling molasses or tar” (blood?)

It is evident that the secretion is more, in proportion as the case is less severe, and less as the attack is more and more intense—that much bile must have been in the gall-bladder, in some of the cases, and its liquid parts absorbed.

Death may accidentally and suddenly take place, and then bile may be found in the stomach and bowels, but, otherwise, in the due progress of the disease—*never*, as the preceding facts in life, and the following post mortem examinations make sure.

Lewis, after carefully examining 23 cases at Gibraltar, says, “very little of it (bile) was found in the stomach and intestines.” Very little, indeed, when in only one case (18) there was in the stomach “8 oz. of a yellowish liquid, which appeared to be nothing else than a part of the *drinks* taken by the patient” and the “small intestines contained a yellowish liquid mixed with mucus” and in another, the stomach contained a liquid of the same color, depositing on standing a little black matter.”

The fluid in all the others was red, and black in 18, and grey or mucous, etc. in others.

It is useless to refer by name to the various authors who have failed to notice the presence of bile after death by Yellow Fever—after death, in the regular process of the disease, it is impossible—if it were to be found it would have been noticed full often—such a fact of such moment to show the liver and its secretion had *no concern* in the disease, would not have escaped observation, and its universal absence in the stomach and bowels, while so frequently present in full quantity in the gall-bladder with the *gall-ducts, open*, deserves especial notice. This is observed in no other fever, not even in *bilious* fever of which patients die, or formerly died, while discharging bile, bile, black and of all colors,

* Sept. '45, p. 138. † P. 279, Am. Jour. Ap. '45.

and the bowels in their upper portion containing a full quantity of *yellow bile*. In Yellow Fever this never happens except death takes place *suddenly* from accident.

Bile and black vomit then are never found together *in life*, or *in death* from Yellow Fever, and bile absent, and because of the absence of bile, in the stomach and duodenum we find acids in abundance; and the accumulation of acids is shown by Prout to depend upon the deficiency—the absence of bile, and, thus, these two facts tend to prove and explain each other.

Having seen that bile is often present in full quantity in the gall-bladder and never vomited or purged in cases towards the period of death, and never found in the stomach and bowels after that event—yet we find the *gall ducts are invariably open after death*.

These facts constitute another discrepancy or contradiction more apparent than real—a contraction from excitation of the gall duct, resisting or not, the profuse secretion of bile, or the most powerful efforts of vomiting—then, a paralysis temporary or permanent, seems exactly to account for the facts observed—the *ducts are open, yet the bile does not flow*.—Let the œsophagus be paralysed, food may be forced down mechanically, but will not pass otherwise—the intestines in the same state—its peristaltic power gone, the *fœces* do not pass—yet in these cases the tubes are *open in death*, while it is no solecism to say they are *closed in life*. It may not be out of place to suggest that this condition of both these tubes are often found in fatal cases of Yellow Fever.—The former shown by the pumping mode of vomiting, an *inverted pharyngeal* movement, the œsophagus and stomach being evidently paralyzed,—a vomiting by the reversed power of the Pharynx alone, and the latter indicated by the intusceptions, is often remarked.

And besides the catarrhal, the croupy condition, the destruction of the mucous membrane of so much of the esophagus—the turbid urine, the *bileless stools*, *all so common* in Yellow Fever, and some, so rare in other fevers, indeed never met with, make the following observation of facts in “Croup-like convulsions” by Marshall Hall well worthy attention:—“Amongst other symptoms are frequently observed *bileless stools* and *morbid deposits* in the *urine*.—That the excited nerves may transfer their influence in dentition, in gastric crudities, in constipation, to the true spinal marrow, and thence to the branches of the pneumogastric nerves, may be regarded as proved.—Now this nerve sends a branch precisely to the *liver* and to the *kidneys*.

Throug the medium of these branches the secretion, or the *excretion*, of the bile and urine may be impeded and the events confirm the theory. *

The gall ducts possessing a coat so nearly if not quite muscular—as near true muscular tissue perhaps as the iris—is admitted to have the power of contraction which, indeed, it has been *seen*, to possess by irritation of the nerve, and being muscular and contractile, propels the bile by a peristaltic movement, invited, physiologically, by the presence of food—but when in a pathological condition—in contraction

* Lectures on the Nervous System.

—may, or may not, yield to any force from fulness of secretion, or efforts of vomiting; and when, like that of the esophagus, stomach and intestines, this peristaltic movement fails under a temporary or permanent paralysis, the occlusion is as complete as if a tumor closed the channel.—It is reasonable to infer this paralysis—it is irresistible—which, it is plain, is the condition of the stomach and esophagus at late periods of fatal cases, and the tendency and *approch* to which, the deficient excitability of the lungs and heart, as shown in the *slow* respiration and the *slow* pulse, and which seems to be at the foundation of much of the character of the disease.—This, it may be, temporary paralysis of the gall duct, coming on at the period of calm, was *well* exemplified in the case of Nelson Newman in which exactly at that period, the *excretion* of bile ceased, for the stools *then* lost the straw color; while the *urine* and *skin* proved the continuance, and soon the *increase*, of the biliary *secretion*.

Few, if any of the grand points of Yellow Fever can be fully understood without reference to the influence of the pneumogastric nerve, presiding as it does over the first *formation* and *healthful preservation of the blood*—from the pharynx to stomach, liver, kidneys, and from larynx to lungs and heart.

Surely the most important of all the secretions, is that of the liver—essentially *curative* of the morbid impression on the organ and absolutely necessary for its removal, although much of the distress and severity of the fever, is due to its *vitiating* quality, yet the mere *amount* of bile absorbed is no source of danger, provided it has a ready exit from the system through the emunctories; the danger, on the contrary, results from the *intensity* of the nervous excitement of the organ producing *poisonous* secretion, or not relieved by a full secretion and excretion, and the system saved by elimination, or it results, rarely indeed, from a total abolishment of secretory power.—This secretion in its fullness is curative also of the *disease*.

But besides all this, in Yellow Fever, the poison perhaps makes an impression also on the *lungs*, giving some other of the peculiarities,—blackness, fluidity, or other abnormal quality to the blood and consequently a vast amount of the danger of the disease—of the mode by which this change takes place we may as yet know nothing, but the fact that the lungs are impaired in their function, is shown by the sense of oppression; the peculiar *slow* respiration, the sighing and the quality of the blood, one or all so sure to be seen in the dangerous forms or stages, yet not to be detected by the senses in the earlier stages of many grades or throughout the mild.

The post mortem proof of this impairment was shown by Lewis, was noted by Dr. Frost at Demerara nearly 30 years before, and by Dr. Mitchell in the Yellow Fever in Virginia in 1841, 42, and has been by many others.

This condition of the lungs is *not* a product *after* death, and, as described by Lewis, is exactly similar to that resulting from the experiments of Dr. Reid, a short account of which, touching this point, may be found in Carpenter's Physiology, p.170, and Magendie's observation of the *same* effect from a like experiment, shows the manner in which the lesion, proof of *functional*, not *organic*, impairment commences,

the section of the nerve "seriously affecting the capillary circulation", and this causing the blackness and fluidity of the blood.

A blackness and fluidity—a deterioration exists, and it matters less *how* induced—if through the lungs than to understand its influence on the capillaries of the lungs, and on those of the gastro-duodenal surfaces, in the production of what has been called the "localization" of the disease—the *inflammation*—not losing sight of similar stagnations and exudations of blood in and from the capillaries and nearly all parts of the body, and to understand the *action* and *reaction* of the fluids and solids on each other, especially in the lungs and the mucous surface of the stomach and bowels.

The observations made by Magendie, so admirably explain these phenomena, and also many other of the post mortem appearances in Yellow Fever, that I must transcribe from his fifth lecture on the blood in the London Lancet Vol. 1. "38, "39.

"I will not for the present examine into the cause of its fluidity ; but the case gives us an opportunity of verifying in the human subject the mode in which such blood effects the condition of the organs.—I have explained to you at length the serious consequences of non-coagulability—I have told you that, when so affected, the blood cannot traverse the capillary vessels, or at least that its circulation through them is materially modified ; that it stagnates in the parenchyma of the organs, is extravasated, alters their appearance and sometimes renders them utterly unfit for the performance of their functions.

For my part whenever I meet with blood of this description, I feel certain that the lungs have been the seat either of engorgement, hepatization, œdema, or apoplexy : and if its fluidity be still greater I feel almost positive that there has been effusion within the cavity of the pleura."

"The blood (become fluid) lost one or more of its normal properties, and the lungs, *in its turn*, suffered from the modified character of the blood.—Stagnation and infiltration took place, etc." "The phenomenon of exhibition will account for a number of lesions in the other organs that are beyond all question, primarely produced by the liquefaction of the blood.—In my opinion, various morbid changes pointed out by pathologists as occuring in analogous cases, such as dark color of the intestines, erosions of the mucous membranes, effusions, sanguinolent diarrhœa, etc., are in ultimate analysis, traceable to the same cause."—

To call that a "localization" which, in this disease, is formed from nostril to rectum, in the mucous tissue of all the parts—in the cellular substance, and in the skin, is to lead one-self into a great error.—But, to show the strong tendency to, and to call it inflammation when seated in the gastro-duodenal mucous tissue, will not be an error and will lead to a necessary avoidance of irritants, in anticipation of its approach, and to a correct mode of treatment after its formation. Yet it differs materially from an inflammation of these parts in fevers or other disorders unattended with the fluid and black blood of this, and requires a corresponding modification of treatment—remedies for the blood as well as for the solids, and those for the former through the

lungs as well as through the stomach as recommended by Magendie and approved by common sense.

It is certain that a *nervous*, a *mere* nervous excitation, precedes the *sanguineous* irritation of the mucous tissue of the alimentary canal, and that it may be, and, in mild cases, is the only irritation, and that it is capable of *vitiating*, *increasing*, or *abolishing* the secretions, is fully seen in the catarrhal and other mild forms and in the most intense; and resulting from the disordered condition of the liver, also solely nervous, this nervous irritation often continues the sole derangement of the stomach and duodenum. Throughout, in many cases, those in which we find the blood to have suffered so little, the liver so well to go through its morbid process, fulfilling that law of the disease by a prompt and full secretion, and in which an excretion, an absorption and an elimination so rapidly takes place as I have so often witnessed.

But this nervous excitation gives the strong tendency to, and might well, alone, suffice to induce the sanguineous irritation which becomes so conspicuous in that mucous tissue, full of capillaries; yet on the other hand, the fluidity causes the peculiar character of the inflammation in this disease and, in its turn, this state of the blood is alone sufficient to produce it.

After observing Yellow Fever a second time in its mild form, mingled with cases of almost all grades, and better prepared to investigate, I repeat now the observations made after witnessing the disease in 1844, proceed from mild to severe grades; that "*nervous* irritations of certain organs (of the secretory vessels of the liver, and of the mucous tissue of the alimentary canal) and the alteration of the blood, are alone invariable and sufficient to constitute the disease. Although *sanguineous* irritation does supervene on the gastro-duodenal surface in a great majority of cases, yet it is not essential to constitute Yellow Fever; mild cases in which no appearance of inflammatory irritation is observed to attest this." "Remittent, intermittent or congestive fevers are not curable by maintaining a secretory action in one or all the organs. Yellow Fever ("the only true *bilious* fever") on the contrary is *always* tending to a cure as long as the liver can be induced to act well (that is to *secrete* and *excrete* copiously); and failing to *excrete*, the case is still tending to cure and *will* end in health, by the termination of the diseased process in the liver, through *secretion* and *absorption*, provided other secretory, *eliminating* organs, save the system from the effects of this absorption, affording time for art to subdue, what nature is incompetent to cure.—The gastro-duodenal inflammation, should that supervene, as it did in Soher's case, and in which "during 7 days there was no discharge from the bowels; no bile was thrown into them, but it is not so clear that it was not secreted in great quantities, absorbed into the blood, taken up and expelled by the kidneys, exemplifying that "his case could *not* have been brought to a happy issue but for the performance of this office (eliminating bile) by the kidneys."

This irritation of the liver has a time, *as a law* imparted, we must assume by the *peculiarity* of the poison, within which *to run its course*, yet not as determinate in this as it is distinct in its nature and in its product, and yet so much so as are the irritations of the skin in Measles, small-pox, vaccination &c., and in *their products*. It may vary in individuals,

or in epidemics, but its general uniformity, whether in cases mild or severe and whether recovery or death takes place, stamps this as a law, which all recognise and apply to the fever and the disease; in some it may not cease entirely; its violence may be only expended within a specific time, dependant, among other causes, upon the failure of a proper establishment of the process of cure, secretion and its exit from the liver and from the system, but *these secure* the liver only requires time, *will* have its allotted time to be relieved, and the system time for elimination when the disease, fever and all, has disappeared, *not to return*.

The affection of the liver is a painless one, and, at its commencement, cannot impede the circulation through the organ, on the contrary, the irritation being in the secretory vessels, requires and invites a greater flow of blood, yet it is not *inflammatory*, and the *one* febrile paroxysm, alone belonging to the disease, may rather be attributed mainly to a *sudden absorption* of bile, vitiated, and acrid in various degrees, and to its difficulty or total failure of elimination; a full open fever is favourable to recovery, so is full secretion of bile, less regardful of the quantity absorbed, and, even of its acrimony, than of its facility of elimination.

In the milder forms, those with open fever, the sanguiferous system evinces rather a *tumultuous* effort to rid itself of some absorbed morbid material and it only moderates gradually into *healthful* action with full elimination, and more promptly with excretion of bile, and *only* with these, the bloated face, the swollen hands, the eruptions, the subsequent boils and abscesses, tend to the establishment of this as truth.

The power of absorption by the liver is admitted to be great, and reasonably may be supposed more rapid if the secretion is less vitiated and more difficult, if more acrid—a provision for its safety, in view of the so common fact of the occlusion of the gall ducts, and the necessity for the exit—a process early at work as shown by the experiment of Dr. Potter, proven to continue to a late period by the failure of Dr. Gillkrest to procure a vestige of bile from the yellow liver, and shown to be of great degree by its continuance, even when large quantities are *excreted*—and in proportion to the quantity absorbed, of less or more vitiated, and *acrid* bile, will the fever be mild, or more intense, with danger also proportioned to its elimination.

The reference of the febrile paroxysm mainly to the *sudden absorption* of bile of *morbid* quality, and the continuance of each through the time required for the relief of the hepatic irritation, aids to show why uncomplicated Yellow Fever has only *one* fever, and *cannot* be cut short—will run its course, the latter fact, so important, so true and so much insisted upon by Harrison, Nott and Lewis, and it explains why to *cut short* the *fever* is to fail to cure the *disease*, as Dr. Harrison has so ably and so conclusively proven by his two Quinine cases recorded at page 332, N. O. Jour., Nov., 1845.

If anything in medicine ought to be considered surely established, it is, that the *fever* in Yellow Fever is not symptomatic of, or necessarily connected with, a local *inflammatory* affection. When any such exists, it is not till long after the fever has appeared, and sometimes the received evidences of it are present, for the *first* time, after the fever has disappeared, *never to return*.

A remarkable characteristic of the disease is that the heart so little sympathises with the local irritations, and less in proportion to the *intensity* of attack, and so certainly, not at all, at advanced periods of these, that the greatest errors of treatment by medicine, were excitant to the stomach, never cause the fever to increase or return, but rather to cease more quickly when used at earlier periods, and the greatest efforts to sustain the system by *such* means during and after the calm, not only fail; but hasten death.

These facts of such general notice in this, pertain to no other febrile disease, and they are inexplicable on any other view of the *cause* of the fever than the one suggested.

The correctness of this view of the cause of the *fever* is rendered more apparent from the fact that there is no fever—no eruptions, no boils, &c., in the most intense grades, in the walking cases, the so called *congestive* ones. In such cases, Dr. Lewis correctly says there has been an over dose of the poison—that they were usually without “any chill, fever or pain; the tongue was natural or very slightly coated—some thirst, pulse slow and wavering,” and “in proportion as the dose is increased” it produces such cases, and not those “active phenomena which distinguish inflammations and essential fevers.”

Dr. Harrison's description of the same class of “generally incurable” cases makes this more clear. He says, page 133, Sept., 1835, “no fever ever makes its appearance;” the patient in this form of the disease presents a *natural* eye, tongue and pulse—“the pulse is slow,” p. 146. The skin is also *natural*, except that towards the extremities it becomes cold, &c.” “The stools (surely without bile) are liquid, and the urine *copious* and *limpid*.”

The calm is virtually present from the beginning—no bile secreted and no relief to the liver and no fever—then *death is sure*; whereas, with bile absorbed, there is fever, and “a case of Yellow Fever promises to terminate well in proportion to the (full, open, “well”) development of the fever”—Harrison, page 331, Nov., 1845, N. O. Journal,—and with excretion and elimination of bile, *life is certain*.

At page 146, Sept. No., 1845, he proceeds with these cases again, thus: “The peculiar disease goes on, however, though fever be absent, as indicated by the blood, its want of fibrine, the occurrence of passive hemorrhages, black vomit, suppression of urine; in short, all the characteristics of the disease;”—at page 133 “these cases are characterized by extreme restlessness from the beginning,” which, with the liquid (bileless?) stools, *copious* and *limpid* urine, *clear* skins, surely point to a suspended or diminished and acrid biliary secretion, or to its deficiency, or total failure of absorption for their explanation; and, could there be seen to appear, at “the end of the second or third day” either bile “vomited or purged,” or by copious “*deep* yellow urine, the black vomit, &c. would *not* occur”—the prognosis (of recovery) would be almost certain, as said by Nott and more fully by others.

With full secretion and absorption of the bile, the patient has fever, and with excretion or full elimination never goes into the calm; the excitability of the organs is not destroyed; the progress of the case is then surely, though gradually, to health; but these processes defective or abolished, early or late in the attack, and he has no fever; the calm

is invariably at hand; the organ is wholly or nearly entirely deprived of its excitability.

Although it may be considered absurd to attribute any of the deterioration of the blood, as any of the symptoms of the disease, to the absorption of bile; though allowed to be so acrid as to cause excoriations of various solid parts; absurd, as making this frightful and obscure disease of too simple a nature; yet impelled by observations, and the facts from so many correct observers, as well as from general principles and experimental facts, I believe that in some peculiar and extreme vitiation of this secretion may be, will be, found much of that change in the blood which at present is attributed to some occult cause, acting in an imaginary manner. Why not a few drops or more of bile, acrid bile, as well as of other secretions or fluids, be a poison careering through the blood, producing more and more a gradual deterioration, as in long periods of incubation, or progress of the disease, or causing death rapidly with black vomit, producing liquid, blood in each, according to its quantity or own degree of vitiation?

Magendie made black vomit by injecting putrid water into the veins. Would he not have succeeded in the same by acrid bile taken from the gall-bladder of a person dead of yellow fever? Fresh water, neither bland bile would create danger, but let each be putrid or acrid! Thus, without reference to an unseen, an imaginary poison travelling through the system, we might (I believe we do) see the blood of this *bilious blood pestilence* created; but I fear created in a manner too simple to be credited, and not the less a bilious disease, because, in mortal attacks, there is no secretion perceptible to our senses; yet scarlet fever terminates rapidly in death without exanthem, sore throat, or fever. Cholera has its premonitories, its rice-water discharges, and shriveled, blue skin, and yet death without either; and each during epidemics are not the less considered the same disease on account of these deficiencies from excessive violence.

Let principles and facts recognized as indubitable in other diseases, be applied to this; look at ischuria, sometimes a functional disease—"There is frequently considerable disease of the kidneys. The suppression may be partial or complete; it leads to fever, thirst, a urinous taste in the mouth and smell of the perspiration, nausea, [urine from skin and hands,*] vomiting, [of urine,†] hiccough, coma, convulsions"† and death.

From absorption of urine these, (why not?) those of yellow fever from *acrid* bile. Compare other characters, showing each a disease from a known and palpable poison—morbid urine and morbid bile—and note the also similar efforts of nature to relieve, &c. We are obliged to assume the influence of a deleterious agent, a "poison," in the production of many diseases; but let us drop the assumption as we would a simile, the moment we can recur to more seeming matter of fact, and the more we simplify, the nearer we approach truth, for nature works by few laws.

It is beyond dispute that the cases of Soher, Nelson Newman and Long, and the facts recorded of the Woodville and Natchez epidemics,

* Elliotson. † Marshall Hall.

with those from so many authors, prove the "safety," the recovery from bile secreted, excreted or eliminated, and that the same reference, as conclusively establish the "fatality" from no bile, secreted, excreted or eliminated; and the same reference brings ready proof that, "although the liver is charged with a multitude of sins; of many of which it is entirely innocent, it is lost sight of, despite the overwhelming evidence of its profound impairment, in yellow fever, which, could the title be exclusively appropriated to it, should be called the *bilious fever*, and thus the liver justly held responsible for, at least, one fever or (*and*) entitled to the credit of being the organ by and through which it can be most successfully overcome."

The preceding facts, taken from a few of our best authorities, have been hastily thrown together, yet with a reliance that the deductions from them are correct. And the hope is indulged that this attempt, unsatisfactory as it may be, will lead to a mode of investigating the disease different from that heretofore adopted, and about which as yet there is so much contradiction.

Natchez, December 20, 1848.

II.—*The Treatment of Chronic Bowel Complaint.* By WM. A. BOOTH, M. D. of Louisiana.

Treatises on "Bowel Complaint" superabound. Every author of a book on Practice issues one upon its two forms, Diarrhœa and Dysentery. These treatises contain such laborious and minute descriptions of the disease with its complications and alternations, as almost to persuade the reader that the writer thought them original. The accuracy and accessibility of these descriptions preclude the propriety of an attempt on our part to present another to the profession. Such an attempt would, but be to repeat a repetition the thousandth time. The necessity for such a waste of ink and paper is also precluded by the fact that any tyro in medicine can diagnose bowel-complaint.

These remarks will not however, apply to the treatment of the Chronic form of this affection. We know of no satisfactory treatise on this part of the subject. The grand remedies are not mentioned; or they are recommended in inefficient doses; or as *dernier resorts*. This disease, as it exists in the lower portion of the Mississippi valley, is well calculated to instruct the observant practitioner. Here it prevails extensively, so extensively that it may be considered *the* disease of the country. A Physician in full practise is never without a case of it. It is a dangerous disease, but, with *timely* and judicious management, is almost invariably curable. It is not so mild as in more northern climes, nor so rapidly and hopelessly fatal as within the Tropics.

The extent to which Chronic bowel complaint prevails in this section, and the middle course it pursues, give to the Physician an unusually fair chance for judicious experiment.

The opportunities thus afforded have been embraced by many in New Orleans and its vicinity, but owing to the silence which has been

maintained, each one has had to learn for himself. At least, this was the case with ourself.

To elucidate, let us glance at a few of the standard authors and criticise their treatment. Johnson's total ignorance on this subject is exhibited by the following remark. "At length the system absolutely refuses all impregnation from mercury, and we have the mortification to see our patient waste away and *die* for want of the only remedy that possibly could arrest the hand of death ("change of climate.") Salivation and change of climate the only remedies!—We are disposed to laugh, but we forbear. Dr. Johnson was a great, a good and a sincere man, and is in solemn earnest on this occasion, for he adds, "And how can it be otherwise upon the principle which I have stated?" Watson recommends diet, counter-irritation and opium. This is a fine prescription so far as it goes. *Diet is absolutely necessary*. Counter-irritation is a great *adjuvant*. Opium is indispensable in almost every case. But these remedies, singly or in combination, will not effect a cure in one out of every ten *obstinate* cases. They simply give nature a fair chance to restore the diseased membrane—neither possesses any *directly curative* power. And it is as absurd to stop the treatment at this point as it would be that of Chronic inflammation of the throat, fauces, urethra, or skin, unless no (other) remedy exists, possessing such power over the diseased bowels. Stokes recommends the all important remedies with some earnestness, but one would not, from the manner of his recommendation, resort to them sufficiently early, give them in sufficient doses or continue them sufficiently long. He would not, in short, give them a proper position in the treatment of the case. Eberle lays more stress upon them than Stokes, but we think the same remarks would apply to his essay. We pass over the essays of Dunglison, Brown, Copland, Cheyne and even Dr. Woods', our admired and favorite preceptor, which are by no means satisfactory, and select those of Dr. Dickson, formerly of Charleston, for more minute criticism. This course will enable us to give our own views more explicitly. As the Doctor resided only a degree and a half north of this, we turned to his treatises on this subject with eagerness; but were greatly disappointed. So far from the treatment recommended by him being superior, it is decidedly inferior to that of Eberle or Stokes. The Doctor, following in the footsteps of all his predecessors, treats the Diarrhœa and Dysentery in both their forms as two distinct diseases. This is an unnecessary distinction and leads to error in practice. The pathology of these diseases is precisely the same. Both consist in inflammation of the mucous membrane of the bowels, and require treatment based upon the same principle; and are curable by the same remedies, provided the disease does not implicate the digestive apparatus. These remarks are especially applicable to their chronic forms.

The diet advised by Dr. Dickson in Chronic Diarrhœa, is, we think, highly objectionable. We cannot regard "biscuit" in the ordinary acceptation of the term, or "poultry," or "tender beefsteak," or "broths pretty well seasoned," as suitable food for an invalid, dangerously affected with this disease. Nor do we deem it necessary to tell such an one to take "no more fluid at any time than is absolutely necessary to quench thirst," *if* we have previously directed him to drink nothing but tea,

and pure water, well thickened with gum arabic, or slippery elm. In addition to this diet, the Professor says, "a little sound Madeira or Sherry may be permitted or perhaps in preference, Port-wine." The propriety of this permission depends entirely upon the state of the case. As a general prescription it is improper. A standing objection to it, when no other exists, is that it tempts the patient to err, by increasing his appetite without preventing the evils of indulgence.

"Great advantage," says Dr. D., "has attended the use of the flannel compress on the abdomen, and the roller as advised by Dewees," and also, "your patient should guard himself carefully against all vicissitudes of weather, and avoid every exposure to cold and moisture." "Let him exercise much in the open air if he be able."

The flannel roller is very appropriate at the termination of the disease, after the counter-irritation has sufficiently subsided, and the patient begins to expose himself. The recommendation relative to vicissitudes of weather is of universal application. We cannot say the same concerning the last item of advice. We are almost inclined to reverse it.

But this would perhaps be going to the other extreme. The debilitating character of the disease, and the appropriate diet combined with the fact that exercise is *aperient*, render "much exercise" improper. The exercise should, therefore, be exceedingly gentle and never pushed far enough to produce fatigue. Where there is great debility, complete quiescence is the safest course.

The medicines mainly relied upon by the Doctor are chalk, opiates and astringents. All he says of the nitrate of silver, is that it has been more serviceable in his hands than the sulphate of copper, and is indicated, and will do good service when there is fiery redness of the tongue and fauces or apthous ulceration. Were we in this section to wait for the *mouth* to become diseased before we prescribed for the *bowels* this medicine or those of similar tendency; and to depend chiefly upon chalk, opiates and astringents, if we got rid of our patients otherwise than by Death, it would be, by directing them according to the closing instructions of the Professor, *to travel away from us* either by sea or on horseback.

Dr. Dickson's views relative to Chronic Dysentery are but little more satisfactory. According to his experience the prognosis "is on the whole unfavorable, although we are not permitted to despond: Few maladies are so intractable, and few so likely to return after a seeming cure has taken place." According to our experience the prognosis is *generally* favorable, there is little in the disease itself calculated to produce despondency, and few diseases are less likely to return after a *real* cure.

So firmly are we impressed with its curability, that we lay it down as a rule in case of failure, either that we have not prescribed properly or that the patient has not followed our prescription.

Dr. Dickson lays great stress upon the general management of Chronic Dysentery. This is good in theory, but the details of general management are the important items in practice. One of the items laid down by him coincides somewhat with the opinion we expressed when treating of Chronic Diarrhœa. "The patient, he says, should be kept very much at rest, and *in a recumbent posture* and spend most of

his time in a warm and well ventilated chamber. When the atmosphere is dry and genial, he *may be* INDULGED in a sailing excursion, or a ride in an easy carriage." This squints towards the other objectionable extreme. That a patient should be advised to "exercise much in the open air" in Chronic Diarrhœa, that is *Chronic Enteritis*, and be required to keep his room and lay on his back in Chronic Dysentery, *alias Chronic Enteritis*, is very singular to us. It absolutely becomes mysterious when we consider the high source, whence the conflicting advice proceeds. The judicious Practitioner will certainly be governed in this matter by the condition of the patient, the stage of the case, and the diet and medicine he is *then* prescribing.

"The diet, the Doctor says, should be light and nutritious" with the exception of the words "and even slightly stimulant" this is precisely the same remark with which he heads his specifications concerning the diet in Chronic Diarrhœa. With these specifications we found great fault. If the same are here implied, and we are left to this supposition, then the same objections would apply with equal force.

Concerning the medicinal treatment, the Professor observes "that there is no great certainty in the exhibition of any of our formula of medicine." We deem this a very reasonable conclusion for one who "would advise a mercurial course" as the first and best remedy "combining small doses of calomel or of blue mass with cret. ppt. (an irritant), or Dover's powder repeated at proper intervals throughout the day and administering full doses of anodyne at night." Of all courses the opiate, astringent and mercurial course combined is the most delusive. We have tried it under the most favourable circumstances and *know* that it will not answer. It was once our favourite practice, and when it failed we thought the patient certainly had not taken the medicine, or had deviated from the dietetic regulations. It happened at length that patients were put under our complete control—The medicines were regularly taken; their diet was closely watched; yet the opium, and the astringents and the calomel did not cure them, and we found to our astonishment that the fault was not *theirs*, but their Doctor's. It is easy to account for the delusiveness of this system. The mucous membrane of the bowels is irritated, inflamed or ulcerated, it matters not which. In consequence of this the liver ceases to act, or to act properly. Calomel properly given restores the healthy action of this organ. The diseased membrane is accustomed to the stimulus of healthy bile. Its absence causes griping; its contact is soothing. During its passage therefore, there is a cessation or diminution of uneasiness. The opium completes the quiescence, and without or in conjunction with the astringents, restrains the peristaltic action, thereby producing comparative constipation. The delay, thus produced in the passage of the ingesta and secretions, affords time to the absorbents to take up their watery portions. This increases the consistence of the next discharge; and the healthy bile tinges it with a more healthy hue: the two conjoined give it quite a natural appearance. The patient now feels that he is well; the Physician hopes so. In a few days however the bowels again become irritable, the liver again ceases to act and the opium and mercurial course is again resorted to, and with the same success. If the case be *really* chronic, the seeming cure will be followed

by a similar relapse. And this alternation of relief and relapse may occur fifty or an hundred times before death releases the patient, leaving the Doctor to console himself for his failure by some supposed deviations from his prescription; or to regret that he had not administered more opium and more calomel. Now during the whole course of such an attack *the inflammation*, though very slight and manageable, runs *gradually*, but almost constantly, into ulceration. It is simply *blunted* and concealed, *not cured* by this mode of treatment. The remedies used are not directly curative: calomel is, it is true, an admirable application to local inflammation, when it can be directly applied, but it is impossible to give it, at least to an adult, in sufficient quantity and with sufficient perseverance to cure in this way the diseased portion of an intestine without producing too much purgation, or an excessive and morbid action of the liver. The first effect increasing the debility of the patient and calling for an extra amount of opiates and astringents. The second demanding the stoppage of the medicine; and the torpor or collapse, which ensues, its repetition.

The administration of "James Powder" and "Pulv. Antimon.," is contrary to all correct notions of the pathology of the affection under consideration, and removes all mystery from the conclusion at which the Professor has arrived—"that there is no certainty in the exhibition of any of our formulas of medicine."

Perspiration, as a result of the subsidence of the disease, is a very favourable symptom. But a temporary relaxation of the skin, in a protracted disease, forced by medicines calculated to *aggravate* that disease, can not be advisable. We do not say it would be invariably wrong, when the skin is peculiarly harsh and dry to give "Ipecac. in minute quantities" *a few times*. We sometimes do this; but it is done in such a manner as to justify the supposition that this medicine will be absorbed before reaching the diseased part. We, however, could never consent to risk the "Pulv. Antimon." at all, nor to regard either of these powders as remedies to be habitually administered. Where they seem specially called for, the warm bath with or without salt, will generally prove more efficacious, and is less dangerous.

"The efficacy (of astringents both mineral and vegetable) is apt to be transient, and if not promptly serviceable they will be apt to disappoint you." This coincides with our belief. The Practitioner should be prepared to administer astringents at every period of the attack: *they are admirable transient remedies*; but he should constantly remember that they possess but little curative power; *that they simply check the diarrhœa*.

Our author speaks of the sugar of lead in high terms. This is a favourite remedy with all authors. It deserves a high position in the treatment of "bowel complaint" and in *the active* chronic forms of this malady, when the discharges become very frequent or exhausting, but it is not an exception to the preceding remarks. It is a transient remedy and can not be depended on as the curative one in a protracted case. Like calomel, it is highly curative in local inflammation and possesses, in addition, refrigerant and sedative properties, which render its application very soothing; but it is so highly astringent that it would, if given in sufficient doses and continued sufficiently long to come into the desired

contact with the diseased part, produce an injurious diminution of the secretions and dangerous constipation, unless combined with an objectionable quantity of nauseating or mercurial medicines.

Dr. Dickson mentions, but does not recommend, the sulphate of Copper. He says "The Nitrate of Silver deserves special mention here. It seems to be very generally applicable, and a patient perseverance in its administration will seldom fail to palliate the symptoms present in a notable degree, if it does not effect an absolute removal of them." Why not then give it the first position in the treatment of the disease? You have, Doctor, mentioned no medicine of which you have spoken thus highly or of which you could speak so highly with the same amount of truth, yet you tell us nothing about the dose—the mode of administering it—what we are to expect of it, or how long it may be safely continued. You leave us to infer, although the expression "generally applicable" seems to contradict the inference, that we must wait for the indications mentioned by you under the head of Chronic Diarrhœa. We have never used "the nitrous acid." We consider "the nitric acid" an admirable remedy in the stage of convalescence. After the proper medicines have been administered, all appearance of inflammation has ceased, and the disease has yielded to a considerable extent, but seems to be kept up by relaxation of the bowels and debility of the digestive and biliary organs, it may be resorted to with a fair prospect of success, especially if the patient has repeatedly run through the mercurial course. Ten drops thrice daily in a tumbler of water, to be sucked through a quill, is the dose and mode in which we prescribe it. The nitro-muriatic acid bath is a good accompaniment if there is œdema of the lower extremities and great torpor of the liver. We do not believe, as the Professor intimates, although it is evident he has no experience in their use, that the two acids first mentioned "exert some specific power locally"; or that they correct "the foul secretions from the diseased surfaces." Such a belief would lead to erroneous practice. It would at first cause their too early employment, and ultimately their utter rejection.

The Doctor does not recommend the balsamic and opposes the use of the terebinthinate preparations. We think both the failure to recommend the one, and the opposition to the other, great clinical blunders.

The case quoted from Forestus and reported to have been cured, "by indulgence in eating unripe apples", and others in which the beneficial effects "of the madder and other harsh varieties of this very flatulent fruit" have been affirmed, could not have been cases of Enteritis. In the summer of 1847 the writer had every appearance of having diarrhœa. His bowels became irregular in their action; sometimes he had a natural operation, then several loose ones. This alternation continued until he began to feel decidedly unwell and uneasy. He then resorted to medicine and diet and in a few days was relieved. So soon as the effects of the remedial course wore away, the complaint returned. Fearing then that the disease was chronic diarrhœa, he thought seriously of entering upon its treatment, but it was the era of that table luxury roasting-ears, and he determined before doing so to indulge his appetite for this article of food, at least one time. He eat

of it and eat freely. The next morning he was much better. Encouraged by this unexpected result, he took another dose at dinner. The next day he was well. He eat it throughout the summer and did not relapse. Having mentioned this circumstance to a professional friend, he replied; it was the remark of a distinguished Lawyer of this place, that he had no fear of disordered bowels, if he could procure roasting ears.

Now if the above were published as cases of Chronic Enteritis, and corn as their cure, half of the profession would have been credulous enough to have believed the statement. And had they come within the knowledge of any knowing old woman, or any smart officious, thick-skull, long-tongued man, every sufferer with this disease or any thing like it, in his or her neighbourhood, would have been gorged with CORN or teased with prescriptions to that effect. The fact in both of these cases was that "the looseness of the bowels" was the result of constipation. The mechanical irritation of the corn acting as a gentle aperient, removed this condition.—Hence their subsequent regularity. The revelation of some little unnoticed peculiarity connected with "the curiosities of medical experience" would doubtless remove all their mystery. Believing this, we would not prescribe in a case of Chronic Dysentery either "sour cider" or "mellons" or "unripe apples" or "roasting ears".

As in Chronic Diarrhœa so in Chronic Dysentery, Dr. Dickson winds up the treatment by directing his patients *to leave*. This is very good advice, but if the case is obstinate and the Physician believes "there is no very great certainty in the exhibition of any of our formulæ of medicine", he should not defer it to its termination. If travel presents a hope after all other restoratives have failed, its curative tendency, assisted by good general management and the taking of safe formula of medicine, presents *at first* almost a certainty of cure. It is, therefore, the imperative duty of a physician, holding the above opinion, to give this advice, accompanied with a candid avowal of the reason for so doing, *early* in every obstinate attack of the sort. It is murder to *defer* it. It is no less the duty of the more sanguine physician to advise change of climate, so soon as he finds that the remedies upon which he relies are not meeting his expectations. To defer this advice until the case is almost hopeless; or to fail to urge it with earnestness, is to sport with the human life you have been trustingly selected to preserve. This however is a prescription which few will follow. Poverty is too general; and where that does not exist, *the love of money*, as contrasted with the love of life, or rather with the fear of death *in prospect*, that is a week or a month off, is usually too great for it to be followed *in time*. As such it behooves us to exert ourselves to learn how to cure the disease in the section in which it originates.

The preceding criticisms exhibit our notions concerning its general management, and the estimate to be placed on many of the remedies commonly used. From them our views of its pathology may also be inferred. It may however be proper to state here, that the uncomplicated chronic bowel-complaint of this country in ninety nine cases out of every hundred is kept up by, and consists in Enteritis. This Enteritis being in one of three stages; 1st *permanent* irritation, or as

some call it, subacute inflammation; 2nd inflammation commonly so called, that is of such a grade that a wayfaring Doctor, though a fool, could not fail to detect it; 3d ulceration. Now all these states are nothing but different grades, or stages of the same pathological condition, and require, with but slight modifications, the same treatment. This point we wish distinctly noticed, for upon its full reception and proper appreciation, depends at least the *timely* administration of the proper remedies. *The great mistake* usually made is, *continuing to treat the disease as acute after it has become chronic*. A second, and but little less important mistake consists in attempting too great accuracy with regard to stages. The avoidance of these errors is the secret, which reveals the mystery of success. To tell when Diarrhœa or Dysentery have assumed the chronic form, that is, have become permanent affections, is frequently a matter of some difficulty. The difficulty lies in determining whether the relapses are *new* attacks, or merely *the old one* aggravated. If however these relapses recur in a few days of each other without any palpable and adequate cause, it matters not how thorough the cure may have seemed, it is best after the second or at the farthest, the third relapse, to regard the disease as having assumed the chronic form; and to treat it as such, although the pulse and skin be natural; and there be no morbid appearance of the tongue, no disorder of the digestive function, no tenderness of the abdomen upon firm pressure, no slime or mucous or blood in the stools, and no pain or uneasiness preceding, accompanying, or following their discharge. In fact upon the subsidence of every acute attack we should inform the patient of the deceptive nature of his disease and its proneness to become permanent; and impress upon him the importance of exercising the utmost prudence in order to *prevent* such a result. In case such a tendency manifests itself and medicines become necessary and we can not decide as to the propriety of commencing the chronic treatment, as good a plan as any is "to split the difference" and whilst continuing the counterirritation and diet, to employ a combination adapted to either stage of the complaint. The following pills are as good a one for this purpose as any with which we are acquainted.

R *

Pulv. Opii grs. viij to xij
 Suph. Cupri grs. iij
 Mass. Hydrarg, grs. xij to xxiv
 Mft. pillul. no xij

If a dozen or two of these fail to relieve in the circumstances just noticed, or having relieved are soon succeeded by a return of the diarrrhœal or dysenteric discharges, we may dismiss our doubts, and enter upon the siege of chronic bowel-complaint.

Having determined to pursue this course, what is to be done? *the first step* is to tell the patient candidly and explicitly your opinion of his situation; to impress upon him the probable tediousness of the curative process and call his special attention to the most disagreeable

*We learned this recipe from our townsman Dr. J. A. Scudday, and were under the impression when we wrote the above that he relied upon it in cases evidently and obstinately chronic, but find upon inquiry that our views relative to its efficacy, and the time of resorting to it, coincide with his.

parts of the treatment, to which you intend to subject him. If you come to your determination *in time*, you can say to him with great truth : "You can be cured, if you will follow to the letter, and *to the end* my prescription. Your disease is a sore in the bowels. It must be treated as an external sore. You are aware that an external sore, though nearly healed, will, by knocking off the scab, or irritating it in any way, be thrown back towards, or to its original condition. This is the way with an internal sore. Diet,—take medicine, afflict yourself with counterirritants, you will improve, but the slightest impropriety in diet, the smallest quantity of indigestible food, the first imprudent exposure will stop that improvement, re-excite the inflammation, knock the scab from the healing ulcer, annul all the benefits that have resulted from the afflictive course of self-denial and suffering, through which you have passed, *and call for its protracted repetition.*" Having made this full and fair statement, you can then inquire, of the patient whether or not he will submit *perseveringly* to the terms proposed ? If he reply affirmatively, you may begin the treatment. But the brightest geniuses have short memories where self-denial is demanded ; and you will find it necessary repeatedly to recapitulate. Should he however become wearied in well-doing, and fail to take his medicine as directed, or deliberately vary from the dietetic regulations, tell him mildly, but firmly, that though very democratic in other instances, you are somewhat tyrannical when disease is under your management, and that if he fail henceforth to adhere consistently to your prescription, he must seek another physician. A different course may gain a few more cases at first, but it will ultimately bring its follower into disrepute. The one recommended is demanded by self-respect,—will give permanent reputation, and accords with the honour and dignity of the profession.

Some physicians follow, to the letter, this latter plan with comparative strangers, but become lenient after having become familiar. A case in point is fresh in our recollection. A physician of this State was treating a woman for chronic bowel complaint, whom he had cured within the few years preceding, of this same disease, and of almost every other ailment to which flesh is heir. He had taught her in the previous attack, and during her other complaints, the impropriety, folly, and danger of irregular treatment, and deviations from his prescriptions. During the last attack, her condition varied repeatedly from the danger of death to the verge of health. The approaches to health were attended by apparent or rather practical forgetfulness of her physician and his prescription. Feeling a sympathy for the fair sufferer, he scolded, but looked over her deviations, persuaded her to try the same prescription again, or modified or changed it as much as he could in order to please her taste.

Seeing at length the error of his way, and that the case would become incurable unless subjected to regular treatment, he followed the plan proposed above, and named a month as the shortest time in which she could be cured ; she hesitated, but soon agreed to the terms. The treatment was commenced and continued to the *end*. After a severe contest the disease yielded ; the diet and medicine were changed.

About the expiration of the month, the physician called upon the fair one and found her sitting up with every appearance of confirmed con-

valescence, but was met with no congratulations on account of the fulfilment of his promise. On the contrary, she was irritable, disposed to give the credit of her cure to a professional gentleman who had been called in consultation about six days before, and had advised nothing new; also to blame her regular attendant for not having previously cured her, and after he had departed, capped the climax of ingratitude by telling a falsehood about his conduct and the part he took in the conversation, which falsehood gave her protector an excuse to do, what, it was thought for a reason worthy of the man, he had long desired to do, namely, to withdraw from him his practice and influence. Now if the physician, whose conduct we are criticising, had been consistently firm, had not yielded to a false leniency, this opportunity for the lady to lie, and a subsequent one for her would-be-protector to exhibit his meanness and pusillanimity would not have been presented. She would have been cured long before.

We have dwelt thus long on this part of our subject, because it is seemingly the least, but really the most important; and would here say to the younger members of the profession, take warning, and be firm *all the time with all persons*, with friends and enemies, with *women* wearing the garb of *ladies*, and men dressed in sheep's clothing.

The first step having been thus taken, you will *next* prescribe low diet. Our views upon this point have been to some extent given. It will be proper to state here, that it will not as a general rule be necessary to limit the patient to sago, tapioca, arrow root, &c. The stomach generally revolts at these articles, and if the use of them be required, there will be double the number of *concealed* violations of your orders. For although none but the most hardened tell falsehoods at the termination of the case, many are guilty of doing so during its progress. In view of these facts, and in order to lessen the temptation, we allow the most liberal diet which is safe. This consists of soda crackers, stale, light (not heavy) bread toasted, and soft boiled rice. A small quantity of fresh butter may be allowed over the crackers or bread, and boiled milk with the rice. To this diet many will readily adhere. Having been commenced, it should not be stopped so soon as a cure is supposed; it should be continued until the case is certain. Nor should a certain cure be inferred because the passages have become natural. Such passages sometimes occur in the midst of a bad case. Before a change of diet is advised it should have become the habit of the patient to have natural passages. The change, even then, should be very gradual. First, a soft boiled egg, (should your confidence in the prudence of the patient be sufficient to cause you to believe he will refuse a hard boiled one, if presented,) then a little tender chicken or fried ham, and so on until your perceive that he can bear the rough and promiscuous diet of the healthy.

There is one seeming exception to these directions. In some rare cases of long standing, a sudden change from low to nutritious, from delicate to gross diet, from crackers to ham, effects a cure; especially if conjoined with suitable tonics. In such cases we suppose the inflammation to have been healed by the dietetic and medical treatment previously used; and the laxity of the bowels to be kept up by a want of tone; this want of tone being occasioned in part by the medicine and

diet. There is, however, no certain way to diagnosticate this condition. It must be guessed at.

The next thing to be prescribed is counter-irritation. This is invariably appropriate, and should never be neglected. In a dangerous case, it is indispensable. In a mild one, it greatly facilitates the cure. Do not wait for soreness of the bowels on pressure, or red tongue, or pain before, with or after the operations. The existence of these symptoms are indicative of inflammation; but it may, and often does, exist without any one of them.

Cupping is the best counter-irritant to begin with. A small quantity of blood drawn in this way, aids astonishingly in the removal of any remaining acuteness, (inflammation.) Begin with it, therefore, and repeat it every day, or every other day, until this end is clearly obtained. It is not only good at the beginning, but at every stage of the case, and especially so whenever acute symptoms reappear.

When cupping has been carried sufficiently far, a succession of large blisters should be prescribed. When you think enough of these have been applied, and that the case is cured, direct a strengthening or pitch plaster, well sprinkled with tartar emetic, to be worn over the bowels, by way of giving it a beautiful and permanent finish. After this has dropped off, and the sores therefrom are sufficiently healed to bear it, the flannel roller should be recommended, if the patient still be delicate or the weather cool, damp, or very changeable.

Some of these directions may appear ridiculous and extravagant, but a little experience will teach the docile, that it is greatly preferable to afflict the patient a little too long, and thereby insure a thorough cure, than to subject him to a relapse for a want of perseverance.

As diet and counter-irritation will not alone cure an obstinate chronic bowel complaint, it becomes necessary to take a third step in its treatment. Without experience, the propriety, if not the necessity, of such a step would be presumable. General remedies alone do not cure obstinate external sores. At least we always aid them by curative topical applications. Similar aid is more important in the disease under consideration. A sore limb can be raised and kept still. Inflamed eyes can be protected from the light, but the peristaltic action of the bowels is involuntary and uncontrollable. Food must be taken into the stomach, and fæces pass through the bowels. It is highly important then, if it be possible, to apply to their diseased part local remedies, possessing a direct curative power.

Do such remedies exist? Is such an application possible? We think the affirmative of both questions indisputable. This will, we believe, be admitted. It will also be admitted that the same medicines which heal external sores, inflammations of mucous membranes, within reach, would as readily heal the inflamed portions of the same membranes out of reach could they be as readily brought into contact with them. No physician would hesitate to apply such medicines could he directly apply them.

The best remedy for inflammation in all stages, *actively* acute, is Lunar Caustic. So soon as the *acuteness* of an inflammation in the throat, or the fauces is *blunted*, it is *immediately* applied. No one now waits for an eye or a throat to be sore two or three months, or thirty,

or twenty, or ten days before making the application. Neither is such delay justifiable in inflammation of the bowels; *the same principles* should govern us. The only additional question to be asked is, can this medicine, or any other of similar tendency, be given in sufficient doses and be continued sufficiently long to come into curative contact with the diseased portion of the bowels, and yet not injure the general system nor those other portions of the alimentary canal over which it has to pass, before arriving at that upon which it is desired to act?

The Sulphate of Copper, the Sulphate of Zinc, or any other of the same class may, we believe, be given in this way. The objection to those mentioned is their aptness in large doses to produce nausea.

The Nitrate of Silver possesses all their advantages and none of their disadvantages. It is not apt to disagree with the stomach; the most delicate generally bear it well in pills, and few reject it in solution. It combines efficiency with safety, and is the master medicine in Chronic bowel-complaint.

In addition, therefore, to advising diet and counterirritation, *prescribe it as soon as you perceive that the subsidence of the acute symptoms is not followed by the permanent disappearance of the too frequent, or morbid discharges.*

The Chrystalized Nitrate should be given in the form of pills, made large by admixture with some inert substance, or in mucilaginous solution. A third of a grain, three or four times daily, is the dose to begin with, but it should be speedily increased to a grain. This is our maximum. After it has been reached, we never continue the medicine longer than two or three days in succession. Then intermitting a day or so, we recommend the same course, and repeat it again and until the disease is cured or the remedy fails. When it is borne in solution, we sometimes give eight grains in twenty-four hours, then intermit and repeat. The object in giving it in large doses in rapid succession is to insure its contact with the diseased part. Smaller doses might be entirely altered by the intestinal acids. Larger doses at longer intervals might be absorbed too soon, and even those proposed might produce too much irritation, if kept up without intermission.

We do not, however, mean to say that a grain of the nitrate is the largest dose which can be safely administered, but we do say that to prescribe a smaller dose than this, or this dose less frequently than thrice per diem, is not giving it a fair trial.

In addition to its administration by the mouth, Lunar caustic should also be administered per anum. We believe that a Chronic Diarrhœa or Dysentery, no matter how high its origin, very rarely exists long without implicating the lower bowels. As such, it is the safest plan to address remedies to them. We usually direct fifteen, or twenty grains of the caustic to be dissolved in a pint of water and injected into them once daily, or every other day. If there be ulceration of the rectum, the quantity of water ought to be lessened, and that of the caustic increased, or it should be used in the solid form. We have, however, generally found the solution recommended strong enough to answer our purpose. Our object in using such a quantity of water is to have the bowels touched as high as possible. Large as is the quantity, it is frequently retained a good while and sometimes is not returned

at all. The fact that it is immediately returned is no reason for supposing that it does no good. One might with the same propriety deny the utility of gargles.

We never knew but one person who suffered pain from an injection of the strength suggested and it seemed to benefit this one, at least it did no injury.

During the administration of Lunar Caustic, Opiates may be given in combination with it, either per mouth or anum; or separately. The latter plan is generally preferable. The patient should be directed to keep a box of opium pills, and take one of them after each discharge whenever he has more than one in the twenty-four hours.

If the liver becomes unusually torpid, calomel or blue mass may be given during the intermissions of the nitrate. Some combine this substance with blue mass, but we doubt their compatibility.

Should the nitrate of silver disagree with the patient or fail to cure, as it and all other medicines sometimes will, the Balsam Copaiva may be tried.

R — Bals. Copaiv.
Aq. Font. aa $\bar{z}$ ij
Tinct. Opii. $\bar{z}$ iiss
Pulv. G. Arab. $\bar{z}$ ij—M.

A teaspoonful of this mixture three times daily is the most rapid curer of chronic bowel complaint with which we are acquainted. The only reasons for placing it second in the list of remedies, are that it is not so safe as the first, and cannot be so generally administered: to most persons its taste is excessively disagreeable. It occasionally runs off rapidly by the bowels, thereby greatly increasing the debility, and it cannot be given if there be any disorder of the digestive apparatus. But if the disease involve no portion of the intestinal canal above the lower end of the duodenum, the stomach will retain it, and the patient is strong enough to risk its purgative tendency, or near enough for you to stop it in time, or correct its improper action; it is the best remedy you can prescribe, whether there be ulceration, or *only inflammation in its first chronic stages; whether the discharges be mucous or watery, large or small.* It is not customary to wait for ulceration of the urethra to occur before administering Balsam in Gonorrhœa. Nor is it the custom of the profession before doing so to wait for this disease to become a half month, a whole month, or six months old. So soon as *its acuteness has subsided*, this remedy is prescribed. A similar course should be pursued in chronic bowel complaint.

Comparing these two diseases according to their grades of violence, and stage by stage, Balsam Copaiva in connection with the general management previously advised, a proper respect being had to the contra-indications just noticed, will cure the last as rapidly and as perfectly as the first. It cures them too precisely on the same principles. It is an irritant to healthy mucous membranes and a restorative of such membranes chronically inflamed. It cures clap, not only by the healing power of its contact with the diseased urethra, but also by its irritant

action upon the mucous membranes of the bowels, and the sound portion of it which lines the urinary apparatus.

This is precisely the way it cures bowel complaint. It heals the diseased part not only by its locally curative, but also by its counter-irritant, power. The only questions then to ask ourself before resorting to it in this affection are these: Is the patient strong enough to bear the increase of operations *it may* produce? Are the remaining portions of the intestinal tube in a sufficiently sound condition to bear its temporary counter-irritation without permanent injury?

We use the word *may* in the first query because this medicine, instead of increasing, frequently diminishes the number of operations. It is, however, always best to guard the patient well on this point, to provide him with a solution of morphia, or a box of opium pills, with directions similar to those above, and to tell him should their use not promptly check the discharges, to apply to you immediately. After which you can stop the Copaiva, or lessen the dose as circumstances require.

As there is no incompatibility between it, and mercurials or astringents, these may be used with it whenever indicated. "We prefer giving the Balsam at first in as large doses as the patient will bear, and reducing them as the case progresses, in order to bring as much of it as possible into immediate contact with the diseased part. Merely enough opium should be combined with the mixture to quiet the stomach, and no more should be given during its administration than is demanded by the frequency of the operations. An unnecessary quantity of opiates or astringents, by preventing or retarding its passage through the bowels, causes it to be absorbed and a greater portion to pass through the urinary organs.

When the nitrate of silver has failed to act with its usual efficacy, and contra-indications require the Copaiva to be rejected, we have known the Spirits of Turpentine in doses of ten drops, thrice daily, in mucilaginous solution, to act like a charm.

The Balsam of Pear occasionally acts well, given in the form of tincture, and in tablespoonful doses, where this amount of liquor is admissible. It is not an unpalatable preparation, and agrees finely with the stomach.

We have used Strychnia, as recommended by Graves, and in various other conditions, but have found it to be merely palliative. The idea of using it in conjunction with Copaiva, the one to restore the nervous function, the other to heal the inflammation, occurred to us, and seemed worthy of trial. The experiment was made, but the Copaiva appeared to act better without, than with the Strychnia.

The thousand other remedies, which may be given in chronic bowel complaint are passed without notice, not because we wish to limit the number, or prevent their use, but because our sole object is to lay down the proper principles of treating this disease, to give the remedies mentioned their proper position in its treatment, and to urge their timely employment. We intended closing with a few cases illustrative of our views, but now deem their report unnecessary.

Thibodaux, December 20th, 1848.

III.—*The Annual Report of the Board of Health.*

SECT. 8th of an act to establish a Board of Health in and for the parish of New Orleans, approved 16th March 1848, is as follows, "And be it further enacted &c; That it shall be the duty of the Board of Health to make an annual report to the General Council as to the health of the city for the preceding year, and to suggest means for improving the same." In conformity with said law we respectfully submit,

THE ANNUAL REPORT OF THE BOARD OF HEALTH,

On the Sanatory condition of the City of New Orleans, for 1848.

It was reasonable to suppose that the fatal epidemic of 1847 would incite the city authorities to renewed exertions to promote the health of the City by the vigorous and persevering use of the measures for cleansing the streets, lots, &c., so repeatedly recommended by the Board on former occasions.

In the last report published in the *New Orleans Medical and Surgical Journal*, Jany. 1847, the committee made the following remarks: "Difficult as it is to prove, what is actually the cause of disease in the malignant fevers of the South, the experience of thousands of patient observers, in all parts of the world, for many centuries, has traced a connection between certain *local causes* and *certain diseases*. In the absence then of *all* the knowledge on this subject, which we desire to possess, we should not be deterred from doing that which ample experience has demonstrated to be eminently beneficial. We repeat let all known causes of disease be removed, and all such measures as have been recommended rigidly enforced; and to accomplish this, we invoke the aid and co-operation of the Mayor and the three City Councils, feeling assured, especially after the experience of the last season, that New Orleans may thus be rendered as healthy as any city on this continent, from January to December."

It will be admitted by those who have paid attention to this subject, that with pure air and pure water, no malignant endemic can be generated. And under the same circumstances, we should have little to fear from imported diseases; they would necessarily become so mitigated as to allay all alarm, and would speedily disappear.

The question then naturally arises, can this great object be accomplished in our city? Pure water we have already, although it is not furnished in sufficient abundance to many, and not at all to some of the inhabitants, especially of the working class who reside in the rear of the City. This could be easily remedied.

Nature has furnished us with water; but art has every thing to do in purifying the air. It is not to be supposed that, in a large city like New Orleans, situated on the alluvial bank of the Mississippi, any efforts of the city authorities can avail to make the atmosphere as pure as what we may breathe on the top of the Blue ridge or of the Alleghanies, yet much can be done to mitigate the disease and render the city as comparatively healthy as any other in our Country.

Wise police regulations have been adopted from time to time by our Councils, and commissaries have been appointed to carry them into effect; yet of what avail are resolutions and ordinances, if they are not

rigidly enforced, and if the officers appointed for the purpose are not only remiss in the performance of their duty, but actually do, or permit to be done, the very things prohibited by the Board of Health, and which have over and over again been spoken of as the most fruitful causes of our malignant endemics. We allude to the deposit on the bank of the river of so much of the offal of the city, consisting of animal and vegetable remains in a state of putrefaction, well incorporated with liquid viscid mud. We say, let *this* nuisance be at once abated; let the commissaries be compelled to do their duty and their whole duty, subject to fine, or removal from office in default thereof.

In an article written by a member of this committee who was not at the time a member of the Board of Health, and published in the Medical Journal, September 1848, after recapitulating all the sanitary measures that have been so frequently urged, the writer observes,

"Such measures have been repeatedly recommended, but without producing any important result. We must suppose that the city authorities have been, and still are, aware of their importance and practicability. Why then, has so little been done? How is it that we hear every season the same complaints of the filthy condition of the gutters and streets, in many parts of the city, of the offensive garbage thrown on the bank of the river and into the docks, and of stagnant water in unoccupied lots, annoying the whole neighborhood? Are the members of the City Council afraid of the expense? Suppose a hundred thousand dollars were annually appropriated to improve the sanitary condition of the city, I believe there is not an intelligent man in the city who would not heartily sustain the Council in such an enlightened and benevolent policy."

The article from which the foregoing extract was taken was written on the 20th July, since which time to the close of the year, the condition of the city became worse and worse. In the number of the Journal for November 1848, one of the Editors, Dr. Hester, remarks under the head of American Medical Intelligence "*The Board of Sanatory Commissioners of London, and some other European cities, state in their reports 'that it is the combination of humidity with impurity of the atmosphere, which so powerfully predisposes to Cholera.'*" This important conclusion, to which the above commission arrived, after carefully investigating the subject, is eminently applicable to this city; for are we not constantly surrounded by, and living in, a humid atmosphere? And do not our streets, gutters, alleys, vacant lots &c., &c., abound in filth, debris, and every variety of animal and vegetable matter?"

The state of things here alluded to, is not only in this climate favorable to the production or aggravation of Cholera, but of every description of malignant endemic fever.

It has been observed on a former occasion that however the elimination of an endemic may have been influenced by humidity, yet heavy rain, occurring almost daily during its prevalence, has not produced any sensible effect. When the Yellow Fever becomes epidemic in the City, it pursues its onward course until the allotted period of its visitation has arrived, unaffected by drouth, or rain, or storm, or hurricane.

From observations made at different points in the valley of the Mississippi during a period of more than twenty years, the average quantity

of rain that annually fell, amounted to about 2.5 feet, since 1845 ; however, there has been a steady and rapid increase since over that average. That year (1845) the quantity of rain as ascertained by Mr. Lillie's rain guage, which is graduated to the thousandth part of an inch, was 2.419 feet, and there were 108 rainy days.

In 1846, the quantity of rain during the year was 6.39 feet, and there were 114 rainy days. In 1847, it had increased to 8.407 feet, which fell in 104 days. And in 1848, it amounted to the unusual quantity of 10.750 feet, and the number of rainy days was 116, as follows :

In January,	1848,	8 rainy days—	fall of rain	18.387 inches.
In February,	"	9 do	do	11.390 do
In March,	"	6 do	do	2.705 do
In April,	"	8 do	do	7.643 do
In May,	"	5 do	do	11.675 do
In June,	"	16 do	do	30.306 do
In July,	"	22 do	do	14.110 do
In August,	"	15 do	do	9.388 do
In September,	"	6 do	do	2.075 do
In October,	"	5 do	do	2.230 do
In November,	"	4 do	do	7.265 do
In December,	"	12 do	do	11.100 do

It is worthy of remark that the quantity of rain that fell in the month of June was fully equal to the average annual quantity for more than twenty years.

The whole known amount of interments in the City of New Orleans from 18th December, 1837, to the 12th of February, 1848, a period of eight weeks, was 869 ; being at the rate of 108 and a fraction per week. During that time the following cases of fever were reported : of simple fever, 5 ; of bilious fever, 1 ; of congestive fever, 2 ; of malignant fever, 2 ; of promiscuous fever 1 ; of putrid fever 1 ; of putrid malignant fever, 1 ; of remittent fever, 1 ; of nervous fever, 1 ; of scarlet fever, 13 ; of *typhoid* fever, 46 ; of *typhus* fever, 95 ; of *yellow* fever, 1.

The congestive, malignant, and pernicious fevers should all be classed under one head ; and there is no need of any distinction between putrid, and putrid malignant fever.

From the 12th of February to the 28th of May, a period of fifteen weeks, the number of deaths reported amounted to 1748, rather exceeding 116 deaths per week. Amongst them the following fevers were reported. Simple fever, 6 cases ; ataxic fever, 1 case ; bilious fever, 3 cases ; congestive fever, 7 cases ; *typhus*, 181 cases ; intermittent, one case, malignant fever, two cases ; of nervous fever, 7 cases ; of remittent 1 ; of *scarlet* fever, 74 cases ; of *typhoid*, 55 ; and of pernicious fever 5 cases.

From the 28th of May to the 19th of August, twelve weeks, the number of interments was 1478, rather exceeding 123 per week ; of which there were reported 11 cases of simple fever ; ataxic, one case ; adynamic, 2 cases ; bilious fever, 4 cases ; bilious remittent, 2 cases ; congestive fever, 38 cases ; intermittent, 5 cases ; of remittent, 4 ; of nervous fever, 3 cases ; malignant fevers, 11 cases ; scarlet fever, 23

cases; malignant ditto, one case; of typhoid fever, 25 cases; typhus fever, 45 cases; and *yellow fever*, 113 cases.

From the 19th August to the 21st October, ten weeks, the interments amounted to 1662, making an average of more than 138 per week; of these deaths, we have recorded, as simple fever, 7 cases; adynamic, 2 cases; bilious fever, 11 cases; congestive, 26 cases; intermittent, 8 cases; malignant ditto, 5 cases; nervous fever, 1 case; malignant fevers, 12 cases; typhoid fever, 15 cases; typhus fever, 25 cases; remittent fever, 2 cases; and *yellow fever*, 635 cases.

At the time of writing this report, the specifications of the various diseases of which persons have died in this city during the remainder of the year 1848, have not been made out. The total interments from the 21st October to 1st November, amounted to 167. And the total interments for the months of November and December were 1795, in all since last report, 1962. Of these, about fourteen hundred were cases of Cholera.

The aggregate number of reported interments in the city of New Orleans, from the 18th December 1847, to 1st January 1849, is 7,719, the greatest mortality having occurred since the 19th August, 1848.

The bills of mortality were no doubt swelled during the first six months of the year, by the sick soldiers returning from Mexico, exclusive of those who died in the Military Hospitals; amongst them were several cases of yellow fever, contracted in Vera Cruz, which did not spread beyond their persons.

Our endemic yellow fever commenced slowly and gradually at remote points after the middle of July; it attained its climax between the last week of August and the end of September; and disappeared early in October. It was not considered epidemic by the Board, notwithstanding the large amount of persons that must have had the disease, as inferred from the number of deaths. This decision of the Board was based upon very interesting statistics taken from the books of the Charity Hospital, by Dr. Fenner, to whose industry and research we are indebted for many important facts that might otherwise have been overlooked. We quote from the article already spoken of on the sanatory condition of New Orleans.

"In 1841, when the fever was epidemic, it appears from the statistics, that there occurred at the Charity Hospital 1,113 cases of yellow fever, 328 of intermittent, and 42 of remittent fever during the months of August, September, October and November.

In 1842, during the same four months, there were 410 cases of yellow fever, 563 intermittents and 121 remittents. It is more probable that the disease was sporadic and not epidemic that year.

In 1843, when it was again epidemic, there were 1053 cases of yellow fever in six months from July to December; during the same time, 587 cases of intermittent and 194 of remittent fever.

In 1844, it was evidently sporadic, there being but 152 cases of yellow fever against 990 of intermittent and 174 of remittent fever.

In 1845, there was but one case of yellow fever in the Hospital.

In 1846, it was sporadic; in four months, September, October, November and December, there were 149 cases of yellow fever in the Hospital, 1126 of intermittent and 68 of remittent fever.

In 1847, commencing with July and ending with November, there occurred at the Charity Hospital 2,811 cases of yellow fever, 988 of intermittent and 188 of remittent fever. It was epidemic and unusually severe.

Three inferences are drawn from the foregoing facts: *First*—That when the intermittents predominate to a considerable extent throughout the summer and fall months over the yellow fever cases, as in 1844 and 1846, the disease may be safely set down as sporadic.

Second.—When in August and September the intermittents suddenly decreased with large preponderance of yellow fever, as in 1847, then there can be no doubt of the existence of an epidemic; which was the case in 1841 and 1843.

Third.—That the intermittents, never being entirely suppressed, during the climax of the epidemic yellow fever, and increasing so rapidly as the epidemic declines, considerable affinity would appear to exist between the two descriptions of fever, which, if true, would forever settle the question of quarantine in the case of the yellow fever.

We propose now to apply this rule in deciding the question of sporadic or epidemic as the character of yellow fever of 1848.

That year, from July to November, there were in the Charity Hospital 1694 cases of intermittent, 421 cases of remittent, and 1195 of yellow fever.

In July,	219	cases of Intermittent,	101	Remittent,	31	Yellow Fever.
In Aug.,	310	do.	do.	87	do.	462 do.
In Sept.,	291	do.	do.	64	do.	597 do.
In Oct.,	334	do.	do.	68	do.	105 do.
In Nov.,	270	do.	do.	23	do.	

The foregoing are the admissions into the hospital during the time specified, leaving 270 intermittents and 78 remittents remaining in the wards from previous admissions.

There were also at the same time numerous cases of dengue, which appeared and passed away with the yellow fever.

From the consideration of these circumstances as compared with the results of former years, the Board came to the conclusion that the yellow fever of 1848 was sporadic—an opinion based upon the axiom that all other febrile diseases are overpowered by an epidemic and assume the same type. When, however, such diseases co-exist with yellow fever and largely predominate, there is no reason whatever to suppose the existence of an epidemic; and if they are about equal, the probability is, that yellow fever is sporadic; although the independent foci of the disease may be very abundant.

Now as regards the yellow fever of 1848, against 1195 cases of that disease, we find 1154 cases of intermittent, and 320 of remittent fever, in all 1474 cases, besides the numerous cases of dengue, congestive fever, &c., and without including the cases that were previously in the hospital. Therefore, if the rule laid down be correct, the Board of Health were fully justified in the opinion they expressed.

One hundred and ten deaths from scarlet fever occurred in 1848 prior to the 19th August, at which time the sporadic yellow fever had reached its climax, and since which time, none were reported up to the 21st October.

Four hundred and eighty-seven deaths of typhus and typhoid fevers occurred from the beginning of the year to the 21st October, of which forty cases were reported during the climax of the yellow fever. We repeat the opinion, more than once expressed on previous occasions, that a distinct building, or suitable buildings, should be provided for the immigrants suffering with typhus or typhoid fever; and that type of disease should never be introduced into the Charity Hospital.

We would remind the members of the different councils, of the alarm which existed here on account of the ship or typhoid fever in 1847, and in the first part of the year 1848; of the frequent meetings of the committees; of the discussions that took place, and the resolutions that were adopted—all of which however resulted in nothing being done.

Amongst those resolutions, we will on the present occasion resuscitate the following—

“That measures be adopted by the proper authorities to relieve the Charity Hospital.

“That suitable buildings be rented or purchased near the river between the Convent and the Barracks, for the reception and taking care of such persons as may arrive in this city affected with ship fever.

“That measures should be adopted to prohibit any vessel having on board persons affected with ship fever, from coming up to the levee. Every such vessel should be required to anchor opposite the point where the building or buildings may be located, and all persons afflicted should be immediately removed therefrom, and the vessel thoroughly ventilated and cleansed.”

We recommend the immediate adoption of some such measure as the foregoing.

All the accounts received from Ireland, represent that unfortunate country to be, if possible, in a worse condition than ever. The immigration into the United States is greater than at any former period, and the reports of mortality caused by ship fever, should stimulate us at once to the exercise of vigilance and energy. We learn from the papers that a vessel arrived at New York from Liverpool, after a passage of 68 days, with three hundred and thirteen passengers, seventy-six of whom were sick with the ship fever, and three died on the passage. The mate was landed in a dying state, and the captain was very sick. The Express was informed, by Dr. Harris, that there were then over seven hundred sick in the hospitals at quarantine; that they were lying two in a bed; and that this increase of seventy-six will make it very difficult to find accommodations for the sick. We also hear accounts of the ship fever being amongst the immigrants at Philadelphia and Boston.

They have been arriving in this city for the last two months in unusually large numbers, and crowds are still expected. Ship fever then may be anticipated, and some preparation should be made to meet it on its arrival.

Small pox is another disease which ought never to be introduced into the Charity Hospital. It is a disease which can be easily insulated, whereas in a large hospital, crowded with patients, its ravages might be extensive and fatal. In almost all large cities we find a distinct hospital provided for this loathsome disease.

We notice in the November number of the Medical Journal, for 1848, some curious statistical facts, to which we desire to call the attention of the public authorities. They are taken from the record of the Board of Health, and are published in an article on the health of the city, by Dr. Hester—

“From the 1st of May up to the 14th of October, the total number of deaths in New Orleans, was 3332; of these were entered by certificates	
From physicians, - - - - -	1551
From the Charity Hospital, - - - - -	725
From private hospitals, - - - - -	250
Died at other places and brought here for interment, - - -	6
By citizens' certificates, - - - - -	237
By coroners' certificates, - - - - -	116
By commissaries' certificates, - - - - -	206
By midwives' certificates, - - - - -	94
Uncertain, - - - - -	147
Total, - - - - -	3332

Of these up to the 28th October, the number of deaths from yellow fever was 716, of which more than one-half died in the hospitals of the city. From the foregoing statement it will be seen that not quite one-half of those who died, came under the care of physicians in private practice.

It may be asked, were the 206 cases returned by our commissaries suffered to languish and die without medical aid?”

But there is a question of far graver consideration. What are we to understand by the 147 interments set down as *uncertain*?

Can any other inference be drawn but that the 147 reported uncertain were buried without any certificate, in direct violation of a law, in the strict observance of which, every good member of society is deeply interested?

Who can tell how many were summarily disposed of by bad men, instigated by motives of envy, jealousy, cupidity, or revenge; or how many died fairly of disease?

We trust that we may never have occasion to allude again to this subject.

These interments it will be observed terminate in the month of October, about six weeks before we heard of a single case of the cholera in the city.

We have already seen that more rain fell in the month of June than in any other month of the year 1848, and a fraction more than the annual average for twenty years prior to 1845. Also that September and October were the driest months.

It was originally designed that a meteorological journal should be kept by the Secretary of the Board of Health, and a sum of money was appropriated for the purpose of purchasing the necessary instruments. They were never procured, and probably the money was well saved, as the meteorological journal, kept by Mr. T. D. Lillie, a weekly abstract of which is published in the Medical Journal, answers every purpose. We therefore copy from that journal—

1848.	THERMOMETER.			BAROMETER.			Course of Wind.	Force of Wind— Ratio, 1 to 10.
	Max.	Min.	Rg'e.	Max.	Min.	Rg'e.		
January 1, ...	71 0	38 4	33 9	30 36	30 11	0 25	S. E.	3 $\frac{1}{4}$
January 8, ...	73 0	42 0	31 0	30 36	29 90	0 46	S. E.	2 $\frac{3}{4}$
January 15, ...	73 5	34 5	39 0	30 48	30 14	0 34	E.	3
January 22, ...	67 0	49 5	17 5	30 24	30 01	0 23	N. E.	2 $\frac{3}{4}$
January 29, ...	66 5	51 5	15 0	30 22	30 02	0 20	N. E.	3 $\frac{1}{2}$
February 5, ...	72 0	45 0	27 0	30 19	29 84	0 35	S. E.	3 $\frac{1}{4}$
February 12, ...	72 5	40 0	32 5	30 30	30 20	0 10	N. W.	3
February 19, ...	76 5	52 0	24 5	30 35	29 94	0 41	S.	3
February 26, ...	79 0	48 5	30 5	30 30	29 87	0 43	N. W.	3 $\frac{1}{2}$
March 4, ...	78 7	46 0	32 7	30 42	29 98	0 44	N. W.	3
March 11, ...	66 0	43 0	23 0	30 49	29 95	0 54	N.	3 $\frac{1}{4}$
March 18, ...	75 7	52 5	23 2	30 40	30 05	0 35	S. S. W.	2 $\frac{1}{2}$
March 25, ...	81 0	65 0	16 0	30 28	30 00	0 28	S. W.	3 $\frac{1}{4}$
April 1, ...	83 0	60 5	22 5	30 33	29 50	0 23	S. E.	2 $\frac{1}{2}$
April 8, ...	77 0	61 5	15 5	30 47	29 83	0 64	S.	4
April 15, ...	82 5	64 0	18 5	30 20	29 94	0 26	S.	3
April 22, ...	79 0	58 0	21 0	30 34	30 10	0 24	S. E.	3
April 29, ...	84 5	61 5	23 0	30 33	29 73	0 60	S. E.	3 $\frac{1}{4}$
April 30, ...	86 0	71 0	15 0	30 20	29 75	0 45	S. E.	2 $\frac{3}{4}$
May 6, ...	87 5	70 0	17 5	30 25	29 87	0 38	N. E.	3
May 13, ...	85 0	72 5	12 5	30 32	29 79	0 53	E.	3 $\frac{1}{2}$
May 20, ...	85 5	72 0	13 5	30 32	29 98	0 34	S. E.	3 $\frac{1}{2}$
May 27, ...	91 5	74 0	17 5	30 16	29 90	0 26	S. E.	2 $\frac{3}{4}$
June 3, ...	88 5	73 5	15 0	30 05	29 70	0 35	S. S. W.	3
June 10, ...	88 5	74 5	14 0	30 15	29 90	0 25	S. E.	3
June 17, ...	82 5	71 0	11 5	30 18	29 94	0 24	E. S. W.	3 $\frac{1}{2}$
June 24, ...	90 5	74 0	16 5	30 30	29 95	0 35	S. W.	2 $\frac{3}{4}$
July 1, ...	89 0	75 0	14 0	30 13	29 95	0 18	S.	3
July 8, ...	90 7	77 0	13 7	30 17	29 88	0 29	W.	3 $\frac{1}{4}$
July 15, ...	89 0	73 5	15 5	30 20	29 60	0 60	S. W.	3 $\frac{1}{4}$
July 22, ...	87 0	74 5	12 5	30 10	29 87	0 23	S.	3
July 29, ...	84 0	74 5	9 5	30 24	30 08	0 16	S.	2 $\frac{1}{2}$
August 5, ...	90 0	76 0	13 0	30 26	30 00	0 26	S. W.	2 $\frac{1}{2}$
August 12, ...	88 5	74 5	14 0	30 26	30 04	0 22	E.	3
August 19, ...	87 0	75 5	11 5	30 12	29 95	0 17	S. E.	4
August 26, ...	86 0	76 5	9 5	30 30	29 95	0 35	N.	3
September 2, ...	91 0	78 0	13 0	30 20	29 90	0 30	N. W.	2 $\frac{3}{4}$
September 9, ...	87 5	75 0	12 5	30 18	30 00	0 18	S. W.	3 $\frac{1}{4}$
September 16, ...	89 5	77 0	12 5	30 19	29 95	0 24	S. E.	3
September 23, ...	86 0	65 0	21 0	30 30	29 90	0 40	N. E.	3 $\frac{1}{4}$
September 30, ...	82 5	60 5	22 0	30 22	29 90	0 32	N. W.	3 $\frac{1}{2}$
October 7, ...	84 0	58 5	25 5	30 50	30 06	0 44	N. W.	3 $\frac{1}{2}$
October 14, ...	84 5	70 0	14 5	30 35	29 94	0 41	N.	3
October 21, ...	83 0	59 0	24 0	30 43	30 04	0 39	W.	3
October 28, ...	80 0	67 0	13 0	30 32	30 00	0 32	S. W.	3 $\frac{1}{4}$
November 4, ...	77 0	46 0	31 0	30 50	29 94	0 56	N. W.	3
November 11, ...	73 0	48 0	25 0	30 57	30 22	0 35	N. W.	3 $\frac{1}{2}$
November 18, ...	73 0	42 0	31 0	30 48	30 18	0 30	N. W.	3 $\frac{1}{2}$
November 25, ...	63 0	43 0	20 0	30 58	29 98	0 60	N. W.	3 $\frac{1}{4}$
December 2, ...	67 0	35 5	31 5	30 48	29 75	0 73	W.	3 $\frac{1}{2}$
December 9, ...	76 5	52 0	24 5	30 24	29 96	0 28	S.	3 $\frac{1}{4}$
December 16, ...	69 5	43 0	26 5	30 16	29 96	0 20	N. E.	3 $\frac{1}{2}$
December 23, ...	79 0	58 0	21 0	30 29	30 10	0 19	S.	3 $\frac{1}{2}$
December 30, ...	74 0	42 5	32 5	30 35	29 92	0 43	N.	3

The warmest day of the year occurred between the 27th of May and the 3d of June; the Mercury in the thermometer was up to $91^{\circ} 5'$.

The coolest day was between the 8th and 15th of January; the Mercury was as low as $34^{\circ} 5'$; the range between these two temperatures is 57° .

The greatest weekly range is 39° during the week ending the 15th of January; the lowest weekly range was in the latter part of the months of July and August, being in both instances $9^{\circ} 5'$.

The Maximum height of the mercury in the barometer was $30^{\circ} 58'$ between the 18th and 25th of November; the Minimum about the 1st of April $29^{\circ} 50'$; range for the whole year $1^{\circ} 08'$; greatest weekly range $0^{\circ} 73'$ in the last week of November; the least range $0^{\circ} 10'$ is between the 5th and 12th of February.

Soon after the organisation of the first Board of Health under the new act of the Legislature referred to in the caption of this report, it was resolved that the offal and other filth deposited in the streets should be removed by the contractors in *two hours*; the authority for this decision of the Board is found in the 5th section of the act already quoted.

"Be it further enacted etc. : That it shall be the duty of the Board of Health to designate the hours when offal or other filth shall be deposited in the streets, and the time when the same shall be moved by the contractor for cleaning the streets. In case the regulations of the Board of Health on this subject should be violated, it shall be the duty of any Health Warden of the Ward in which said regulation has been violated, to report the same promptly to the attorney or assistant attorney of his Municipality; said attorney or assistant attorney shall immediately institute suit in the name of his Municipality for the penalty, which is hereby imposed, of not less than twenty, nor more than one hundred dollars, and in case said penalty be recovered, said attorney shall be entitled to a tax fee of ten dollars to be paid by the defendant, and the penalty shall be for the use of the Municipality."

This is unquestionably a wise provision of law, and it would prove eminently beneficial in promoting the health of the city and the comfort of its inhabitants, if rigidly enforced—but it is to be feared that like many other wise regulations and judicious ordinances, it has slumbered on the statute book.

So far from the offal etc. having been removed two hours after it was deposited in the streets, (a measure that was earnestly recommended by a former Board of Health) it has often remained for two days—and frequently has been washed back into the gutters by heavy rain and thoroughly mixed up with the liquid viscid mud, producing a fermenting, putrefying mass, that cannot be otherwise than productive of some formidable disease.

It may be alledged that owing to the vast and unusual quantity of rain which fell during the year 1848—the consequent wretched condition of the streets which could not, under such circumstances, be repaired—and the many obstructions at the intersections of streets, where bridges were being substituted for open gutters, and which state of things existed for so many months—it was impossible for the contractors to comply with the requisition of the Board of Health, and that the Health

Wardens seeing this, forbore through a sense of justice to direct the institution of suit according to law.

We would hope that this is the true state of the case, rather than to suppose that there has been wilful neglect of duty.

Admitting then that the weather was in fault and not the contractors, we have still to protest against the disposition which is made of the abominable mixture of putrid vegetable and animal remains thoroughly incorporated with the viscid mud of the gutters. Over and over again it has been urged by different Boards of Health that all such offensive offal should be removed to some point where it could not possibly contaminate the atmosphere, which our citizens are compelled to breathe. It was formerly deposited near the edge of the swamp in the rear of the City; but so many houses have been built, and population has so extended in that direction, that it would not be proper at present so to dispose of the nuisance in question. The correct plan is to carry it in flats, constructed for the purpose, some distance from the bank, and throw it into the current of the river, to be carried off at once from the city and its vicinity.

Instead of this, as the fresh alluvial deposits are dug up and carried away to be used in building, the excavations are frequently filled up with the garbage of the city, causing the most offensive smell and polluting the atmosphere. This was peculiarly the case alongside of the wharf of the first Municipality ferry, where the bank had sunk several feet.

Moreover, this practice is prejudicial to the commercial interest of the first Municipality, for it has on several occasions caused the removal of five or six ships from the first to the second Municipality—and it creates an abiding prejudice against the harbor of the first, in favour of the second and third Municipalities.

Before the passage of the last act of the Legislature on the subject of a Board of Health, the various commissaries were considered the proper persons to carry into effect the sanitary resolutions of the Board. They however were appointed and paid by the different councils, and the Board had no control over them. And moreover, when on several occasions, complaints were made to the aldermen by whom they were appointed, by members of the Board of Health, by captains of ships, and the Harbor Masters, all such complaints and remonstrances were unheeded.

It does not appear that the Health Wardens are entitled to receive any remuneration for their services, which are of a peculiarly disagreeable nature, often exciting prejudice and even enmity, if the duty, required of them, is faithfully performed.

“Sect. 4. ***** It shall be the duty of said Health Wardens from time to time to visit and inspect the condition of the houses and lots in their several wards, and should they discover therein any nuisance likely to prove injurious to the public health, it shall be the duty of any Health Warden of the Ward in which said nuisance may be found, to order its removal; and if within the time designated by such Health Warden for said removal, the order be disobeyed, it shall be the duty of said Health Warden to report the same to any two members of the Board of Health; and if said members approve the order made by the Health Warden,

they shall direct the immediate removal of said nuisance at the expense of the tenant or owner of the property."

Many persons are exceedingly jealous of what they consider intrusion into their domicils, and very tenacious of rights, which they imagine are inseparable from the occupation of a house and lot. Of this, the Health Wardens are doubtless aware, and without some adequate compensation, it is not probable that the duty required of them by law, will be ever adequately performed.

Under such circumstances the Board of Health can do little more than to record passing events—as the advent, progress and termination of endemics or epidemics—the number of interments occurring daily, weekly or monthly, accompanied by comments—and they may point out nuisances and fruitful causes of disease, and devise and recommend the proper measures to abate the one, and remove the other.

They are conscious that the public eye is fixed upon their every act; and whether they understand it, or not, they have voluntarily assumed a heavy responsibility. Professional men *will* differ in opinion; and it is right that they should, for truth is as certainly elicited by the collision of mind, as is fire by the contact of flint and steel. They must therefore expect, and endure without flinching, the criticism of their professional brethren, and the denunciation of the press.

The yellow fever having disappeared, the city became more healthy under the balmy influence of the comparatively dry and mild fall season.

But if the surface of the earth was dry, a short distance beneath, it was far different; the alluvial soil was so completely saturated with the previous heavy rains, that no work on the streets would stand one day, in the great thoroughfares of business parallel with the river; hence no positive amelioration of local circumstances took place during the pleasant weather of our Indian Summer.

Two warning voices were raised in anticipation of the approach of Cholera. In the September number of the Journal there are the following remarks:

"We have had from time to time information of the progress of the Asiatic Cholera in the Eastern part of Europe. Late intelligence represents it to be raging in Moscow. No quarantine measures or military posts guarding all the approaches to a country have ever been able to retard or avert its progress. Cleanliness, however, is its greatest palliative; and all writers and observers agree that whatever part of a town is most filthy, with stagnant water etc., feels especially the weight of that severe scourge.

The ravages would be dreadful in this city in the present condition of the streets, wharves and low lots filled with stagnant or putrid water. The bare possibility of such a visitation should stimulate our City authorities to carry into effect those sanitary measures which have now, and so often before, been recommended to them as the results of common sense and long and universal experience."

Again, in the November number (after alluding to the condition of the streets), the Editor observes:

"Let this subject be looked to in time, for if neglected until the Cholera makes its appearance among us, we shall be too much occupied in burying the dead, etc. to give it any attention."

That the Cholera was not so fatal as was anticipated under the existing circumstances up to the close of the year 1848, must be admitted; and it is surprising, considering the large number of immigrants that were daily arriving at our port and thronging our streets.

Whether it was of Asiatic origin, or originated in this city, remains to be discussed.

It has already been observed that when the yellow fever prevails either in the sporadic or epidemic form, it appears to be very little, if at all, influenced by changes of weather; it may rain or shine—or be a little cooler or warmer—it matters not, the disease pursues its onward career until it has completed its allotted period of duration.

There is little doubt however but that the unprecedented quantity of rain which fell in 1848, had directly or indirectly considerable influence in the production of Cholera.

Probably since the time when the first paving was done in New-Orleans, the streets had never been in so bad a condition as at the beginning of the month of December. The continued rain and the saturated state of the earth had rendered repairs useless or impracticable; the elements of fermentation and putrefaction accumulated fearfully in every direction, until the atmosphere was polluted with poisonous exhalations, in which a sickly acid smell at times predominated, and which were pressed down in a concentrated state near the surface, by the dismal fogs which shrouded the river and the city in gloom.

Under such circumstances, it was apparent to all intelligent persons that *malignant and fatal disease of some kind or another* was inevitable, unless the air should be purified by repeated frosts. But instead of the much desired frost the weather became more and more oppressive; there were frequent and heavy rains alternating with a damp and *sultry* state of the atmosphere, which moreover was charged with watery particles, making respiration laborious to the most vigorous lungs; and abundantly productive of rheumatic and catarrhal affections; at the same time complaints of Diarrhœa or Dysentery, or a general disturbance of the functions of digestion and assimilation, might be heard at every corner.

The highest *weekly* range of the thermometer in the month of December is set down by Mr. Lillie at 79°, but there were days, when the temperature was higher; on several occasions 80° were noted down, and even 84° were more than once reported on good authority.

On the 5th of December Dr. Fenner says that he “attended a gentleman on Customhouse street who labored under vomiting, pain and spasms in the bowels, and prostration to such a degree that, *if epidemic cholera had been supposed to be here, no person would have hesitated to pronounce him a case.*” We are the authors of the italics, desirous to call particular attention to the fact stated. This was six days before the arrival of the ship Swanton after a passage of thirty nine days from Havre, with 280 steerage passengers, chiefly Germans and some French.

Again some days previous to the arrival of the Swanton, Dr. Fenner informs us in his interesting article published in the Picayune the 28. January “on the late Epidemic”, that three or four negroes were

attacked with Cholera morbus on the same night and at the same house in Gravier Street. *** Similar cases were observed in the practice of a number of Physicians in different parts of the city, all going to show, as it appears to me, that the epidemic influence of Cholera was gradually being matured and developed in our midst."

On the 6th December a ship arrived from Hamburg with about 250 steerage passengers, after a passage of fifty five days. We are informed that Cholera prevailed in Hamburg when the ship sailed, and that six or seven cases terminating in death occurred on board before the vessel was clear of the river Elbe. From the time the vessel was at sea until she arrived here, there was no case of Cholera on board. A man supposed to be one of the immigrants from Hamburg died in the Charity Hospital soon after the epidemic broke out here, who stated that he had recently arrived from Germany on board a vessel which had lost several passengers by Cholera. (Dr. Fenner.)

We pass over the case of the bark Callao from Bremen which arrived on the 18th December after a passage of forty eight days with 152 Germans,—that there should be purging and vomiting etc. amongst the passengers, is as natural under the circumstances of the case, as the existence of ship fever in a vessel from Belfast or Liverpool crowded with Irish, debilitated by famine, and almost heartbroken by misery.

The ship Swanton arrived on the 11th December from Havre with immigrants.—No Cholera existed there at the time of her departure, but no person can tell from what part of Germany, or the Continent of Europe, some of the passengers may have come.

The day after her arrival two cases resembling Cholera were noticed in the Charity Hospital; they were immigrants by the Swanton, and both died. The third case was that of a woman carried directly from the ship to the Hospital in a state of Collapse; she also died.

The next case reported, was a man working as a stevedore on board the ship Elizabeth, in the Third Municipality, at least a mile and a half from the ship Swanton. After this the number of cases increased rapidly, and on the 21st December the Cholera was pronounced to be epidemic by the Board of Health.

It is important to bear in mind that the three vessels, to which an attempt might be made to trace the Cholera by importation, arrived successively on the 6th, 8th, and 11th December, and that before the arrival of the first of said ships—to wit: on the 5th, a case of Cholera of unequivocal character was attended by Dr. Fenner.

As regards the doctrine of importation, the facts just quoted so far from sustaining, are decidedly adverse to that opinion. There may, however, be other facts that have not yet come to light, which might entirely change the aspect of this interesting question.

Why the term *Asiatic* Cholera should have been given by the house physician of the Charity Hospital, to the first case which he saw—one of the immigrants from the ship Swanton—we cannot clearly comprehend.

If not imported, it could not be Asiatic. We find, on looking over the obituary reports, cases called Cholera, Asiatic Cholera, Cholera Morbus, Cholera Asphyxia, Cholera Spasmodic. They were all modi-

fications of one and the same disease, bearing the same affinity to our common bowel complaints, that yellow fever bears to the extensive family of intermittents; and varying *only* in intensity according to modifying circumstances.

The Cholera that prevailed in this city was unquestionably influenced by changes of weather; it was aggravated when the days were cloudy, and damp, and sultry, with a heavy fog; and it declined when the atmosphere was clear and cool, and the wind from the North or North-west. And this occurred not only once, but two or three times. Asiatic Cholera, however, is not controlled in this way; its ravages are as terrible in St. Petersburg in about 60° North, when the Neva is frozen over and the ground is covered with snow to the depth of several feet, as in the sultry valley of the Ganges, between the 25th and 30th degrees of North latitude.

The Asiatic Cholera of 1832, whose steps could be traced over half the surface of our earth, traversing land and ocean with like speed and energy, was as fatal to the *acclimated* as to the unacclimated. No such thing as immunity against that direful pestilence was known; *all*, whether rich or poor, natives or foreigners, learned or ignorant, without regard to age or sex, were as much exposed to *its* terrible assault, as the commanding General, his subordinates, and the rank and file, on the field of battle.

Such is the character of Asiatic Cholera, an anomaly in Medical history, confounding theories, disregarding what were considered as established laws, at variance with all precedents, inscrutable to the most enlightened and patient observers.

Is this the character of that form of Cholera which prevailed in this city in the month of December?

The large majority of the victims were unacclimated persons—immigrants recently arrived; citizens who might have transacted business for years in this city, but who were always absent during what is called the sickly season, and consequently unacclimated. Many negroes contracted the disease and died; but the climate of Louisiana is as obnoxious to negroes coming from other States as it is to the white race.

We are not aware that an individual born in the city, and who has permanently resided here, contracted, or died of the disease.

Some acclimated persons, however, fell victims to the Cholera; and so far as our knowledge of the facts extends, they forfeited their immunity by great imprudence and reckless neglect. In every instance where we have been able to ascertain the facts, there were premonitory symptoms acting from two to five days, during which time the disease could be easily controlled and arrested. In the stage of collapse little could be done; one here and there might escape against all hope and all calculation—these were exceptions to the general rule. And may not the same be said of all the diseases peculiar to our climate? They all end *when fatal* in a stage of collapse.

If, however, acclimated persons who were prudent, escaped an attack of the disease, they were nevertheless affected more or less by the peculiar state of the atmosphere. A looseness of the bowels was common—not amounting to diarrhœa, but rather characterized by one

of the distressing symptoms of dysentery, the *tenesmus* or bearing down of the rectum, attended with great disturbance of the hæmorrhoidal vessels. And it is remarkable how speedily this state of things was aggravated in bad, and ameliorated in good weather, precisely as was the epidemic.

From the report of the Metropolitan Sanatory Commission of England on the subject of Cholera, and published in the May number of the Medical Journal in 1848, many interesting facts and conclusions may be derived.

In the first place, the futility of quarantine. "Such were the measures at first proposed to protect the Country against the introduction and spread of Asiatic Cholera. The notorious failure of quarantine regulations, and the advent and fatal career of the Cholera, in spite of all the precautions adopted in 1831, by the Central Board of Health, have induced the Metropolitan Commission to turn their attention to the condition of the localities, in which Cholera first made its appearance, and which it was generally found to select."

We have the testimony of Drs. Russell and Barry who studied the history of the disease as it appeared in St. Petersburg; of Dr. Hamett who made a report on the Cholera in Dantzic; of Dr. Becker, of Berlin; of Dr. Automarchi in Warsaw; and we may add the concurrent testimony of nearly all the medical men who have investigated this singular disease that the first appearance of Cholera was in low marshy places, where it raged with the greatest virulence.

"The report of the Central Commission of Paris states that the disease first appeared and subsequently spread, above all other places, in the greater number of the *quartiers* situated upon the borders of the Seine; that it was most prevalent and most fatal in the low, close, undrained, and uncleansed localities."

There is scarcely an exception to this opinion of enlightened and intelligent physicians residing in both hemispheres.

If there exists a condition of things favorable to the developement and propagation of any disease, why cannot said disease originate in one place as well as another under the same circumstances?

In no part of the world could circumstances be found more favorable to generate pestilence, than what was evident in this city to every eye early in December."

We quote again,

"For all accounts from India agree in stating that it (Cholera) breaks out and principally prevails in low marshy situations and particularly near the banks of rivers; that whenever a village or military station lies upon or near low, marshy, or damp ground, the inhabitants suffer in direct proportion to their proximity to such a situation."

In this respect it bears a close relation to fevers arising from exhalations. In the campaigns in Flanders, and in Spain, as well as in India, the army surgeons have always traced fevers to such local cause.

All known facts go to prove that Cholera is not contagious or communicable from one person to another. In the second proposition of the Report already quoted, it is said "there is no evidence that Cholera spreads by the communication of the infected with the healthy. ***** While the manner of the invasion and extension of this disease thus

precludes all thought of its propagation by the communication of the infected with the healthy, there is another fact which is altogether irreconcilable with the notion of contagion, namely, that as no human means have succeeded in excluding it from particular spots, so no extent of communication with the sick has been able to carry it to other places."

On this point, amongst the European physicians, there appears to be no difference of opinion, and we fully concur in so sound a conclusion.

"The third proposition established in the report, is to the effect that Cholera observes in its progress the laws of ordinary epidemics, being influenced by the same physical conditions, and attacking similar classes of persons. These conditions may, according to the commissioners, be comprised in impure and humid air, and unsuitable or insufficient food and clothing, ill constructed dwellings, and defective appliances for the regulation of warmth, or protection against cold. The want of sufficient and proper food, it is argued with justice, is an agent of very inferior power to the habitual respiration of impure air; and it is justly affirmed that in the present state of most towns and cities, the number of persons whose constitution is enfeebled by want of food, compared with the number whose vital energy, is depressed by want of pure air, is found to be an exceedingly small minority."

According to the same Commissioners, whose frankness in sacrificing their preconceived opinions and prejudices to truth, is worthy of all commendation, "there is but one safeguard against Cholera, as against other diseases of the same class, viz: such sanatory arrangements as will secure the purity of the atmosphere, particularly by the immediate and complete removal of all filth and refuse, and that not only from the principal squares and thoroughfares, but also from the streets, courts and alleys of the lowest portion of the population."

If the health of this city is ever improved—if we expect to ward off Yellow Fever, Cholera, and other pestilential diseases, all such measures as are recommended above to purify the air, must be adopted and followed up with energy and perseverance.

We invoke the especial attention to the words "*complete removal, &c.*," which means any thing but what is now, and has been for several years the practice in this city—we mean the transferring of the filth and refuse of the city from the streets to be concentrated on the Levee, or the exposed bank of the river, or under the wharves.

We again recommend public floating bathing establishments on the plan of Mr. Bensick. In the business season, they might interfere too much with the shipping, and our commercial interest, but certainly a wharf might be spared by each Municipality for such purpose during the summer season.

We also urge the extension of water privileges, and we mean the water of the Mississippi river, to every part of the city. It may be necessary to charge for hydrants within houses, designed for the special benefit of families—but there should be at least a public and abundant supply of good, pure water, furnished by the city in neighbourhoods where the labouring classes congregate. Pumps would be the most convenient.

In conclusion, we do not affirm that the cholera of 1848 was imported or that it was not imported; but we must say that we have not yet seen the

shadow of proof of its importation. And moreover, we see no reason, why, under the peculiar circumstances of the case, cholera should not be engendered here as well as elsewhere; at least that form of it which prevailed in this City. It was little else than aggravated diarrhœa, which was fatal in a few hours after arriving at the stage of collapse, and analogous to our ordinary intermittents which occasionally pass at once into a similar stage of collapse, called congestive fever, cold plague etc.

It being established that cholera is not contagious, or transmissible from one person to another; also that the cause of it, whatever that may be, is carried through the air, visiting one place and avoiding another, bidding defiance to quarantines, and every human effort to arrest its progress; our whole attention should be directed to bring about that condition of things as speedily as possible, and regardless of expense, which every writer distinctly avers to be the only means of avoiding this dreadful scourge, or so modifying it, that it would no longer cause alarm and panic.

We say *regardless of expense*, because the city of New Orleans *owes every thing to commerce*; no article of export is produced or manufactured here; we receive, and ship to other markets, the produce and staples of the States washed by the waters or drained by the tributaries of our noble river. The prosperity and increase of the city must then ever depend on the extent of our commerce, and our commerce is intimately connected with the health of the city. Who can calculate the amount of produce diverted to other points, by the exaggerated reports that were borne on the wings of the wind from this city during the month of December to every part of the great valley? One tenth part of the profits lost in this way, might, if properly applied, have averted the stern visitation; and hundreds of men would not now be seeking employment in vain, nor would their families be wanting for the actual necessities of life.

This is a consideration that should interest every man engaged in business in this city, and every proprietor of real estate, for every epidemic, to a certain extent, cripples the former, and reduces in value the latter.

Since we commenced writing this report, the prediction we made, has become verified. The ship John Garrow has arrived from Liverpool after a boisterous passage of sixty five days with two hundred and fifty five steerage passengers, and the Secretary of the Board was compelled to send some twenty to thirty cases of ship fever to the Charity Hospital.

There is therefore assuredly an immediate necessity for establishing some temporary building as a house of recovery or branch of the Charity Hospital, for the reception of such cases, since from present appearances we have every reason to expect numerous arrivals of immigrants from England, Ireland and Germany.

Now is the time for enlightened and persevering action by our worthy Mayor and Municipal councils; and we feel assured, that whatever measures they may think proper to adopt, based on sound and well tested experience, and involving all necessary expenses, will be cheerfully sustained by an intelligent and liberal minded community.

Part Second.

REVIEWS AND NOTICES OF NEW WORKS.

1. *Bibliographical Notice; an Illustrated System of Human Anatomy, special, general and microscopic.* By SAMUEL GEORGE MORTON, M. D., Penn. & Edinb., Member of the Medical Societies of Philadelphia, New-York, Boston, Edinburgh and Stockholm; Author of "*Crania Americana*", "*Crania Ægyptica*", etc., with three hundred and ninety one engravings on wood. "*Oculis subjecta fidelibus.*" Royal 8vo, pp. 642. Philadelphia : Grigg, Elliot & Co. 1849.

Little did the medical profession expect another new work on Anatomy, seeing that very recently, a considerable number of original and foreign works, splendidly illustrated, had been issued from the medical press of this country. But, a careful examination will soon convince the medical student and the practitioner, that Morton's new, scientific, and artistic work, is an invaluable one; contains a great deal of information, particularly in relation to microscopic anatomy, not found in any work printed in this country. Philadelphia, the mother of medical schools, may well be proud of her Wistars, Horners and Mortons. This beautiful illustrated Anatomy is every way worthy of the justly celebrated and learned author of the *Crania Americana*. This latter work, together with his *Crania Ægyptica*, has, by universal consent, placed him, in this department of science, above every author, ancient or modern.

Dr. Morton's style as a scientific writer requires no comment, being chaste, perspicuous and concise. It would be difficult, perhaps impossible, to designate an American author whose works will compare with his, in artistical execution. His "*Crania Americana*", "*Crania Ægyptica*", "*Pulmonary Consumption*", and, now, his "*Anatomy*", are illustrated with surpassing magnificence. With respect to the latter, it may be said, that the nervous and vascular systems are not so largely illustrated as they might be, and doubtlessly will be, in the next edition. In the mean time, the student will find a good supply of excellent works on these topics, already published. Upon this subject, the author says, "that some divisions of the work are less fully illustrated than others, for it was found impossible, in a first edition, to do equal justice to them all. Thus attention has been more especially given to the central, rather than to the peripheral portion of the nervous system,

simply because the latter constitutes one of the most difficult problems of Anatomy and Physiology, and therefore demands all the light pictorial aid can bestow upon it. The microscopic characters of the several tissues are also delineated with extreme care from the best authorities; and to accomplish this object, I have been content to bestow less illustration upon those parts of the vascular system that possess least practical importance." (*Preface*, viii.)

This, compared with most works on Anatomy, presents a broad title, though it does not comprehend half of the anatomies that lie in the philosophies of nature. The dissector may know every muscle of the hand from the *Flexor sublimis perforatus* to the *Extensor minimi digiti* of the little finger,—every fanciful point of demonstration in the brain, from the *Thalami nervorum opticorum* to the *iter ad quantum ventriculorum*, without having dreamt of the vast expansions of philosophical anatomy.

Vesalius, Fallopius, and Eustachius, in a great degree founded special anatomy in modern times, though this honor has been awarded by some to Mondino or Mundinus, of Bologna, whose system of Anatomy, published in 1315, was probably founded chiefly on dissections, though it has been asserted that his information was derived from the fragmentary doctrines of Galen, as preserved by Arabian authors. At a latter era, great advances were made in anatomical science by the Hunters, Vicq-d'Azyr, Daubenton, Bichat, Cuvier and St. Hilaire, who illustrated and connected the last, with the present century, by an enduring track of light.

The present able work of Dr. Morton, treats of special, general and microscopic anatomy. Of the first variety, Dr. Morton says, "I need not be reminded that my enterprise must necessarily be devoid of that originality which gives charm to authorship; for a writer on Special Anatomy in our day must for the most part be content to clothe familiar facts in a new language. Nor is even this object always attainable in a path that has been trodden for centuries by ambitious and scrutinizing minds, who, like voyagers in search of undiscovered lands, have left comparatively little to reward the zeal of their successors. Not so however in Microscopic Anatomy, which presents a new field for discovery, replete with surprising and instructive revelations. It has already banished a legion of venerable conjectures, and substituted simple truth for vague hypothesis, and by unveiling the very germs of organized and organizable matter, has taught us the origin and development of tissues, together with a portion, at least, of their hitherto doubtful functions." The Hopeful school is the best; the sceptical the worst. What the microscope has already done, will be surpassed, no doubt, in time to come. It is now two hundred years since the Microscope was first applied to discover the minute anatomy of organized beings, by an Italian, the celebrated Malpighi.

Bichat's Treatise on the Membranes, and his subsequent works, developed that simple, but fruitful principle which seeks in all the organs, under every form, and in every position, the identity and relation of the elementary or primordial tissues—a principle which founded general anatomy in our own day and generation.

Several other Anatomies remain, beyond those here enumerated, as

Pathological Anatomy, Surgical Anatomy, Comparative Anatomy. It is believed that the materials are accumulating, nay already exist, for another variety—another department, namely that of Teleological rather than Physiological, or Physical Anatomy. Causes, the adaptation of means to ends, unity of plan in organisation, Morphology, Types, Analogues, Homologues, Psychical and Craniological phenomena, the Organic and Animal lives of Bichat, the doctrine of animal perfectibility of Lamarck, the doctrine of the conditions of existence of Cuvier, the nervous reflex action of Prochaska, the sensiferous and motiferous spinal roots of Alexander Walker, the general subject of Animal Forces, and of Vital Dynamism in all its forms as influenced by modern civilization, and forms of government—with many other kindred subjects both transcendental and experimental—these would supply an abundance of materials for a distinct, not incoherent treatise occupying a middle ground, upon which physiology and physics meet face to face, each reigning by terms, always modifying each others' action, often producing results altogether peculiar to the common offspring of both. Several curious problems in vital statistics, could be mentioned of a similar character. How is it, that all of the Baronetages in Great Britain uniformly and rapidly tend to *extinction*, especially in the male line of representations? How is it that the Representative form of Government in the United States, gives a ratio of vital progression, not found in any other? Let not monarchical Æsculapians attribute these questions to the plebian prejudices of Republican insolence. If the half of what the *monarchists themselves* admit be true, these questions should be almost paramount with every philosophical physician—for mere statesmen and statisticians have scarcely emitted a ray of light upon them. The physiological maledictions connected with the patrician orders in Europe, must have cause, whether it be anatomical, craniological, neurological, psychological or governmental. The following statements are all that can be admitted at present: The Westminster Review for April 1847, says, "that the male heirs of the peerages in England in a *great majority* of cases become *extinct*, and all the baronetages, become *extinct* for want of male heirs, though many of each have female representatives." Galignani, in his Guide to Paris, (1844), says *that nearly all the old Parisian families are extinct*—particularly the male portion. In the great City of Paris, *not one thousand* persons can reckon their ancestors as far back as Louis XIII.

But let us return to the more legitimate, and extensive subject of anatomy.

How is it possible for a single professor, in three or four months, by delivering from 3 to 6 lectures in a week, well and truly to teach half a dozen of anatomical sciences, not to name physiology, which is often included in his course?

Dr. Morton's "illustrated anatomy" is dedicated to *Medical Students*, in these words: "To Students of Medicine the following pages are most respectfully dedicated by the Author;" this ought to be amended, by adding every M. D., who practices medicine, and, who wishes to retain in his memory the exact and useful knowledge gained during years of toil and study in the dissecting room. The student little needs a book, until he gets into the dissecting room, and has the cadaver before him.

Ideas, derived solely from plates, cannot make an anatomist. Preconceived notions being inaccurate, are often disadvantageous, anterior to actual observation. It is when the student becomes a doctor, gets his diploma, throws away his dirty dissector, doffs his dissecting garments, and emerges from the oppressive atmosphere of the dead, to launch upon a stormy sea, that he will need such a book as much as Telemachus needed Mentor. Too many Æsculapians adopt Faust's logic: "I am not at home," says he, to a *dead* body; "I prefer the *living*, with plump forms and roseate cheeks." The student need not expect to retain in memory during life half of the knowledge acquired in the dissecting room without good books and plates of anatomy, unless, he continue the practice of dissection from time to time. Happily for the cause of science, the prejudice against dissection is in the most civilized parts of the world, rapidly vanishing before the progress of knowledge. Compare the present with the past. Was there ever a greater psychological revolution, developed in the progress of social physics? Great was the sensation in Italy, when Mundinus, in 1315, dissected two females, and, in the next year another. Du Bois, (latinized into Sylvius), born 1478, a Royal professor of Surgery at Paris, famed as an anatomist, and as the preceptor of Vesalius, never ventured to dissect *a single human body in his theatre*, but contented himself with teaching Galen and anatomizing dogs! How melancholy was the fate of his more adventurous pupil, who at first condemned to death by the Inquisition, was exiled from his native land, and perished miserably on a foreign shore, *for dissecting a dead man*, whose muscles *quivered or contracted*—a thing of almost constant occurrence, though the body may be decapitated, drawn and quartered. On the sixteenth day of January, eighteen hundred and forty nine, C. S., departed this life. His body was constantly watched for four hours; occasionally Doctor's McCormick and Smith were present, for considerable periods, and were deeply attentive—the former taking notes. A blank book, a pencil, two tested thermometers, and an accurate watch, were the instruments used for two hours. During nearly all this time, the dead man, as yet untouched by the knife, attempted to expel the thermometer from the rectum—the instrument moved or oscillated from side to side, forty eight times per minute; he performed countless motions; his underjaw moved—one side of the body underwent tremulous, convulsive and sometimes powerful motions and contractions which were soon after repeated by the other; he bent his arms, opened and clenched his fists. His fingers were bent so strongly that it was found difficult to open them during the paroxysms of contraction,—at other times, they were rigidly extended. Yet he was dead as the persecuted Vesalius now is—dead as his judges, the inquisitors, are.

Dr. Morton's Anatomy will doubtlessly be generally read and appreciated; it is therefore unnecessary to examine it in detail; indeed, the nature of the work allows not this kind of analysis. A few casual remarks may be allowed upon one or two topics arising incidentally while turning over the pages of this fine book.

The *index* is not very bad. Compared to other books on medical science, it is good. Among the best books extant, not one in fifty is well indexed. The French content themselves with a mere *table des*

matières. Every practical physician, in the habit of consulting books in the hurry of business, could a "tale unfold" expressive of vexatious disappointment, if not downright malediction, not very flattering to many an excellent author. Even dictionaries of science, arranged though they be, in the manner of indexes, that is alphabetically, ought, nevertheless, always to have indexes. Blessed be the man who invented indexes! He deserves immortality. The author of the *Dunciad*, while ridiculing this species of Bibliography, admits, nevertheless, its utility: he says—

*Index learning turns no student pale,
Yet holds the eel of Science by the tail.*

The "Illustrated Anatomy" of Dr. Morton has no Table of Contents. A fault this is. It were better that the overture to the play, the prelude to the concert, the programme to the procession, the bridemaid to the bride, should be dispensed with, rather than the table of contents to so good a book. The table of contents is an epitome—a concentrated analysis—an articulated skeleton of the whole work,—the opposite of the index, which decomposes the skeleton into its primordial elements, placing the palate-bones in company with the pubic—the anus with the *amygdalæ*,—by the way, what was our surprise on turning to our author's index, to discover that neither of these words could be found therein!

From Celsus to Morton, sixteen or eighteen centuries, anatomists have continued to assert that the jejunum, is as its name imports, *empty*: Ab ea jejunum intestinum incipit, non ita implicitum: cui tale vocabulum est, quia nunquam, quod accipit, continet; sed protinus in inferiores partes transmittit: (Celsus de Med. lib. iv. c. i.) This is an error, at least in Cadavera in New Orleans. The jejunum, if not a *plenum*, comes nearer to it than any other portion of the intestinal canal, being the principal recipient of chyle, bile, blood, black vomit, the choleric liquid, etc. This statement is countenanced by teleology. The end is indicated by the means—the purpose by the adaptations. The structure of this bowel, beyond all others, is unfavorable to the Celsian doctrine, namely, that it *retains nothing, but immediately transmits every thing to the inferior portion of the canal*,—because, it is the most capacious portion of the small intestine, and because it is furnished with an immense number of villousities, duplicatures, *valvulæ conniventes*, all of which, from their mechanism, must act as retarding causes, giving for every inch or foot of that intestine, a greater extent of mucous surface to be passed over, in a given distance, by alimentary or other matters, than any other intestine—a difference of, at least two to one. Dissectors know very well, how difficult it is even after freeing the bowels from their annexæ, and laying them out straight, to open the jejunum, except by scissors of a peculiar form, owing to these valvular obstructions and duplications; these latter, however, decrease on passing below this intestine. Nature is not addicted to platitudes, inadvertencies, and inutilities. It is probable, therefore, upon teleological principles, that she intended to make this intestine, including, of course, its duodenal appendage, a reservoir for the chymous masses delivered from the stomach, to be converted into chyle—to be absorbed, and assimilated. Hence the size, the retarding media, and fulness of the jejunum.

"The Mesenteric glands," (omitted in the Index) "are large and in great number. They lie between the laminæ of the mesentery, and commence near the intestine and increase in size as they approach the base of the mesentery." 478. A young, morbid anatomist would bless the name of Morton or any anatomist of the normal or healthy structures, if he would fix some probable or approximative standard of comparison as to the number, size, color, tenacity, vascularity, and consistence of these, and many other organs. "The spleen is a large gland situated in the left hypochondrium, of an irregular discoidal form and about three inches in diameter. Its external surface is convex and in contact with the diaphragm. Its external surface is concave," etc. 332. Omitting every element in the composition of the spleen, but its weight or size, the morbid anatomist would see but little change in the spleen of a *cholérique*—an experienced anatomist, would, probably, see none whatever, though it might be reduced one third or more in size. Dr. Cragie estimates the weight of this organ, at from six to fifteen ounces, (Ency. Brit. Anat.); Prof. Dunglison at half a pound—adopting the high authority of the latter, as nigh the truth, it will be found that a most remarkable change occurs in this organ, in cholera—a change that would not be thought of, perhaps, in the absence of some approximative standard. Dr. J. Bell, of Philadelphia, always able, always useful as a writer, has, in the *New System of Practice*, by himself and Dr. Stokes, a good summary on this organ.

"The bladder is a large sack lying behind the os pubis."—338. "*Function.*—The bladder is merely a receptacle for the urine."—342. This part of Dr. Morton's work, *Splanchnology*, appears, from the slight examination made of it, very interesting. The phrase, "a large sack," though correct in one natural condition of the bladder, is incorrect in another natural condition. When full, it is "a large sack"—when empty, it is scarcely a sack at all, being contracted into an almost solid mass! Hence, all the wonderment about that great lesion of the bladder, *contraction*, in *choléniques*, is but a "waste of sympathy," since it is the most natural thing in the world in cases where the urinary secretion is arrested.

The spirit which pervades this work is admirable. Full justice appears to be done, or at least intended, towards the numerous authors referred to by Dr. Morton. His ascription, however, of certain supposed discoveries in the nervous system, to Sir Charles Bell, though constantly and confidently proclaimed by British authorities, may be questioned. It has been shown in the *New Orleans Medical and Surgical Journal*,* that the priority of discovery is due to Alexander Walker,† whose publications preceded those of Bell, and who developed very clearly the fundamental doctrine ascribed to Bell, namely, the double function of sensation and motion as being due to separate nerves. Mr. Walker, as early as 1808, in one of his publications, says, "wherever a part having both *sensation* and *motion*, is supplied from one nervous trunk, that trunk envelopes both a nerve of *sensation* and one of *volition*. Their motions take place in *different* directions; the latter resemble the arteries, the former the veins," &c. The most that can be said in favor of Sir

* September and November Nos., 1847.

† Works on the Nervous System, London, 1834.

Charles Bell, in this particular, is that he attempted to invert the functions of what Mr. Walker called "the anterior fasciculi," &c., without adding anything to the fundamental conception which Mr. Walker had previously announced with unsurpassed clearness and force.

But Dr. Morton avoids controversy, or rather has no need for it, seeing that all the world admit his claims to originality in relation to the several subjects in which he has distinguished himself, and at the same time reflected honor on our happy country.

The poet Denham has well characterised all those controversies which aim at victory, not truth—

"The tree of knowledge, blasted by disputes,
Produces sapless leaves instead of fruits "

It is a pleasing spectacle to see, as in the case of Dr. Morton, disinterested and laborious researches appreciated, and genuine merit honored at home and abroad.

BENNET DOWLER.

II.—Clinical Midwifery—Comprising the histories of five hundred and forty-five cases of difficult, preternatural, and complicated labor, with commentaries. By ROBERT LEE, M. D., F. R. S., Fellow of the Royal College of Physicians, London; Physician to the British Lying-in-Hospital; Lecturer on Midwifery at St. George's Hospital. First American, from the second London edition; Philadelphia; Lee & Blanchard; 1849.

The reports embraced in this little volume, comprise the experience of fifteen years extensive practice of the author, both in private and hospital practice. The histories of the cases embodied in this work were recorded by the author at the time they were presented to his observation; hence their truthfulness and great practical value to the obstetrician. In his first report, Dr. Lee alludes to the striking difference which governs the practice of the physicians of Great Britain, France and Germany, in the management of instrumental labor; in some hospitals, the forceps are rarely used; in others, again, they are almost daily resorted to by men of equal skill and experience, and all professing to be governed by the same general principles of obstetrical medicine.

In Great Britain, the forceps, he says, is never used until the "uterus is fully dilated, and the head of the child has descended so low into the pelvis that an ear can be touched, and the relative position of the head to the pelvis accurately ascertained."

When the pelvis is distorted or deformed, and the soft parts rigid and unyielding—when the presentation is natural and the strength of the woman adequate to expel the child, the forceps should not be applied.

But if hemorrhage should complicate the labor, producing exhaustion—if convulsions should supervene or other accidents, jeopardizing the life either of the mother or child, or both, then the forceps, in the hands of a skillful operator, may be of the greatest use

in hastening the delivery of the woman. Be it remembered that such instruments are only to be employed in accomplishing that which the uterus seems unable to bring about without such help. Dr. Lee mentions several authors who advised the introduction of the long forceps, even while the head of the child remains above the superior aperture of the pelvis.

Our author details the particulars of eighty cases of difficult parturition, in which the forceps was employed. His report of these cases will be found to possess more than usual interest for the obstretical practitioner.

Dr. Lee gives us a comparative view of the frequency of forceps and craniotomy-cases in ten lying-in-hospitals. Of ten thousand one hundred and ninety-nine cases of labor, attended by Dr. Clarke in the Dublin Hospital, the forceps were used in only fourteen cases, and craniotomy in forty-nine instances; thus making one in seven hundred and twenty-eight cases for the forceps, and one in two hundred and forty-eight for craniotomy.

Baudolocque applied the forceps to one in five hundred and sixty one cases, and craniotomy to one in two thousand eight hundred and ninety-eight. Boer, of Vienna, used the forceps in the proportion of one to two hundred and seventy-four, and craniotomy in one to seven hundred and thirty-seven. Carus, at Dresden, applied the first instrument, forceps, to one in fourteen, craniotomy to one in two hundred and eighty-three. Siebold, of Berlin, to one in seven, and craniotomy to one in two hundred and ninety-three!

The above statistical facts are well calculated to startle the honest practitioner, and to shake his faith in the principles of operative midwifery.

By what rule of action or principles of science, it may be asked, were these distinguished accoucheurs governed in conducting so many patients through the perils of labor? If the established principles of our science be the same everywhere and of universal application, it follows from the above showing that obstretical science, either has no fixed principles, or that those who practice this branch of medicine, differ widely in their mode of applying them; or shall we assume that nature is partial in her operations, granting an easy delivery to the females of one language or country, and entailing upon those of another the "original curse," aggravated by the intervention of art.

Dr. Lee proceeds then to give succinct accounts of the eighty cases of labor in which it was deemed necessary to resort to the use of the forceps.

We need not say that he is high authority in obstretical medicine, as his works on this branch of science are received both in this country and in Europe with commendation.

The first reported is so interesting and instructive, and is so full of warning, that we copy it entire—

Case 1.—On the 28th of June, 1823, I was present at the delivery of a woman, æt. thirty, who had been in labor nearly three days and nights, under the care of a midwife. It was the first child. The orifice of the uterus was not fully dilated, and it was very rigid. The vagina swollen and tender, the abdomen tense and painful on pressure. Tongue loaded; urgent thirst; counten-

ance flushed ; pulse rapid and feeble. The labor-pains, for ten or twelve hours, had been gradually becoming more feeble and irregular. The head of the child was strongly compressed and much swollen, and the greater part of it was above the brim of the pelvis. An ear could not be felt, and the hollow of the sacrum was empty. It was determined by the practitioner who had charge of the case to attempt to deliver with the long forceps, and he observed before proceeding to introduce the blades, that it was a case in which the superiority of the long over the short forceps would be observed in a striking manner ; and that in less than a quarter of an hour the delivery would be safely and easily completed, and the life of the child preserved. The blades of the forceps were, however, introduced with great difficulty, and still greater was experienced in getting them to lock. Strong traction was then made for several minutes, and the blades slipped off the head. They were re-introduced and the efforts to extract renewed, and continued till the instrument again slipped off. This happened several times, but the attempt to deliver with the long forceps was not abandoned till the operator was exhausted with fatigue. The head was then perforated, and extracted with the crotchet. Violent inflammation and sloughing of the vagina followed, and about three weeks after delivery it was ascertained that a large vesico-vaginal fistula existed. She was abandoned by her husband, and was afterwards reduced, in consequence of this misfortune, to the greatest possible misery. This was the first time I ever saw the forceps applied in actual practice, and I was struck with the vast difference which exists between the application of the forceps to the head of an artificial fœtus put into a phantom, and the head of a living child. I was led to suspect, from what I now witnessed, that a dangerous degree of boldness and hardihood might readily be acquired by long practice upon a phantom, where this was not combined with attendance on cases of difficult labor. The unfortunate termination of this case made me resolve carefully to watch the progress and termination of all the cases of difficult labor which I could meet with, and preserve accurate histories of them, which has been done to the present time.

The second case was similar to the preceding, and therefore we omit reporting it.

We have taken some pains to ascertain the result of the eighty cases of instrumental labor reported by Dr. Lee, and must say that his experience is by no means calculated to inspire us with any great degree of faith in the practice of resorting to instruments in cases of tedious and difficult parturition. Of the eighty cases in which instruments were used, *eleven* proved fatal to both mother and child ; in three instances the mother alone died, and in forty-two the child perished ! This is certainly a high rate of mortality ; even admitting all the cases to have been of a serious and difficult nature. About one half of the mothers who survived this instrumental delivery, suffered to a greater or less extent from the mechanical violence necessary to effect delivery. Many of them had sloughing of the soft parts, secondary fevers, &c., and not a few, vesico-vaginal fistulas, rupture of the perineum and its attendant evils. In some instances, we are told by the author, that the cicatrices, consequent upon the sloughing of the soft parts, were so extensive that the vagina became nearly obliterated, requiring the knife and dilatation to re-open the passage to the *os uteri*.

With these facts before us, it may be rationally demanded, what would have been the result had these eighty cases been left to the efforts of nature, aided only by such therapeutic means as the circumstances of each required ? We are free to confess, that with our limited experience in this branch of medicine, and our unbounded confi-

dence in the "*vis medicatrix naturæ*," we have but little doubt that the success would have been equally, if not more gratifying, to the practitioner, if instruments had not been resorted to in many of these cases.

We are moreover informed that in several instances, when the forceps was applied and the head could not be brought down, the instrument was withdrawn, and the case being left to nature, labor took place without assistance and both mother and child survived.

The above facts should warn the young accoucheur to be cautious in the use of instruments in obstetrical practice; much must be left to the natural resources of the woman; in no case should art interfere without the clearest indications, without the most satisfactory assurance that the uterine contractions are unable to expel the child.

If parturition be a natural, a physiological process, (and why not?) surely we should pause before we attempt to wrest the work from the hands of nature and subject it to the uncertain laws and principles established by man.

There have not been wanting writers of celebrity, who condemn every thing like the use of instruments in midwifery; we are not prepared to go so far, but we do believe that the forceps, perforator etc, are often resorted to without due consideration, and not unfrequently to the serious injury of both the mother and child.

Patience, says a distinguished writer, is more useful to the practitioner than skill in the use of obstetrical instruments.

Time may accomplish more than force. We once attended a young female who was in labor with her first child for five nights and four days; we had but little faith and less skill, we are free to confess, in the application of instruments; we, at a proper time, gave ergot and the delivery was accomplished with safety to both mother and child.

As far as we are informed, instruments are but little used in this section of the country in midwifery practice; at least but few instances have come to our knowledge.

Dr. Lee's "Second Report," *embraces difficult labours from distortion of the pelvis, swelling of the soft parts, convulsions, hydrocephalus in the fœtus, and other causes, in which delivery was effected by the operation of craniotomy.*" This report contains ninety-six cases of labor, all of which were rendered difficult by one or more of the above causes. We can not go into details, but they will be found interesting, and we refer the reader to the work.

From the above number of difficult labors, we see that life is not only precarious after birth, but that in the very act of entering the "great theatre," a number of morbid causes assail us on every hand, and death disputes our passage to the light of day.

The "Third Report" treats of the *induction of premature labor in cases of distention of the pelvis—cancer of the gravid uterus—uterine and ovarian tumours—organic diseases—dropsy of the ovary—obstinate vomitings—hemorrhage from the bowels—chorea and insanity during pregnancy.* The third report of our author will, it may be seen, discuss a number of problems in midwifery, so difficult of solution. This "report" furnishes the history of seventy-nine cases of complicated labor; these added to those included in the two previous reports, will give us some idea of the difficulties and dangers that attend the parturient

state. Thus we see that disease assails us even in *utero*, hence the great mortality in the infantile state—a state in which we are tenacious of life, yet the deleterious agents which surround, and act upon us, are multiplied in proportion to the imperfect development of our organs.

The “Fourth Report” of our author includes the *histories of one hundred and one cases of preternatural labors*. Every variety of presentation is given in these observations, and the best method of managing each particular case is clearly and succinctly laid down. These observations are full of practical instruction, and should be carefully examined by the practitioner.

In his *Fifth Report*, are reported the histories of sixty-two cases of placental presentations. We have looked over these cases, and find nothing new or striking in the practice. Dr. Lee, when he finds it practicable, introduces his hand, ruptures the membranes if entire, and turns and delivers with the least possible delay. This, we believe, is the practice with the best accoucheurs of the day. In some cases it is only necessary to rupture the membranes, and delivery is soon accomplished without further assistance.

His “*Sixth Report*” contains the *histories of forty-four cases of uterine hemorrhage in the latter months of pregnancy—during labor, and after parturition*. If hemorrhage came on during pregnancy, and the usual therapeutic means, such as cold applications, the horizontal position, venesection, &c., failed to check it, Dr. Lee at once brought on labor, and in this way, generally arrested the hemorrhage. In other cases, viz: such as hemorrhage during labor, he acted as already pointed out. He employs pressure, cold applications and the tampon to arrest hemorrhage after the expulsion of the contents of the uterus. He reports some cases of apoplexy of the placenta, as causing the death of the foetus in utero—such instances are rare in practice.

In his “*Seventh Report*” Dr. Lee gives an account of thirty-five cases of retention of the placenta. We shall report in a few words, his treatment of such cases. His first effort is to introduce the hand, if the os uteri is sufficiently open to allow this operation to be performed, and to bring away the retained placenta. In some cases he administers ergot; this practice, from his report, did not seem to succeed very well. He does not seem to have tried the effects of large doses of anodyne medicines in these cases; still he mentions a few that came under his care, to whom opium had been administered before he was consulted, and we observe that the placenta was readily extracted. This fact, however, did not seem to have attracted the attention of our author—strange that it did not! In the report of the cases under consideration, retained placenta seem to have been marked by unusual fatality, according to the showing of our author. We have seen several cases of retention of the placenta, and although some secondary fever, with headache, rigors, etc., followed after a few days, still but few were attended with other serious results. The following is the treatment of Dr. L. in cases of retained placenta, soon after the birth of the child: “The best method of preventing retention of the placenta is to apply the binder immediately after the birth of the child—to make pressure with the hand over the fundus uteri at short intervals, and slight traction upon the cord downward and backward in the direction of the

hollow of the sacrum. By these means the upper part of the uterus usually goes on contracting till the placenta is detached, and pressed down through the os uteri into the vagina. In all cases, whatever the cause of the retention may be, if the placenta at the end of an hour is not detached from the uterus, and expelled, it should be withdrawn artificially. The difficulty of removing the whole or portions of the placenta, adhering with more than natural firmness to the uterus, or retained by contraction of the cervix, is only increased by delaying to interfere after an hour has elapsed from the delivery of the child."

We leave it with the experienced accoucher to decide how far the above advice may be correct and sound. It is difficult to lay down any set of rules to guide us in these cases; generally it is best, we conceive, to remove the placenta as soon after the birth of the child as practicable, not only because it relieves the mind of the patient and her friends, but likewise removes from the attendant's mind all fear of hemorrhage and its dangers.

In his "*Eighth Report*" Dr. Lee furnishes us with the *histories of forty-eight cases of puerperal convulsions*. The details of these forty-eight cases of convulsions are exceedingly interesting, and are entitled, from their practical value, to the serious study of the profession.

It is impossible to recapitulate the substance of these reports in a short paper like the present; suffice it to say, that to arrest the convulsions, artificial delivery was resorted to in some cases—venesection, cups, leeches, blisters, cathartics, calomel, cold applications, etc., etc., in others, especially where the brain and the other nervous centres seemed oppressed with blood, enemata, both cathartic and stimulant, were freely employed, and the antiphlogistic plan of treatment adopted in the great majority of cases. After death, the brain was found congested and its blood vessels turgid; in some cases, the ventricles were filled with serum. Such are some of the practical precepts put forth in this little volume. The book is entirely clinical and abounds in useful information, and Dr. Lee has rendered immense service to obstetrical medicine by publishing such a large amount of practical matter.

It may be had at Steel's book store, 14 Camp street.

III.—*The Principles and Practice of Modern Surgery*. By ROBERT DRUITT, Fellow of the Royal College of Surgeons. A new American, from the last and improved London Edition. Edited by F. W. SARGENT, M. D., Author of *Minor Surgery*. Illustrated with 193 Wood Engravings. Philadelphia. Lea and Blanchard. 1848.

This, we believe, is the second American edition of the excellent work of Mr. Drutt; no trifling evidence of its intrinsic merit. As most of our readers are, we hope, well acquainted with the valuable facts, contained in this volume, we shall not attempt to analyse the work.

The book is divided into five parts; the two first treat of the principles, and the three last of the practice, of surgery. Under the first and second parts, he treats of the "*constitutional effects of local injury and disease, and of the principal processes of local disease.*"

Part the *Third* treats "of the different species of injuries;" part the *Fourth* "of injuries and surgical diseases of various tissues, organs and regions;" and in the *Fifth* parts "of the operations of surgery." This division of the subject is simple and natural, and affords strong evidence of the practical character of the work.

As our main object is to furnish the readers of the Journal all the practical information we may possess, we propose to give the views of Mr. Druitt on Gonorrhœa, a disease of frequent occurrence and often difficult to manage. Our author calls it variously, *gonorrhœa virulenta*; *blenorragia*; *urethritis*. He defines it a discharge from the mucous membrane of the male or female genitals; generally, says Mr. Druitt, produced by contagion from a similar discharge during sexual connexion.—We pass over his description of the symptoms of the disease, as those who may be the most interested in this account, are doubtless familiar with its various stages.

If the inflammation extends far up the urethra, we shall have much pain in the perineum, and spasm of the *accelerators* during micturition, interrupting the stream of urine, and producing much suffering. We omit to notice the various stages of gonorrhœa, described by the author. Every discharge from the urethral or vaginal mucous surface is not necessarily of a venereal character; sometimes it proceeds from local irritation; sometimes from sexual indulgence; violence and blows in the perineum; again it may be of a constitutional origin, not emanating from any contamination. This discharge may precede, accompany or follow an attack of *gout* or *rheumatism*. It is sometimes sympathetic of other diseases, and will cease when such affection is cured. Some medicines, as guaiacum and cayenne pepper, are said by our author, to excite in some constitutions, an urethral discharge. Mr. Druitt states "that a man may contract a pretty severe discharge from a woman, who is perfectly chaste, and has not been previously infected by a third party." To have connexion with a woman during, or soon after, the menstrual discharge, may give rise to urethritis, attended with chordee, orchitis, etc.—The same result may follow from a leucorrhœal discharge, as we have more than once witnessed. Our author next enters upon a very interesting question, viz: the *diagnosis* of the disease. He declares, that we have no certain means of distinguishing *simple gonorrhœa* or *urethritis*, from the *genuine venereal gonorrhœa* or *clap*.

He declares the disease of the urethra, however produced, is the same in its nature—the same in its symptoms, and requires the same plan of treatment.—There is no danger of communicating the disease, (genuine venereal gonorrhœa) from one party to another, previous to the appearance of the discharge; so in like manner, after the running ceases, no fears need be entertained of imparting the contagion.—Mr. Druitt recommends the following *prophylactic treatment* to avoid contracting the disease. "Immediately after a suspicious connexion, it will be prudent to make water, so as to cleanse the urethra, and then perform a thorough ablution with soap and water. If the patient is subject to gonorrhœa, it will be well, to wash out the front part of the urethra with a syringe of some astringent lotion; and, if any fissures or excoriations be perceived, they should be immediately touched with lunar caustic and dry lint applied."—We have much faith, in a combination of some of the

aromatic with the astringent tinctures, in preventing the communication of this disease from one person to an other, particularly in syphilitic affections.—We have recommended equal parts of the Tinct. of gentian—of myrrh—of rhatany—and of cinnamon—to be used, as a wash, immediately after an impure coition and we have been assured with perfect success. This point, however, is difficult to decide, as the party could not say positively whether the other party was or was not tainted.

Treatment. In the first stage of the disease, "*before acute symptoms have come on,*" Mr. Druitt advises injections of two grains of the nitrate of silver to eight ounces of distilled water, to be repeated every 4 hours; this to be followed by some astringent injections, such as the sulphate of zinc, or alum. During this time, the patient should take a mild purgative, and afterwards copaiba, or cubebs, three times daily; avoiding at the same time fermented liquors, coffee, salt, spice and all stimulants. He also advises enveloping the penis in a piece of lint, saturated with some refrigerant lotion. We object to the treatment advised by Dr. Druitt: In the first place, we would advise at least 10 or 20 grains of the nitrate of silver, to the ounce of water as an injection. This is our usual practice and in the great majority of cases, it will arrest the discharge in the very commencement of the disease. This plan must be enforced in the forming stage of the disease—later, it will but aggravate the inflammatory symptoms of the disease and add to the sufferings of the patient.—We have found the following mode of treatment to succeed in many cases: Take of the nitrate of silver two scruples, cerate simple, four drachms, extract of conium^{or} belladonna, one scruple, mix and with this ointment cover a common sized bougie, say No. 6–8, and introduce it into the urethra to the depth of five or six inches, let it remain there three or four minutes, or until a burning sensation is felt in the part. then withdraw the bougie, pressing at the same time the wall of the urethra against the instrument; by this means a portion of the ointment will be retained in contact with the diseased mucous surface and act beneficially upon the morbid part. This operation may be repeated two or three times daily for as many days. It may seem to aggravate the inflammatory symptoms, at first; these will, however, subside of themselves, and some simple diluent drinks, and slightly astringent injections, will accomplish a cure in a few days.

The above plan of treatment, in uncomplicated cases, and especially when the inflammation does not run too high, or extend far back towards, or even up, to the neck of the bladder, will, we feel quite confident, effect a cure, if resorted to in the early stages of the disease.

The remedies usually prescribed and taken for a gonorrhœa, often inflict serious injury upon the stomach, producing all the distressing symptoms of dyspepsia. Hence the balsamics should be given but a few days at a time, then suspended or incorporated with some mild tonic extract, or washed down with some bitter infusion.

The last edition put forth by the author, contains important and valuable additions on several subjects, but partially touched upon in the preceding editions.—The American Editor, Dr. Sargent, has not failed to slip in a word of advice, from time to time, when a suitable opportunity presented itself. The book is none the worse for his annotations as they are always practical and judicious.

The engravings are not worthy the surgical facts, which they are intended to illustrate. They might be dispensed with, and the student would not be the less wise on that account.—In conclusion, we believe we hazard nothing, in saying, that *Druitt's Surgery* is the best book on that subject, in the English Language. It is for sale at Mr. White's Book Store, Canal street.

IV.—*Report of the Surgeon General, (DR. LAWSON), to the Secretary of War, at the opening of the second session of the 30th Congress, Washington, 1848—9.*

This is a meagre account of the medical department of our gallant army operating in Mexico. We shall no doubt have hereafter a full account of the achievements, (surgical &c.,) of this important arm of our service. Surgeon General Lawson knows too well the value of army medical statistics to withhold them from the profession; he is aware, too, with what avidity and satisfaction such authentic information would be received by the medical public.

We shall await with impatience a full account of the diseases that assailed our heroic little army—of the influence of the Mexican climate upon *American* constitutions, of the effects of this climate upon the wounded, embracing meteorological observations and such other information as the nature of the service, in a foreign country, might be expected to furnish.

All this, with much more, we feel satisfied, will be published in due time, under the supervision of our excellent Surgeon General. From the report of the surgeon in chief, we learn that the total amount of funds appropriated up to the 29th of March 1848, for the medical department of the army, amounted to two hundred and fifty three thousand four hundred and eighty dollars and thirty six cents. Of this sum there has been expended on account of pay, and other claims of private

physicians, ...	\$23,433 55
On account of medical supplies &c.	146,902 12
Leaving in the hands of disbursing agents	10,510 00
And in the treasury of the U. S.	72,634 69

Total \$ 253,480 36

“During the war with Mexico, medical and hospital supplies of every kind, have been, as far as practicable, regularly furnished to the army at home and abroad but at an extraordinarily increased ratio of expense.” (“true.”)

The expenditures for medicines, surgical instruments, hospital stores, bedding, &c., &c., from the 30th of June, 1847, to the 30th of June, 1848, the close of the fiscal year, amount to, as already stated, the sum of \$146,902 12; and judging from the large number of claims daily coming in upon us *since*, some of them of more than doubtful legitimacy, we shall have to pay many thousand dollars more, before we can get rid of the responsibilities imposed

upon us by other persons than the agents of the medical department of the army. Ample supplies of medical stores were regularly provided by the medical purveyors at New York and New Orleans, and forwarded to the main depots of the several army corps operating against Mexico. A large amount of these stores, however, were lost at sea, (\$14,398 worth in one vessel, for instance,) while other portions were damaged or destroyed on the coast, in the transfer from the shipping to the shore.

Again, in consequence of the want of transportation, or the limited means of transportation furnished, added to the interrupted communication between the troops in the field, and the depots on the seaboard, no inconsiderable amount of the medical supplies were lost, damaged, or destroyed on the road.

These heavy losses at sea and by land, together with the waste of the stores among new troops, and those under a constant change of position, brought about the necessity of purchasing medicines, &c., in the enemy's country, at very extravagant prices, thereby swelling the expenditures of the medical department of the army greatly beyond the sum which would have met the requirements of the service under ordinary circumstances.

The amount of claims of private physicians, settled up to the close of the last fiscal year, is pretty considerable; but this, I apprehend, is only a moiety of what is to be paid, for physicians were employed by almost every independent military commander at will, until all responsibility ceased, or control over the matter was lost.

The existence of war brings about a forced state of things, and leads to abuses of privilege and of power, and extravagancies of every kind, which cannot always be resisted or prevented; so that we shall have to set down the excess of our ordinarily limited disbursements with the heavier expenditures of other branches of the military service, as the unavoidable results of a state of war.

I essayed to prepare, as usual, a tabular abstract from the quarterly sick reports received at this office; but finding that the returns from many of the corps were so imperfect that no reliable statistical information could be derived from them, I had reluctantly to abandon the idea of presenting a report upon the sickness and mortality of the army during the past year.

Since the last annual report, two medical boards for the examination of applicants for appointment to the medical staff of the army, have been convened in the City of New York; the first on the 27th of October, 1847, and the second on the 1st of May, 1848.

By the last of these boards, Assistant Surgeon J. W. Russell was examined; and having fully come up to the standard of professional merit required, he was accordingly recommended for promotion.

Before the two boards, one hundred and thirty eight candidates were authorised to present themselves for examination, forty-eight of whom only reported to the board.

Of the number who reported, ten withdrew, two were found to be physically disqualified, and thirty-six underwent the examination; and of these last, ten were approved and recommended for appointment.

From the approved candidates, four were regularly taken up as vacancies occurred; when the act of Congress, approved July 19th, 1848, prohibited any further vacancies being filled in the medical department of the army.

Since the passage of that act, one Surgeon and two assistant surgeons have died, and three assistant surgeons have resigned,—leaving the army without the necessary services of five medical officers.

I should be wanting in my duty to the army and the government, were I not to say here, that the two surgeons and twelve assistant surgeons, authorised by the act of Congress of the 11th of February, 1847, as additional to the medical staff of the army, are essential to the good of the service.

Previously to the commencement of hostilities with Mexico, the number of forts and other military stations occupied by troops was fifty-six, (a few of them requiring the services of two physicians,) and these were provided with medi-

cal aid from a corps consisting of twenty surgeons and fifty assistant surgeons.

At the present time, the military posts already occupied by troops, and those about to be established in Texas, New Mexico, Oregon, and California (some of them requiring two or three medical officers, perhaps,) will reach the number of eighty-five or more; all of which will have to be provided with medical aid from the same number of medical officers as before, if the proviso to the third section of the act of Congress of the 19th of July, 1848, is continued in force.

That the medical staff of the army, consisting of twenty surgeons and fifty assistant surgeons, were insufficient to furnish the necessary medical aid to the troops occupying but fifty-six posts, may be inferred from the number of private physicians who were then employed; and if the seventy surgeons and assistant-surgeons were inadequate to the wants of the army at that time, it is manifest that the same number of medical officers cannot meet the requirements of the service now, when the military force is to be spread over a vast extent of country, occupying thirty-five or more new stations.

Under the conviction, then, that the medical staff of the army cannot be reduced below the number of twenty-two surgeons and sixty-two assistant surgeons, without manifest injury to the service, I do most earnestly recommend that the proviso to the third section of the act of Congress, approved 19th of July, 1848, be, as far as the medical department is concerned, repealed.

Almost all the officers of the medical staff, whose services were not essential at other points, have been employed with the several army corps operating against Mexico; and whether serving in hospitals, at depots, or with troops in the field, they invariably acquitted themselves faithfully of all their obligations to the army and to the government.

The surgeons and assistant surgeons, immediately attached to corps in the field, not only bore their full measure of the toils and the privations, but also participated largely in the dangers of the service; for in each and every conflict they were exposed to the shafts of the foe—the same as were their brethren in arms of the line of the army.

All of which is respectfully submitted.

THOMAS LAWSON,
Surgeon General.

Hon. WM. L. MARCY,
Secretary of War.

From the foregoing statement, we learn that the medical staff of the army was inadequate to its wants; numbers of our brave citizen-soldiers unaccustomed to camp duty and the discipline of a military life, perished for want of timely medical aid.

It is well known that raw recruits, volunteers, who are suddenly summoned to take the field in defence of their country's rights, are far more liable to the diseases, incident to such service, than the well drilled and thoroughly disciplined soldiers of the regular line; hence the great necessity of an additional supply to the medical staff, when such recruits are called into service. In the regular army, one surgeon and an assistant, may be quite sufficient for a regiment of one thousand men; but with the same number of volunteers, there should be added at least one assistant, making with the full surgeon, three medical officers. We speak from a little personal experience in this matter. In such service as our army was called upon to perform in Mexico, an assistant surgeon should have been assigned to every two companies of men, with a full surgeon to every five hundred men. Such was, however, far from being the case. It is notorious that the medical staff was wholly inadequate to the wants of the army; hence the great amount

of suffering and death that marked every step of our little army through the enemy's country. A miserable spirit of economy, of parsimony, shall we call it, crept into the army *Bureau* which was destined to entail upon the health of our volunteers the most disastrous consequences. In every branch of the service, except the medical, every thing was well appointed, well supplied. All honour is due those surgeons who acted with our troops both in the hospitals and in the field; well did they discharge their perilous duties; nobly did they confront the dangers that surrounded them; bravely did they face the shafts of the foe, when duty summoned them to the "ensanguined field." But there must be a limit to human exertion, if not to human will; they could not be everywhere; they could not achieve impossibilities; they performed all that chivalry, united with science and skill, could accomplish. More, their country could not, did not expect of them. Enough of this. The Surgeon General, it will be seen in the above "report" to Secretary Marcy, speaks in flattering terms of the conduct and bearing of the medical corps of the army.

He declares "they bore their full measure of the toils and privations, as well as participated largely in the dangers of the service," being exposed in every conflict to the "shafts of the foe, the same as were their brethren in arms of the line of the army."

For this exposure and these privations, what has been their reward? Has a single one been brevetted, or received even a smile of approbation from the heads of Department? For the most laborious, the most responsible duties, well and ably performed, not one, to our knowledge, has received a crumb from the executive table; whilst hundreds of the line of the army have been promoted for their gallant services in the field. For this we do not complain, such won their honours, and are justly entitled to them. But why proscribe the medical corps, when Quartermasters, and others, who never faced the foe, are brevetted and receive the flattering commendations of the commanding General in his dispatches to the war office?

We thank the Surgeon General for his report.

IV. *Report of the Sanatory Committee of the Board of Health of the City of New York on the subject of Asiatic Cholera, which prevailed at Staten-Island, (the Quarantine Station) 1848.*

This report, made by Dr. A. B. Whiting, Health Officer of the Port of New York, to the Board of Health of the same city, embraces the history, progress and symptoms of all the cases of Asiatic Cholera that occurred at the Quarantine Establishment in Staten Island.—In this report, Dr. Whiting gives the following account of the ship New-York and the sickness on board:

"On the second of December, the packet-ship New-York arrived at Quarantine with a number of persons sick, having lost seven during the last week of her voyage, with a disease that has since proved to be Asiatic Cholera. The New-York left Havre on the 9th of November, with three hundred and thirty one steerage-passengers, twenty one cabin, and thirty three crew; a total of 385.—All continued well until

the 25th, Saturday, when one steerage-passenger—a German, aged 29 years, in robust health, was attacked with vomiting and purging, accompanied with cramps of the upper and lower extremities. The Captain supposed it to be Cholera Morbus and prescribed judiciously for the symptoms, but they continued until the third day, when he died.”—

The next case was attacked in the same manner; and with like symptoms, such as purging, vomiting, cramps, cold surface and the like; several others sickened and died.—In two days after the vessel reached Quarantine, 12 new cases of the disease occurred. Up to the time Dr. Whiting made out this report, 63 cases of Cholera had been reported, and of this number twenty nine had died.

The author of the report informs us, that but very few of these attacks were preceded by diarrhœa.—“A majority were attacked between two and eight o’clock A. M.”

We are further told that Capt. Lines, the commander of the ship, did not notice, in the cases that occurred on board his vessel, before he reached Quarantine, the “peculiar character of the evacuations” (rice-water discharges does the author mean.)

Dr. Whiting then gives a very correct description of the most striking symptoms of the disease, corresponding with those that marked it in this city, during its epidemicity.

In the treatment of the disease, he found scruple doses of calomel, combined with opium and camphor, to be the most serviceable. He followed up this plan by small doses of calomel, repeated every three or four hours, until “reaction was indicated by some action of the liver.” We have succeeded in curing some alarming cases, by throwing in *one drachm* of calomel, combined with camphor, quinine and occasionally with some preparation of opium; and this amount of calomel was given in the course of a few hours.—Trifling salivation was induced in two cases thus treated.

Dr. Whiting did not essay the lancet; and we regret this the more, as he informs us many of his cases were in robust health up to the very moment of the attack, when it is presumed that they might have borne the loss of blood, if not with advantage, at least, without serious injury.

Again, bloodletting, we are told by its advocates, is almost the only thing that will produce reaction,—quicken the circulation, and by unloading the engorged organs, reanimate the vital forces,—so completely overpowered in the collapse of cholera.—

The post mortem appearances, reported by Dr. W., correspond, in many particulars, with those observed in New Orleans.—The report is sensible, temperate in its tone, and altogether satisfactory.—

VI.—*A Text Book of Practical Anatomy.* By ROBERT HARRISON, M. D., M. R. I. A., Fellow of the Royal College of Surgeons of Ireland and England; Professor of Anatomy and Surgery of the University of Dublin, and one of the Surgeons of Jervis street Infirmary, etc., etc. With additions, by an American Physician. With numerous illustrations. New York; S. S. & W. Wood, 1848; pp. 720.

This is the old "Dublin Dissector," first published in 1827, now metamorphosed, "*nomine mutando*" into a system of "Practical Anatomy," by an American physician.

Pray, what right has this American physician to christen the offspring of Dr. Harrison. If names be things, then the book is no longer the same. The Dublin Dissector, *alias* the Practical Anatomy of Professor Harrison, has for years been the constant and inseparable companion of the American medical student in the dissecting room. It has formed the basis of a number of anatomical works since its first publication, and may be regarded, for its simplicity and arrangement, as a model for all books on the same subject.

The illustrations, although not remarkable for their elegant execution, yet they may assist the memory and convey some idea of the anatomical structure of the organs.

The work is printed on good paper and with excellent type; in a word, it is beautifully "got up," and bound in handsome style. It is one of the neatest volumes that has been laid on our table for a twelve-month.

We recommend the work to all who wish to learn anatomy—the foundation of medical science. We are indebted to Mr. White, Canal street, for a copy.

VII.—*Medical Lexicon. A Dictionary of Medical Science; containing a concise explanation of the various subjects and terms; with their French and other synonymes; notices of climate and of celebrated mineral waters; formulæ for various officinal and empirical preparations, etc., etc.* By ROBLEY DUNGLISON, M. D.; Professor of the Institutes of Medicine, etc., in Jefferson Medical College; carefully revised and greatly enlarged; Philadelphia; Lee & Blanchard; 1848.

This lexicon embraces over 900 pages, and contains a vast amount of valuable information, much of which is scarcely to be found in any other book.

Between six and seven thousand new terms have been introduced into this edition, not mentioned in any of the former publications.

Besides, about one hundred pages have been added to the work—hence it would appear, that the author is determined to bring it up to the wants—to the actual state of the profession. Professor Dunglison's learning and industry have long since secured to him deserved celebrity, both in this country and in Europe, as an able lexicographer. The work is indispensable to the student and practitioner. The book may be obtained of Mr. White, Canal street.

VIII.—*A Case of Typhus or Ship Fever, with remarks.* By WM. INGALLS, M. D., Fellow of the Massachusetts, Rhode Island and New Hampshire Medical Societies, formerly Professor, &c.; Boston, 1848.

Dr. Ingalls, some time since, addressed a communication to the editor of the Boston Medical Journal, in which he gave his views, strengthened by those of some of the old writers, upon the nature and seat of typhus, or ship fever. He insists that the muscular structure is the seat of typhus; that the miasm producing typhus acts as an irritant on this system, whence all the symptoms of this formidable disease proceed.

His remedies are as easy of application as the theory of the disease is simple. He declares that "opium in appropriate doses, may be found to be more specific to it, in all its varieties, than any other article in the *materia medica*." We much prefer the sulphate of quinine, either alone and in 20 gr. doses, or combined with small quantities of calomel and some absorbent powder, such as *creta-preparat*. If the skin be too hot, the head painful, and the cheeks flushed, with a quick and irritable pulse, sponge either with cold or tepid water until all these febrile symptoms disappear, then give a large dose of quinine, and the chances are three to one that you will develop a copious diaphoresis, lower the pulse, diminish its frequency and increase its volume, and at the same time relieve all the muscular pains, &c.

We regard it as hazardous, however, to attempt thus to *jugulate*,—to cut short, irrespective of cases, attacks of typhus fever. In those cases attended with partial febrile excitement during the forenoon, to be succeeded by increased fever in the evening, the quinine treatment may arrest the fever and hasten convalescence.

But we are entering too much into details. The author has our thanks for his pamphlet.

IX.—*Report on the Medical Department of the University of Pennsylvania*, for the year 1848. Philad. 1848.

This time-honored and useful school of Medicine, although surrounded by rival Institutions, still maintains its reputation throughout the length and breadth of the Union. Her *alumni* continue to watch her progress with interest and will learn with pride from this report that her career is onward and upward, setting an example in "medical reform," which, we hope, her sister Institutions will strive to emulate.

She has long numbered, among her graduates, some of the first minds in the profession; and we trust she will never confer the Doctorate upon any who may be unworthy to receive it.

The University was one of the first Medical Schools of the country, that pledged itself to lengthen the course of instruction. And speaking of the results of this experiment, the Faculty say,—“They have unalloyed satisfaction in stating to the friends of the school, that their expectations in relation to the numbers and character of the class have been

greatly exceeded ; and they consider the fact to have been established by the experience of the past year, that the improvement in regard to the extension of the courses of instruction in the schools proposed by the Convention, is quite feasible." The number of students in attendance at this school, last session, so far from diminishing, actually exceeded that of any former year,—the aggregate being 508. This fact must prove extremely gratifying to the friends of "*reform*," and should encourage them to persevere in the good work.

X.—*Lecture on Obstetrics and the Diseases of Women and Children.* By GUNNING S. BEDFORD, M. D., *Professor of Obstetrics &c.*, in the New York University, New York, 1848-9.

Professor Bedford, in imitation of the example of other teachers of Medicine, urges upon his class the superior claims of his particular department of the profession. This is a species of vanity that is excusable, because its cultivation may be productive of much good to the student, as it is apt to induce the professor to devote all his energies and talents to the elucidation of the principles and practice of his particular speciality. Dr. Bedford has devoted his time and his talents, (which are by no means mediocre) to the cultivation of obstetrical science ; and we always find much instruction, as well as amusement in his writings. There is an independence, a freshness in his description of cases that contrasts favourably with the tedious and common-place details of some other writers, we wot of, in medical matters.

Professor B, always interests us in every thing he prints. He has the courage to tell the whole truth, and this circumstance alone is enough to invest his writings with no ordinary interest. His bold denunciations of Quacks and quackery entitle him to the thanks of every honorable member of the profession. We quote the following from his "*Introductory*" as an evidence of the truth of the foregoing statement :

"In order that you may form some estimate of the cruel wrongs occasionally imposed on suffering woman, allow me to call your attention to a case which occurred some years since in this city, in the practice of an individual who styled himself 'Doctor,' and who, I am informed, enjoyed the confidence of his patients. On a certain night, he was sent for to attend a female in labor with her first child. The pains were severe, and the labor somewhat advanced when he arrived. About ten hours after he reached the house, the patient was delivered of a child, which bore unequivocal evidences of the Doctor's ignorance. One of the parietal bones was crushed ; the left eye was seriously injured, and the thigh dislocated. The child was delivered in this mangled condition, and, heedless of the remonstrances of friends, who were gathered around the bed of the wretched patient, the Doctor proceeded to remove the after-birth. Such was the violence of the force employed to extract this body, and so wanting was his brutal heart in feeling, that, utterly regardless of the piercing shrieks of his dying patient, and the fervent appeals of her friends to desist from his work of death, he continued the torture for half an hour longer, when, suddenly bracing his feet against the bed, with one prodigious effort he dragged the womb from the body of that woman. But the tale of woe is not yet completed ; the

fullness of that man's guilt has not yet been told; the blackness of his heart no tongue can describe. It was proved by those who were witnesses to this foul deed of blood, that, for ten minutes before the womb was torn from her person, the patient gave no manifestation of suffering; she uttered not a word; and this silence the operator stated was an evidence of his skill—an evidence of the relief he had afforded her! Gentlemen, that silence was no evidence of skill; it was the damning testimony of his guilt. That woman was silent, but it was the silence of death; her light had gone out, and with its extinction ended all her sufferings.

It is, indeed, almost beyond belief that an instance of such extreme atrocity, such cold-blooded murder, perpetrated under the broad mantle of science, could be found in this enlightened age, and especially in a community professing itself to be Christian. And yet the records of our courts of justice will demonstrate that similar acts of barbarity have occurred more than once in the city of New York. In this University, you will be taught conservative midwifery; Nature will be constantly held up to you as a model worthy of imitation; the operations to which she has recourse in order to consummate her work will be minutely pointed out, and this substantial principle emphatically inculcated—*that in order to become Nature's substitute in the lying-in room, you must first accurately comprehend the mechanism by which she, when uncontrolled by accident or disease, accomplishes her object.*"

We cannot extend this notice to greater length; we regret this the more, as the lecture contains many "gems of thought," which we cannot copy. We thank Professor Bedford for his courtesy in sending us a copy of his lecture. He has placed us under obligations to him, on previous occasions, for favours of a similar nature.

Part Third.

EXCERPTA.

1.—*Report made by the Royal Academy of Medicine upon the Memoir of Dr. HALPHEN, describing the "Cholera and Yellow Fever," which prevailed simultaneously in New Orleans in the Fall of 1832, &c.*

[We lay before our readers an interesting report, made by the above scientific body of *savans*, upon a work written by a Dr. Halphen, in which he gives a full and detailed account of the Cholera and Yellow Fever that reigned in this city in 1832. From this report some valuable information may be obtained; the more desirable at this time, because we have just passed through an ordeal, though less trying, yet not entirely dissimilar to that portrayed by the author of this memoir. We are indebted to an intelligent friend for the following translation of the report of the academy.—ED.]

The extensive research of a work conscientiously compiled by an enlightened physician, who unites the love of science with that of his fellow-men, confers upon the present memoir a particular claim to the attention of the academy.—Whilst we state that the respected author has observed Cholera, in its epidemic form, on a particular soil and under a special latitude, at a time at which a Yellow Fever epidemic raged simultaneously with the former, we must acknowledge his advantage of having had under his own eyes the two most serious epidemics of modern times, both combined, and the one affected by the other. Nor can we omit to add, that placed as he then was into the very midst of peculiarly interesting facts connected with pathology, our colleague has had the rare fortune of discovering a method of treating the disease, which in almost every instance, was crowned with complete success.

Without presuming to dwell at length upon each and every detail set forth in said work, we will content ourselves with laying before the reader the following particulars:

Dr. Halphen begins with adverting to the localities, in which it has been his province to observe an epidemic of Yellow Fever co-existent with one of Cholera. In following him in his rapid investigations of the physical and medical topography of New Orleans, with regard to climate, soil, streets, dwellings, establishment, food, water, institutions and administrations, we cannot refrain from acknowledging, that in our opinion it would be difficult to dilate within a smaller compass, upon the many causes which, productive of disease, develop its malignancy to a more alarming extent.

The well known catastrophe of 1811, that violent commotion of the bowels of the earth, of which Louisiana, and in particular New Madrid were the scenes of action, has in no small degree contributed to the insalubrity of this country.

Mr. H. describes the above unfortunate occurrence in the following words: Towards the close of a dull, though cloudless November evening, some rumbling subterraneous sounds, the first signs of the impending tragedy, were closely followed by severe and rapid shocks;—whilst the inhabitants, alarmed in the extreme, fled to a neighboring prairie, the earth rocked like a heaving sea, and crevices opened under their feet;—day finally appeared, but it was only to exhibit to their sight trees and houses violently jostling against each other. Soon after, the whole town disappeared, leaving but a single house as if to indicate to the bereaved families the spot but so lately occupied by their respective dwellings. On the one hand, lakes were replaced by hills covered with thousands of live fish, which still gave evidence of the sudden metamorphosis; on the other, extensive prairies and vast fields sunk in, and became in their turn lakes of considerable depth. The banks of the river, formerly so elevated, were suddenly lowered more than forty feet;—immense inundations encroached upon the soil; and lower Louisiana became one of the most terra-queous districts on the surface of the earth.

As another cause of insalubrity of this country, we may well cite the cemeteries which, situated in the immediate vicinity of the town, occupy the lowest localities; and such is the watery condition of the latter, that on digging to the depth of only three feet, a large quantity of water is immediately collected, a fact which tends to show, that the dead bodies, conveyed thither for burial, may be said more properly to be immersed in water, than interred. To remedy an evil of so revolting a nature, it has been thought expedient to surround said burial-grounds with draining-ditches which, filled often to overflowing with water strongly charged with putrid animal matter, incessantly impregnates the air with miasms as offensive as they are pernicious.

It is frequently the case that families, desirous of securing for their relations a less objectional burial, erect against the outer wall of said cemeteries private tombs more or less elevated above the ground. But these monuments, always constructed of brick, and that often in so great a haste that little attention is bestowed upon the quality of the cement employed, possess neither the thickness nor the solidity required. The bodies therein deposited ferment in consequence of the atmospheric heat, and the frail condition of the brick work allows the gases thus developed to escape, infecting thereby the air with intolerable odors as well as with dangerous effluvia.

The hospitals and prisons would supply us with ample material for critical observations of equal importance; but our limits prevent us from dwelling longer on this subject, and we therefore enter at once upon the description of the two cotemporary epidemics of Yellow Fever and of Cholera.

Towards the end of the Summer there appeared suddenly numerous cases of well pronounced pernicious remittent fever—of Gastro-Cephalitis; on the 22d of September, a few cases of Yellow Fever occurred in various parts of the city, which, having at the beginning of October become more numerous, continued to spread more rapidly, until on the 15th of the same month, the prevalent fever was officially declared to be epidemical. Never had the latter been so virulent; never before had it required more powerful remedies. The application of antiphlogistics, says Mr. Halphen, has proven successful, and the general practice of bleeding in the first stage of the disease, so characteristic of that year, has in almost every case forced itself upon my notice;—he furthermore says, and we expressly quote his words: It is well known that this disease shows itself under different forms, according to its predominant features, depending either upon the atmospheric state, upon localities, or finally upon the sanatory condition of the preceding period. In short, the Yellow Fever is susceptible of so many changes in point of character and type, that a treatment which has insured success in one year, often becomes injurious in the other.

It was on the 15th of October, in the midst of the Yellow Fever epidemic, that in its turn the Cholera broke out in the city of New Orleans, to which place, as the author asserts, it had been imported from St. Louis through the

steamboat "Constitution." "The Yellow Fever as well as the Cholera," says Mr. Halphen on another occasion, "spreads through the medium of an infected atmospheric air."

The ravages of this epidemic were such as to cause, within twenty-four hours, as many as one hundred deaths out of a population of about 50,000; a mortality which warrants the belief that within the space of a few days, the city of New Orleans witnessed the demise of at least one seventh of her population.

The sickness, as has been stated, made its appearance on the 15th of Oct'r; on the 12th of Nov'r following a brisk north wind set in, which was suddenly succeeded by a cold spell; in less than three days the scourge had almost entirely passed away, for there remained only a few cases after the 16th.

The only description which Mr. Halphen has given of this epidemic is a particular and very detailed account of observations made by him on 57 cases, eight or ten of which, having originally been Yellow Fever cases, degenerated into Cholera. Perceiving, that the antiphlogistic treatment, by protracting the effects of Yellow Fever, hastened the development of Cholera, the author reversing the treatment of the two diseases combined in one, introduced a system which in most cases of this nature proved highly satisfactory.

The observations, Nos. 22, 28, 32, 35, and 43 tend to show, that no sooner had the Cholera made its appearance than the Yellow Fever began to abate in intensity, and that, what the former gained in its onward course, the latter lost in its retreat.

According to these facts and to the author's own belief, it would seem that the Cholera, a purely asthenical disease, modifies incessantly the effects of the Yellow Fever, which is essentially of a sthenic nature. Whilst the author observed the Yellow Fever invariably to yield to the influence of Cholera, he has never noticed the latter to be influenced by the action of the former, a fact from which he infers, that the deleterious power of the Cholera is much greater than that of Yellow Fever.

A circumstance not less remarkable, and equally well authenticated, says Mr. H., is, that unacclimated persons, who reached the city when the Yellow Fever was at its height, were not so much affected by the latter as they would have been under ordinary circumstances. Moreover it has been observed that those of them who had the Cholera, were not seized so violently as acclimated persons, and that many Cholera patients, who had previously suffered from Gastro-Enteritis, found themselves after their recovery from Cholera entirely free from their former complaint.

All the observations set forth by the author testify to the efficiency of the sulphate of quinine with thridace, a mixture which was given in the form of pills, potions and injections. The pills were composed of from 3 to 4 grains of sulphate of quinine, each, and a half to one grain of thridace, one pill being given every 5, 10, 15 or 20 minutes, until a reaction became manifest.

The potion contained 40 grains of sulphate of quinine and from 6 to 10 grains of thridace; the nature of the case determined the dose of each to be administered.

The third or fourth part of an injection, to be given every quarter of an hour until the diarrhoea should have ceased, contained from 6 to 10 grains of sulphate of quinine, with from 2 to 4 grains of thridace.

Sinapisms and exciting liniments proved in this therapeutic system, of infinite use.

This treatment it is observed, almost invariably effects a reaction, moderate, regular and sufficient in its nature, and forming the basis of recoveries remarkable for the rapidity and the steadiness of their progress.

Without intending to enter into a minute examination of each of the 57 observations given by the author, we will only add that the latter professes himself possessed of a still greater number of facts, all of which have a direct bearing upon cures effected by means of this treatment.

We will also remark that the treatment of the case, specified in observation 23, which furnished an instance of a decided and long maintained ascendancy of Yellow Fever over Cholera, assuming only towards its close the symptoms of the latter, might possibly, after the preceding facts, have terminated more favorably, if recourse had been had to the combination of quinine and shridace. The patient did not use it, and died in consequence.

The same applies to the patients Nos. 45 and 7: In neither case was that curative remedy applied, and they fell victims to the disease. Mr. Halphen had good reasons for saying at the conclusion of his 7th observation, that at that stage of the epidemic he considered a combination of quinine and common antispasmodics as sufficient, for the latter had already, on various occasions, been used with complete success.

The observations Nos. 8 and 20 demonstrate that the condition of patients, who did not avail themselves of the above medicament, grew worse and worse, whilst those who made use of it began forthwith to mend, making rapid progress in their recovery.

In conclusion, we have to report the death of the patient adverted to in observation No. 51, as being the only one that died, notwithstanding the use of sulphate of quinine with thridace.*

Although an attentive perusal of Dr. Halphen's work has prepossessed us sufficiently in his favor, from inducing us to abstain from any lengthy critical reflections on that subject, we would consider ourselves wanting in the duties we owe to the academy and to ourselves, if we did not quote on the present occasion, the following passage of Cicero on "the nature of the Gods":

"All you who think that the Gods overlook the interests of man, read on the page of history the names of the many individuals who, through divine interposition, have escaped the dangers of the tempest; all that is very good, replied the skeptic, but we find recorded no where the names of those who after ship-wreck perished in the sea."

The memoir of Dr. Halphen seems to us well deserving of the approbation and the encouragement of the academy. Said work should, in our opinion, be laid before the committee on publications, were it only to render it more universally useful by extracts.

Read and adopted in full session, on the 27th of August, 1833.

The perpetual Secretary,

Paris.

(Signed) E. PARISSET.

2.—Nervous System.

CHOREA.—When the case is very violent, give tartar emetic in large doses: to an adult, half a grain every hour, for eight or ten doses, and afterwards at longer intervals. The same treatment may be adopted in some cases of violent cerebral excitement when the tone of the system will bear it. (Mr. Corfe, p. 55.)

DELIRIUM TREMENS.—Often arises from hepatic derangement, it is then to be treated by large and repeated doses of calomel, followed by brisk cathartics; improvement is sure to take place after the passage of a large quantity of dark, offensive bile. The use of opium may be almost wholly abandoned. When there is great excitement, employ cold effusion. In all cases of delirium tremens, however, take care to distinguish between that brought on in regular drinkers when they leave off their stimuli suddenly from any cause, and the other kind which is caused by a sudden and violent fit of drinking continued for a few days together. The first kind is from *exhaustion*, the second kind from congestion and even inflammation of the brain. The first

* *Thridace*.—This is the French name for *Luetucarium*.—Ed

kind is the true *delirium tremens*, the second kind may be termed *delirium ebriosorum*. (Mr. Corfe, p. 50.)

The inhalation of chloroform or ether vapour may be cautiously tried, when other remedies fail, in some cases of habitual drunkards who have been deprived of their stimuli, or otherwise exhausted. (Dr. Anderson, p. 53.) (Mr. Warwick, p. 54.) (Mr. Hooper, p. 54.)

EPILEPSY.—Relieve all gastric, enteric, and uterine irritations, by emetics, enemata, and emollient vaginal injections; prevent all mental and bodily excitement; do not suffer the patient to sleep too deeply, nor to be suddenly disturbed; and order simple and nutritious diet, cold sponging and friction, and fresh air and exercise. Watch the patient narrowly, when an attack is threatened; if the paroxysm approaches, dash cold water in the face: and if the fit comes on raise the head, expose the face and neck to the air, and dash cold water in the face to excite a forcible inspiration, by which the larynx may be opened; apply spirit lotion to the head, and warmth to the feet; and guard the patient against accident. (Dr. M. Hall, p. 41.)

HEADACHE.—The hemicrania often met with in females, during the menstrual periods, arises from ovarian irritation. The treatment is by the daily use of hip-baths or sea-bathing: attention to the state of the rectum; abstinence from stimulants; mental employment; infus. valerian, c. digitalis, with pills and assafoetida; occasional local or general bleeding; or, when these fail, a general mercurial action, the cold baths being omitted. Locally, apply belladonna plasters, veratrine ointment, sinapisms, or blisters. When the patient is much exhausted, try a tonic treatment, giving quinine and valerianate of zinc, or when there is chlorosis, sulphate of iron with inf. valerian. For the headache occurring after hemorrhages or debilitating discharges, and often well marked in chlorosis, give steel, alone or combined with quinia, and let the patient have full diet, and use the recumbent posture. Distinguish this carefully from *congestive* headache, the treatment of which is to induce reaction as soon as possible by the warm bath or an emetic, and if the headache persists, with hot skin, leeches to the mucous membrane of the nose, (*not* to the temples,) blisters and diaphoretics. Dr. P. Murphy, p. 279.

HYSTERIA.—When there is a violent excitement, employ cold effusion. (Mr. Corfe, p. 58.)

INTOXICATION.—Wash out the stomach thoroughly with the stomach-pump, and inject three ounces of dilute acetate of ammonia draught.. Mr. Corfe, p. 49.)

NEURALGIA.—In cases arising from disordered stomach or bowels, or from exposure to cold, let chloroform be inhaled. It is contraindicated, as is also belladonna, when the neuralgia is owing to, or attended by, cerebral affection. In other words, chloroform will also be useful when the cause of the neuralgia is *ex-centric*, contraindicated when the cause is *centric*. (Mr. Sibson, p. 56.)

After exhibiting an anæsthetic, apply the actual cautery, heated to whiteness over the affected part, and then cover it with compresses dipped in cold water. M. Notta, p. 57.)

Apply electro-magnetism for twenty minutes every day. (Dr. Ranking, p. 57.)

PARALYSIS.—*Loss of speech from.*—Employ galvano-puncture. Introduce a metallic needle into the neck, directing its point towards the occipital nerve, and connect it with the zinc pole of a voltaic pile; hold the tongue on a sheet of the same metal, and close the circuit by presenting to that organ the knob of a brass director. Give several shocks daily. (M. Camino, p. 61.)

TETANUS.—*Idiopathic.*—Let chloroform be repeatedly inhaled, so as to produce sleep for some time. (Mr. W. H. Cary, p. 48.)

Traumatic.—Chloroform may be useful in temporarily relaxing the spasms and alleviating the patient's sufferings. (Mr. Worthington, p. 54.)

TOOTHACHE.—Apply *carracrol* upon a piece of cotton, to the carious tooth. (Dr. Bushman, p. 57.)

Apply to the carious tooth a piece of lint, dipped into a mixture of two parts of liquid ammonia, with one of simple tincture. (p. 58.)

3.—Organs of Respiration.

APHONIA.—*Hysterical.*—Apply saturated solution of nitre to thick white blotting paper, and when dry, brush it over with compound tincture of benzoin, and repeat the application if necessary. When thoroughly dry, cut into slips, three inches by one and a quarter. Let one be lighted, and the fumes inhaled every night. (p. 93.)

ASTHMA.—*Spasmodic.*—Let chloroform be inhaled. (Mr. Chandler, p. 93.)

BRONCHITIS.—*In Children.*—In a mild case, give James's powder with a little calomel and ipecacuanha every four hours, and when, as perhaps in twenty-four or thirty-six hours, the child is relieved, omit the calomel, and give small doses of antimonial and ipecacuanha wine in a mixture. In severer cases, where the child is strong, apply leeches beneath the scapulæ; give an emetic dose of tartarized antimony, and then by repeated doses keep the child under its influence for a day or two. Be very careful, however, not to depress too much by the use of antimony, which is not borne so much by children as adults; when the medicine ceases to vomit, or is merely regurgitated without effect, and especially if the face should be livid or the pulse faltering, discontinue the use of this medicine, or give it at longer intervals with ipecacuanha in an emetic dose. Emetics are of great use, especially in the evening exacerbation of the disease, and in the morning when the bronchi have become filled with mucus. Nervous dyspnoæ may be relieved by a mustard poultice, and the hot or warm bath, according to the strength of the patient. If the disease becomes chronic, give tonics, especially the extract of bark, apply a stimulating liniment to the chest, and give an emetic of ipecacuanha every night; or if there is much secretion, give decoction of senega, with ammonia and tincture of squill. (Dr. C. West, p. 82.)

CORYZA.—*Of Children.*—In the *simple* coryza, no treatment is required beyond giving a mild diaphoretic with a little vin. ipecac. attending to the state of the bowels, and preventing the accumulation of the secretion at the opening of the nostrils. If there is much difficulty in breathing, do not let the child attempt to suck, but feed it with mother's milk by means of a spoon. In the *malignant* variety of the disease, keep up the strength by tonics and nutriment and inject a lotion with a drachm of alum to two ounces of water, or three grains of nitrate of silver to an ounce of water, into the nostrils, or apply it to the throat. If a mild catarrh continues long, it is probably syphilitic, and requires small doses of hydr. c. creta. (Dr. West, p. 70.)

CROUP.—*Membranous or Inflammatory.*—Bleed freely, and at the earliest possible period; open the jugular vein, and take about three ounces from a child between one and two years old, and six ounces from a child from eight to ten. Then give emetic tartar, an eighth, a quarter, or half a grain every ten minutes, till vomiting is produced; then similar or increased doses every half hour, till *decided* relief is afforded. If this effect does not take place in four or six hours, apply leeches to the top of the sternum, or near the larynx, and afterwards apply a blister to the throat. But if the croupal symptoms have abated, give the antimony at longer intervals, e. g., a full dose every hour or two, then every three, four, or six hours, remembering that emesis and not nausea is to be sought for. Mercurial inunction should have been employed every two or three hours from the first, and now calomel may be given; to children from two to five years old give half a grain or a grain every hour or two, with a

little ipecacuanha, interrupting its use occasionally by an antimonial emetic. If the antiphlogistic treatment is forbidden by symptoms of sinking, cold skin, livid lips, and feeble pulse, while the breathing is still stridulous, place the child in a hot-mustard bath for a few minutes, and give a quarter or half a grain of sulphate of copper, dissolved in water, every quarter of an hour till free vomiting is produced. And get the child under the influence of mercury as soon as possible, by rubbing a drachm of strong mercurial ointment into the thighs every two hours, and giving a grain of calomel every hour to a child two or three years old. If there is profuse diarrhœa, omit the calomel and rub in more frequently. Support the strength by beef tea; and give stimulant expectorants, as decoction of senega with carbonate of ammonia and tincture of squill, every two hours. (To conceal the pungency of the ammonia, sweeten the medicine with treacle or sugar, and mix with one third of milk.)

When croup occurs during the progress of measles, it will not bear active antiphlogistic treatment. Give emetics of tartar emetic, ipecacuanha, or sulphate of copper, according to circumstances. Apply frequently to the fauces, solution of nitrate of silver, a scruple to the ounce; or if there are sloughy ulcerations, apply strong hydrochloric acid, diluted with twice or thrice as much honey. Apply a mustard poultice to the throat; give calomel, or if there is diarrhœa, rub in mercurial ointment and give decoction of senega with carbonate of ammonia. [In giving large doses of tartar emetic to children, the greatest caution is necessary. Its effects when given rapidly seem cumulative; in children, therefore, under two years old, avoid it if possible, or never trust its exhibition to a mother or nurse; its effects should be *watched* attentively by the medical man himself, who ought to have ammonia or chloric ether ready to administer if the prostration be extreme.—ED.] (Dr. C. West, p. 72.)

Spasmodic.—Give emetics of ipecacuanha to infants, or tartar emetic to older children. During the paroxysm, place the lower extremities in a hot bath, and dash cold water on the face; and tickle the fauces to excite vomiting. The intermediate or prophylactic treatment must depend upon the nature of the exciting cause. If this depends on teething, lance the gums; if on improper food, as in children brought up by hand, ascertain what kind of food best suits the infant. Two parts of milk and one of barley water, sweetened with loaf-sugar, or equal parts of milk and a thin solution of isinglass often agree well; so does asses' milk. But a return to the breast is sometimes absolutely necessary. If the bowels are constipated, regulate them by mild aperients, as castor oil, or a decoction of aloes well sweetened with liquorice, or a little powdered aloes mixed with coarse sugar and placed on the tongue, or a liniment of equal parts of lin. sapon. and tr. aloes rubbed on the abdomen twice a day, or a small soap suppository every day, or enemata of warm water or gruel. If there are severe cerebral symptoms let the head rest on a horse-hair cushion with a hole in the centre, use the tepid bath, give neutral salines with small doses of hyoscyamus, or occasionally even apply a leech to the head. (Dr. West, p. 76.)

HOOPING COUGH.—In the *first* stage, keep the child in a mild, dry atmosphere, and let it have light diet. If there is much cough, give small doses of ipecac. or ant. tart.; and if much wheezing, an ipecacuanha emetic every evening. In the *second* stage, if there is little the matter except the paroxysmal cough, give hydrocyanic acid, half a minim every six hours for a child nine months old, in a little sweetened distilled water. But discontinue it if the child appear faint, or dizzy after its administration; and never persevere with it, if good is not done within three or four days. If there is much fever and a short hacking cough, give small doses of ant. tart. or vin. ipecac. with the hydrocyanic acid; and if much wheezing, give an emetic of ipecacuanha, once or twice a day. If there are symptoms of cerebral congestion, apply a few leeches to the head. If there is much dyspnœa, rub the chest and spine

with Roche's embrocation, or soap liniment with tinc. lyttæ. Should the fever and dyspnœa suddenly increase and seem to indicate inflammation of the lungs, do not deplete or give large doses of tartar emetic, except on the positive evidence of auscultation; and even then remember that much of the dyspnœa may be due to the increased irritability of the spinal system. Abstract blood very cautiously; give small doses of nitre, ipecacuanha, and James's powder, rather than tartar emetic; give an ipecacuanha emetic once or twice a day; apply mustard poultices to the chest; and if the paroxysms of cough are severe, combine hydrocyanic acid with the other remedies; and if there is sinking, and the expectoration ceases, while the bronchi are still loaded, give senega, with ammonia and squills, and nutritious diet. In the *third* stage, recommend change of air. If the skin is cool and the tongue moist, and there is much bronchial secretion, give three or four grains of alum every four or six hours, to a child a year or eighteen months old. If there are dyspeptic symptoms and loss of appetite, give small doses of hydrochloric acid. If the cough is frequent, while the only constitutional symptoms are those of weakness, give iron. While, if iron is contra-indicated by feverishness, or gastro-intestinal disorder, Battley's liquor cinchonæ, with small doses of hydrocyanic acid will do good. (Dr. C. West, p. 77.)

LARYNGITIS.—*Acute.*—Besides the ordinary treatment by leeches and tartar emetic, if there is œdema of the epiglottis, (indicated by there being difficulty in swallowing, and the sensation of a swelling in the throat, without affection of the tonsils,) blow powdered alum into the throat, through a quill. (Dr. T. O. Ward, p. 71.)

ŒDEMA, Of the Lungs.—Occurs chiefly as one of the complications of that acute anasarca which often follows scarlatina, and is very dangerous. Bleed freely, and give large doses of tartar emetic. If the extremities are very cold and the surface livid, apply a large mustard poultice over the chest, and give a large dose of nitrous ether every two hours, till the patient rallies sufficiently to bear bleeding. Afterwards treat the general dropsy. (Dr. West, p. 92.)

PHTHISIC.—In tubercular diseases, the albuminous compounds are in excess in the economy, while the oily compounds are diminished. The most rational method of treating this state, especially as the assimilating organs are generally in a feeble condition, is by the direct administration of an animal oil; and experience points to cod-liver oil as the most practically useful. (Dr. Hughes Bennett, p. 285.)

PLEURISY.—In young and healthy adults, bleed in a very "determined manner and with an unsparing hand, until an impression is made on the system, until the pain and difficulty of breathing are removed, until the patient can draw a full breath, or faints;" and repeat it every three or four hours, according to the symptoms, not placing, however, much dependence upon the pulse. And give mercury to effect the gums; three grains of calomel with a third or half a grain of opium, every two or three hours. In the later stages, when the pulse is becoming weak, and there is much dyspnœa, or when the disease is becoming chronic, apply a blister. (Mr. Guthrie, p. 90.)

PNEUMONIA.—When occurring in young subjects of a healthy constitution, and combined as it usually is, more or less with pleurisy, bleed promptly and freely, till the patient faints, and repeat it every four hours or oftener. And give tartar emetic, half a grain or a grain, every two hours, and even up to 12 or 20 grains a day. But in the stages of hepatization and infiltration, or when there is from the first irritation of the bowels, or abdominal tenderness, substitute calomel, given so as to affect the gums. Blister in the later stages of the disease, and when the pulse becomes weak, while there is great dyspnœa. (Mr. Guthrie, p. 90.)

In Children.—In *idiopathic* pneumonia occurring in previously healthy children, depletion is as important as in the adult; and is to be followed up by tartar emetic, given in doses of one-eighth of a grain every ten minutes, (to

a child two years old,) till vomiting is produced, and continued every hour or two afterwards for twenty-four or thirty-six hours. Then if the physical condition of the lungs, and the general state of the patient, are found greatly improved, persevere with the medicine at longer intervals; but if the signs of inflammation are advancing, give mercury with small doses of antimony, and use larger doses of the latter to combat any sudden increase of fever or dyspnœa. If under any circumstances bronchial breathing is distinctly audible, the mercurial treatment is indicated; give, to a child two years old, a grain of calomel every three or four hours, and a little tartar emetic, except contra-indicated by sickness or debility; if the stomach and bowels are very irritable, use mercurial inunction. Do not blister, but employ stimulating liniments, by which there is no risk of those unhealthy sores which often follow a breach of the surface. If at the outset, large doses of antimony do not seem to be required, give two-thirds of a grain or a grain of calomel with two or three of James's powders every six hours. It is difficult to know when to give stimulants; but they are plainly indicated when there is much diarrhœa, the pulse becoming more frequent, and above all smaller and smaller, and the respiration, though slower, more laboured and irregular. Then give wine, even to a child at the breast, and ammonia in decoction of senega, or dissolved in milk, which conceals its pungency. If there is diarrhœa, let the nutriment be arrow root, or the *decoction blanche* of the French; otherwise give strong beef tea, or veal broth. In *secondary* pneumonia, especially if preceded by well marked bronchitic symptoms, antimony may sometimes be given at once without bleeding. (Dr. West, p. 90). (The preceding Extracts, from page 650, were taken from Braithwaite's Retrospect, for 1848.—Ed.)

4.—*Materia Medica and Pharmacy.*

On Emulsions of Castor Oil. By A. MANNE.—Having frequently to make emulsions of castor oil, either by prescription of physicians, or at the demand of my customers, making them in fact four or five times a day, I found myself in a position to make researches relative to the best means of administering this medicine. * * * I have employed gum arabic in different proportions, but have obtained emulsions either too thick or imperfectly made. The yolk of egg has offered me an excellent emulsion, but one yolk is required for every ounce and-a-half of oil. Is it not possible that the yolk of egg injures the purgative effects of the oil by its nutritive properties? Does it not render the purgative heavier and less supportable by the stomach? These doubtful points lead me to doubt the utility of yolk of egg, and determined other attempts at finding a means of making a homogeneous emulsion, agreeable to the sight and more supportable by the stomach. To accomplish this I have had recourse to gum tragacanth, and the following formula has given the most satisfactory results:

Formula for Purgative Emulsion of Castor Oil.

℞. Castor oil,	11 drachms.
Powdered tragacanth,	$\frac{1}{2}$ drachm.
White sugar,	75 grains.
Water,	$2\frac{1}{2}$ fluid ounces.
Syrup of orange flowers,	6 fluid drachms.

Mix, and make an emulsion as follows:

Triturate the tragacanth with the sugar, then add the syrup and agitate rapidly in a mortar, until the mucilage begins to thicken a little, I then add the oil and continue to triturate until the mixture is homogeneous, and add the water little by little during the trituration. In this manner I have obtained an emulsion of castor oil which leaves nothing to desire, and which will remain eight or ten days without an atom of oil separating.—*Amer. Jour. Phar., from Jour. de Chimie Medicale.*

Quinine as a Prophylactic in Puerperal Fever.—M. LEUDET cites several cases which occurred during the prevalence of three different epidemics at Rouen, in which quinine, given in doses of five grains three times a day, and commenced with a few hours after delivery, seemed to exert a protective agency. On the third day, this quantity was diminished, and the drug left off about the sixth day. In those epidemics in which the disease commences very speedily after delivery, the quinine should be administered as soon as labor begins.—*Med. Chir. Rev., from L'Union Méd.*

5.—*State of Medical Education in Turkey.*

[We are indebted to a medical gentleman, at present residing in Constantinople, for the following details. They point to the existence of a state of matters, of which very imperfect accounts have as yet reached this country; and such as, undoubtedly, is at once the proof and the earnest of a new era in the history of civilization having begun in the East.]

“Military hospitals, on a large scale, are either built, or a-building in every quarter of the Turkish Empire. There are about one thousand European surgeons attached to the different regiments, two hundred of whom are Jews. The chief professor of the Medical College, Dr. Spitzer, is a Jew. He is also one of the physicians in ordinary to the Sultan. By him I was introduced to H. E. the Hakim Bashy Ismael Effendi, the chief physician of the Ottoman empire, who kindly permitted me to visit the different lecture-rooms in the Medical College. It is certainly in a very flourishing condition, considering that it has been in existence only eleven years. The pupils are brought, by order of the Sultan, from all departments of the empire, and are lodged, fed, clothed, and educated, at the expense of the government. When qualified as physicians and surgeons, they receive appointments in the army and navy, with salaries of £200 or £300, and upwards, according to rank, without distinction of Jew or Gentile. Until lately, however, there were no Jews in this college; not that the government excluded them, on the contrary, they were invited; but that people, who have been scattered amongst all nations, yet amalgamated with none, would not send their sons to this medical establishment, even although the most flattering prospects of education and worldly advancement were held out to them. But the government condescended to smooth all the difficulties which stood in the way of the improvement of this section of its subjects. Through its agents, it held interviews on the question of conscientious scruples with the chief Jewish Rabbis: and the result was, not only the guaranteeing liberty of conscience to the Jews who should enter the Medical School, but the assigning to them a distinct portion of the college, so that they might live separate from the Gentiles, the appointment of a superintendent of their own persuasion, to see that their religious duties and services should be strictly observed—also a shocket, or butcher of their own; and, in short, every arrangement was made to prevent their being constrained to do any thing contrary to their conscience. In the language of last year's report of the college authorities.—‘Toutes les difficultés ont été aplanies, et le Gouvernement n'a reculé aucune sacrifice pour exercer aussi son influence civilisatrice sur cette partie des sujets de l'Empire.’ The Sultan lately visited the college, and presided at the examination of the candidates for the medical degree. When the pupils are first introduced to the college, they are, for the most part, raw, ignorant, and uncivilized. They are at first taught Turkish, afterwards the Arabic and French languages; next geography, history, arithmetic, and other elementary branches of education, including natural history. They have already a very tolerable museum of natural objects, well preserved and well arranged. A small botanic garden is also attached to the college. After undergoing a thorough elementary education, the pupils enter their medical course, comprising

lectures on anatomy, physiology, chemistry, materia medica, practice of physic, surgery, and midwifery. The only room I did not see was the dissecting-room ; it was closed at the time for want of subjects, which it is difficult to procure in a country where so much prejudice against dissection exists, and even against touching a dead body. I was shown into the grand examination room, fitted up with a great throne of state for the Sultan, who presides at the yearly examination of the candidates for the medical degree. There are also a dispensary and hospital attached to the college. The hospital is divided into medical and surgical wards, and a special ward is set apart for diseases of the eye. Dr. Spitzer delivers clinical lectures in the hospital, which he kindly invited me to attend.—*Constantinople*, March, 1848.—*Monthly Jour. Med. Sci.*—N. Y. *Jour. Med.* 1849.

6.—Question of Professional Secresy.

Dr. VIRIELLE, of Rochelle, was consulted by M. B. for a venereal affection which, without knowing it, he communicated to his wife. She separately consulted the same practitioner. Some time after, the lady being desirous of procuring a separation from her husband, summoned Dr. Virielle as a witness, to testify to the injury she had received at his hands. Dr. Virielle refused to take the oath to speak the truth, as he could not reveal that which his professional obligations entitled him to keep secret ; and he did not believe that even the authorization which the plaintiff offered him, as regarded her own case alone, would allow him to do so. The court decided that while the Code forbade professional revelations on the part of a physician made with an intention of doing injury, it did not authorize a refusal when demanded by justice ; for this would be to exempt him from obligations which are imposed on all other citizens. The refusal is especially unjustifiable when the consultant herself demanded the revelation, and did not seek for any in respect to her husband.

A fine of sixty francs was imposed ; but the medical society of Rochelle have carried the case, by appeal, to the court of Poitiers, where it seems that in 1828, in a precisely analogous case, judgment was given in favor of the medical attendant.—*L'Union Médicale*

[Notwithstanding the approval which the editors of the French Medical Periodicals bestow upon the conduct of the physician in this case, we believe it quite unjustifiable. Inviolable, indeed, should be professional secrets in all the relations of private life ; and no punishment could be too severe for him, who, for mischievous purposes, betrayed those of which necessity, misfortune, or accident put him in possession. But, when summoned to the tribunals of his country, the furtherance of the ends of justice becomes the physician's highest duty ; and if the inviolability here claimed were accorded, and fully carried out, especially in criminal cases, it might lead, not unfrequently, to the utter frustration of these ends.—*Med. Chir. Rev.*—*Ibid.*]

7.—Diseases affecting the System generally.

ANSARCA.—At an early period, make four of five deep incisions, a third of an inch in length, down to the fascia, in each leg ; and keep the patient in an arm-chair near the fire with the legs naked, and resting on cloths. When all the serum has drained away, support the legs by a roller. (M. Lombard, p. 51.)

DISEASE GENERALLY.—It is found that the kidneys can depurate the blood, not only of matters generally regarded as proper to their function, but of substances usually separated by other emunctories ; in fact, they remove all

soluble noxious matter. When, therefore, disease is excited or aggravated by noxious or lethal effete matter in the blood, we should endeavor to procure the decomposition and elimination of this matter by stimulating the depurating functions of the kidneys. And this may be accomplished by the exhibition of the alkalies, and their carbonates, citrates, acetates, and tartrates, which not only stimulate the kidneys, as do the vegetable diuretics, but also increase the metamorphosis of tissue going on the capillary system. (Dr. Golding Bird, p. 57.)

FEVER.—*Continued.*—When the cerebral affection in the course of continued fever is attended by a quick and feeble pulse, great restlessness, total want of sleep, subsultus, and much disturbance of the nervous system generally, Dr. Graves's plan of treatment is highly valuable. It consists in giving tartar emetic with opium, the formula being, tartar emetic, four grains; tincture of opium, half a drachm to a drachm; camphor mixture, eight ounces; half an ounce or an ounce to be given every two hours. (Mr. G. Todd, p. 28.)

Of Children.—During the first week the treatment is chiefly expectant; consisting in the use of the tepid bath at 90° to 92° every morning, and tepid sponging several times a day; light aliment; simple salines, such as the citrate of potash, with a little ipecacuanha wine if there is troublesome cough; and a little castor oil if the bowels are confined. If there is a disposition to diarrhœa, give equal parts of Dover's powder and hydrarg. c. creta, once or twice a day. Ascertain every day whether there is abdominal pain and tenderness; if it exists, and is not relieved by the application of linseed meal or bran poultices, the application of a few leeches may be needed. Local depletion may also be needed, for symptoms of cerebral disturbance, if there is great heat and flushing, and noisy delirium as also if there is much moaning, restlessness and headache. But if the delirium occurs only at night, and is of a tranquil kind, it will suffice to apply cold to the head, and to keep the apartment cool and quiet. During the second and third weeks, nutritious food will be required, and should consist in broth and beef tea; or if there is diarrhœa, arrow-root, milk, and isinglass. Wine may even be needed; and the following form of stimulant will generally be found useful. For a child of five years old, it is four minims of dilute hydrochloric acid, eight of compound spirit of sulphuric ether, and three drachms of camphor mixture, every six hours; a small dose of Dover's powder, as a grain or a grain and a half should also be given at bedtime, to prevent diarrhœa and procure sleep. If there are severe abdominal symptoms, the acid mixture must, of course, not be given. (Dr. C. West, p. 32.)

Intermittent.—In using quinine as a remedy for the West India intermittent or remittent fever, it should be given till its specific effect on the system, termed by the author "cinchonism," and marked by the supervention of more or less deafness and ringing in the ears, is produced. The best way to saturate the system with the remedy, against the paroxysms of intermittent, is to give hourly doses of three grains, till twelve doses have been taken; or if the disease is a quotidian with short intermission, six grains hourly, until six doses have been taken. One of the most valuable applications of quinine is against relapses of intermittent fever. For this purpose, two days before the anticipated relapse, give three grains of quinine thrice daily for four days; repeat the treatment at the time of the next anticipated relapse, and so on for three or four times successively; when by thus baffling the relapse, the disease will be entirely eradicated. (Dr. D. Blair, p. 26.)

Scarlatina.—In some epidemics of malignant scarlatina, the state of the fauces is identical with what occurs in epidemic diphtherite. In these cases, begin early in the disease before ulceration commences, to give one or two grains of calomel every four hours; and apply a solution of Nitrate of silver, a scruple to the ounce, twice or thrice a day, to all the affected parts. Internally give acetate of ammonia, or, in the latter stages, quinine with sulphuric acid. (Dr. J. M. Coley, p. 34.)

GOUT.—An attack of gout is invariably dependent upon impaired function of the liver, and is certain to be relieved by a copious evacuation of bile. The best way to secure this end, is to give one of the following pills every four hours: $\mathcal{R}$. Hydr. chlorid. ext. colch. acet., ext. aloes purif. aa. gr. j.; pulv. ipecac. gr. ij. M. fit. pil. After two or three of these pills have been taken, assist their action by giving a dose or two of the compound decoction of aloes. By this time the paroxysm will have been relieved; and the pills may then be given at longer intervals for a little time longer. (Mr. A. White, p. 44.)

Acute.—In acute gout, begin with a purgative, consisting of from ten to twenty grains of jalap, and from three to eight of calomel, followed by a draught consisting of infusion of rhubarb and senna, with a drachm of the sulphate of potash and a scruple of the carbonate. After that let the patient eat little or nothing for twenty-four hours, and give a diaphoretic drink with ipecacuanha wine, or acetate of ammonia; and at night give a full dose of Dover's powder. On the third day begin with colchicum, giving from twenty to sixty drops of the wine twice a day, or four or six ounces of distilled water, along with five or ten grains of nitrate of potash, two drachms of compound spirit of juniper, and half a drachm of spirit of nitric ether.

Chronic.—In chronic gout, alteratives, directed to the biliary and digestive organs are needed: opiates may be given more freely; and more stimulating diaphoretics, such as guaiacum may be used. Otherwise the treatment differs, from that for acute gout, only in being less active. Veratrine may be advantageously used, externally in the form of pomade, with four grains of veratrine to an ounce of lard, applied to the affected part; and internally, in doses of one-twelfth to one-eighth of a grain, dissolved in distilled water, twice or thrice a day.

Metastatic.—Metastatic gout must be treated by the application of sinapisms, blisters, hot pediluvia, and stimulant frictions, to recall the disease to the extremities. While the feet are plunged into hot water with mustard in it, if the stomach is attacked, we must give cold or iced drinks with opium; if the head is the seat of metastasis, we must apply cold lotions to it; while if the heart is affected, we should apply no means but the counter-irritation, and above all take care not give hydrocyanic acid. (Dr. Robert Dick, p. 47.)

RHEUMATISM.—Acute.—The indications are to relieve pain, to promote the action of the skin, kidneys, and bowels, to use antacids, and to give large quantities of diluent fluids. For these purposes, give a grain of opium, a grain of ipecacuanha, and five grains of nitre, every two, three, or four hours; and a mixture of sulphate and carbonate of magnesia. Envelope the joints in a large quantity of cotton wool, and cover with oiled silk; changing it every twelve or twenty-four hours. Give plenty of simple diluents, and from the first, let the patient have a little good beef tea frequently through the day. And when the patient begins to pass pale urine, with or without pale lithates, he will be the better for generous diet, with wine, ammonia, or quinine, even though the articular affection persists. Too much sweating, too much purging, or too much opium, are equally inadvisable. If the patient cannot bear opium, extract of hyoscyamus, hop or lettuce may be substituted. If the state of the joints does not yield to the application of cotton wool, apply a small sinapism for half an hour to redden the skin, and apply a blister the size of a crown piece; the blistered surface may be allowed to heal, or may be dressed with stimulating ointment; or a succession of small blisters may be used. Watch the state of the heart from the first; and on the first indication of pericardial or endocardial affection, apply a large sinapism over the region of the heart, and when it comes off, a large blister: but *do not bleed* either locally or generally. Give calomel and opium to affect the gums; and, if needed, rub in mercurial ointment, or use it to dress the blistered surface. When delirium, resembling delirium tremens, occurs as a complication of rheumatic cardiac affections, it is "a signal of distress," and must be responded to by an immediate alteration in the treat-

ment. All too free evacuations, whether from the skin or bowels, must be checked; nourishment must be given frequently in small quantities; and even wine, brandy, or porter, may be administered. If the patient is wakeful, give opium. And take care that all exertion is avoided, lest fatal syncope be induced. If, however, there is coma, do not give opium, but apply sinapisms or blisters. (Dr. R. B. Todd, p. 36.)

Chronic Gonorrhæal.—Introduce a bougie into the urethra, twice or thrice a week, in addition to the usual treatment. (Mr. Gorfe, p. 103.)

SCROFULA—In addition to the usual hygienic treatment, give small doses of the *purest sulphur*, which has the effect of accelerating the capillary circulation, and restoring the defective animal heat. The following formula may be used:—℞. Sulphur, purif. gr. v. ad x.; syrup simp. ℥ i.; aquæ 3 vii.; bene terendo ft. haust. To be taken once or twice a day in a tumbler full of new milk. A slight chalybeate may sometimes be advantageously added. (Mr. F. A. Bulley, p. 152.)—*Braithwaite's Retrospect*, for 1849.

8.—Affections of the Nervous System.

APOPLEXY.—When what are called “threatenings of apoplexy” occur, corresponding to what Dr. Hall terms “paroxysmal apoplexy”—in the actual attack, bleed instantly, and give an effectual emetic, enema, and purgative. Afterwards let the diet be more carefully regulated, any discoverable source of irritation removed, business entirely suspended, and all emotion avoided. “The remedy of remedies is travelling.” (Dr. M. Hall, p. 92.)

When apoplectic symptoms arise from disorder of the liver, give a full purgative, consisting of ten grains of calomel and the same quantity of extract of colocynth; and repeat the same in smaller doses, followed by a cathartic draught, three or four times a week. (Mr. Corfe, p. 97.)

DELIRIUM TREMENS.—Let chloroform be inhaled. (Mr. S. L. Gill, p. 102.)

HYDROPHOBIA.—The patient dies from repeated excitation of reflex action, wearing out as it were the power of the spinal centres; and from asphyxia, induced by repeated paroxysmal closure of the larynx. Tracheotomy is therefore to be performed, in order to prevent asphyxia; and the patient is then to be placed on a spring bed, and surrounded by ranges of curtains of lace or net, and every current of air, every shake of the bed or the floor, in a word, every excitation, or cause of reflex action or emotion, absolutely avoided. (Dr. M. Hall, p. 98.)

Let repose be procured by inhalation of chloroform, once or twice a day; and apply counter-irritation to the head and spine, and give active purgatives. (Mr. R. Y. Ackerley, p. 99.)

NEURALGIA.—Use active and prolonged bodily exercise. (p. 104.)

PARALYSIS.—Cerebral.—It is important to notice the state of the muscles of a paralysed limb; for if rigidity of the muscles exists at an early period, local depletion and counter-irritation will be attended with benefit, whereas such measures are not applicable when complete relaxation exists. (Dr. R. B. Todd, p. 80.)

From Lead.—Prevent the further introduction of the poison by cleanliness and frequent washings; stimulate the skin by friction and exercise; give baths containing sulphuret of potassium, in the proportion of one, two, or three ounces, to as many gallons of water; let the patient have good food, and breathe pure air; and apply galvanism as a local stimulant, for ten or fifteen minutes at a time, three times a day. (Dr. R. B. Todd, p. 88.)

Of the Portio Dura.—If arising from otitis, antiphlogistic treatment is needed, and sometimes even salivation will be beneficial. But if arising from cold, or constitutional causes, much treatment will not be required, beyond the application of warm fomentations, and the exhibition of mild purgatives, or dia-

phoretics, alkalies, or iodide of potassium; leeches and blisters are not so useful, and strychnine should not be given at all. (Dr. R. B. Todd, p. 83.)

SCIATICA.—In obstinate chronic sciatica, introduce a bougie into the urethra, twice or thrice a week. (Mr. G. Corfe, p. 103.)

TETANUS.—Though opium does not fulfil the indication of diminishing the excitability of the spinal cord, yet in the present state of our knowledge, we are not justified in discarding it. Our principal object, in the treatment of tetanus, should be to support the patient's strength. Operations in traumatic cases are not only unnecessary but injurious. (Mr. S. G. Wilmot, p. 100.)

Traumatic.—Let chloroform be inhaled, give calomel purgatives, and use counter-irritation. (Mr. R. L. Baker, p. 100.)

Let ether be inhaled; and give morphia or opium, and croton oil purgatives. (Mr. J. G. Lansdown, p. 101.)

TOOTHACHE.—Having dissolved some gum copal in chloroform, clean out the hole, moisten a little cotton with the solution, and introduce it into the decayed part. (Mr. J. Beatson, p. 105.)

If there is free access to the nerve of the carious tooth, apply a bit of ice, or a little frigorific mixture. (Dr. J. Arnott, p. 342.)—*Braithw. Ret. for 1849.*

9.—Affections of the Urinary Organs.

ALBUMINURIA.—The indications of treatment are: 1st, to remove such exciting and predisposing causes as intemperance, and residence in an impure atmosphere; 2nd, to oxygenize the blood by active exercise in the open air; 3d, to avoid fatty and other non-nitrogenous articles of food; 4th, to administer alkalies freely; and 5th, to keep up a tolerably free action of the bowels. (Dr. J. F. Duncan, p. 140.)

DIABETES.—The azotized dietetic plan of treatment is efficacious, and the admixture of a small quantity of vegetable food does not very materially interfere with its favorable operation. (Dr. R. B. Todd, p. 140.)

HYDROCELE.—Inject the following: $\mathcal{R}$. Iodid. potassii $\mathfrak{z}$ j. aq. distil. $\mathfrak{z}$ iss.; tinct. iodine, $\mathfrak{z}$ iv. M. In using this solution, it is of no importance if all the fluid injected does not return. (Dr. Bellingham, p. 203.)

UREA, *Deficiency of*.—This is usually associated with diuresis, and one of the first principles to be attended to is to reduce the quantity of urine, by restraining the patient, as much as possible, from drinking. Cutaneous action must be kept up by the use of the vapour bath and friction, together with Dover's powder and antimonials. The bowels must be kept regular, but active purging avoided. The diet should consist of animal and farinaceous matters, with good porter. (Dr. Prout, p. 132.)

Excess of.—Avoid all rough treatment. Purgatives and alteratives are often necessary, but must be used with caution. Sedatives, especially opium, are usually required, and should be combined with appropriate tonics. The food should be light and nutritious; all diluent and diuretic fluids being avoided. (Dr. Prout, p. 132.)

URINE, *Retention of*.—In retention of urine from paralysis of the bladder, cerebral or otherwise, give small and repeated doses of ergot of rye, up to seventy-five grains a day; and continue its use, in decreasing doses, for eight or ten days after the cure. (Dr. Allier, p. 143.)

VARICOCELE.—A new form of truss is recommended, having some advantages over the moc-main lever. (Dr. L. R. Thompson, p. 204.)—*Ibid.*

10.—Case of Pseudo-Membranous Laryngitis. By Dr. C. D. MEIGS.

A child, $\text{\ae t. four}$, becoming affected with difficult and noisy respiration, was placed under the care of a homœopathic practitioner; the parents having lost al-

ready a child from croup, recognized in this the same symptoms as were observed in the former case, and suggested to the medical attendant that the child was laboring under that disease, but this he declared was not the case, but rather thought that the attack would turn out to be one of measles. The child, however, grew worse and worse; no eruption appeared upon the skin, and at the end of two weeks, the respiration having become increased in difficulty and attended with a distinct croupy sound, while the voice of the child was nearly extinct, the parents became alarmed and sent for Dr. Meigs. His son, Dr. J. F. Meigs, immediately saw the patient and found it in advanced stage of genuine membranous croup, attended with symptoms of the most violent character; an extensive deposition of membranous matter appear to have taken place, and the case was looked upon as almost hopeless. With the view, however, of affording, if possible, some relief to the extreme difficulty of breathing, the doctor directed the application of five or six leeches to the throat on each side of the trachea. Dr. C. D. Meigs now saw the child, and considered it to be in the most imminent danger. The croupal symptoms were intense. Upon auscultation, not the slightest respiratory murmur could be detected in any part of the chest, giving the idea of an individual laboring under complete hepatization of both lungs. The air passed into the lungs with the greatest difficulty, the respiratory effort being prolonged to an extent beyond what the doctor recollects to have ever before witnessed. The child was extremely restless, its head was thrown back upon the spine, and every moment strangulation seemed imminent. A half-ounce of powdered alum was directed, and one drachm of it given to the child at intervals of twenty minutes, until emesis was produced, which did not occur until after the fourth dose. This was rather an uncommon occurrence, vomiting being generally produced by a single dose of alum; it evidently indicated a torpid state of the nervous mass, the result of the great change produced in the blood, in consequence of the imperfect performance of the respiratory function. No nausea or prostration followed the action of the emetic.

Early the next morning, found the child laboring under the most distressing difficulty of respiration. The surface, and particularly the face, lips and tongue, were of a blue color, and nearly all the symptoms of a state of asphyxia were present. Dr. Meigs considered that death was inevitable, but still the operation of tracheotomy, though a forlorn hope, presented itself as the only possible means of relief. This was stated to the parents, who consented that it should be tried. Accordingly, at 11 o'clock, the operation was performed by Dr. Pancoast. After laying bare the trachea, he divided the second, third and fourth cartilagenous rings; immediately upon opening the trachea, a discharge took place of mucus, mixed with blood and portions of plastic lymph. In forty seconds, the child breathed with great freedom. Instead of inserting a tube in the usual manner through the opening into the trachea, Dr. Pancoast secured the open state of this by cutting from the trachea an elliptical portion of cartilage, thus leaving an oval opening into the tube somewhat larger than that of the two nostrils; while the edges of the incision through the soft parts were kept asunder by a leaden wire, which, passing around the neck, had the hooked ends of its two free extremities inserted on each side of the wound. The next day the child was up and running about. In a few days the edges of the incision in the neck were brought together, the wound rapidly healed, and the child within a surprisingly short period, recovered perfectly without a single disagreeable symptom occurring.—*Trasact. Philad. College of Physicians*, 1848.

11.—Case of Membranous Croup.—By Dr. PARRISH.

The patient was a child of eighteen months old, which was attacked with what was supposed to be an ordinary catarrh, attended with a harsh, dry cough. Simple domestic remedies had been administered for several days without effect, before the doctor was sent for.

When the patient was seen by him, the cough was "croupy," and the breathing obstructed to an alarming extent. On looking into the fauces, the whole back part of the throat was found lined with a thick, tenacious secretion. An emetic consisting of two grains of turpeth mineral was immediately administered, which operated promptly, but without relief. Calomel, grs. v, was administered, to be followed by castor oil in a few hours. After the operation of the purgative, there was still no decided relief; the turpeth mineral emetic was continued regularly every four hours, and the calomel in small doses every two hours for several days; but the disease steadily progressed; no discharge of membrane having been induced; the breathing became more distressing, and finally the child was unable to cry; the cough was dry and less developed, and the bronchial tubes appeared to be rapidly filling up. At this stage of the complaint, injections of assafoetida and laudanum were given every four hours, solely with a view of assuaging the sufferings of the child, and all other medicines were suspended. The full effect of the opium was induced, and the jactitation and restlessness diminished, though the breathing continued as bad as ever. The child lay upon the pillow with the head thrown back, and was several times supposed to be dying, from the violence of the paroxysms of dyspnoea. The doctor left late in the evening, with directions to continue the anodyne, expecting to find his patient dead in the morning. On the morning visit he was surprised to find that the paroxysms of extreme difficulty of breathing, had been less frequent, and that the child had slept with comparative comfort, though the respiration was still exceedingly laborious. It was found that a discharge of thick yellow mucus had begun to issue from the nostrils during the night, and on examining the throat, it was evident that the membrane lining, the fauces, was loosening. The bowels had also been freely moved with copious yellow dejections.

This state of things afforded encouragement to resume the use of turpeth mineral, which acted promptly, bringing away large quantities of thick yellow mucus, to the great relief of the infant, who from this time went on improving and recovered rapidly.

Dr. Parrish had no expectation of accomplishing any paramount good by the use of the anodyne in this case, believing that the mechanical obstruction of the trachea and bronchial tubes must inevitably destroy life. It becomes a question, however, how far the dyspnoea in cases of membranous croup, may be the result of nervous spasm, as well as of a mechanical impediment to the passage of air into the lungs. It is evident that the dyspnoea is, to a certain extent, paroxysmal, and it is also true that in many cases where death has occurred, the accumulation of membranous deposit in the air passages, as discovered on a post-mortem examination, has not been sufficient to produce strangulation from mere mechanical obstruction. By keeping, therefore, the system under the influence of anti-spasmodics and anodynes, in addition to the remedies calculated to arrest the inflammation, may we not gain time and enable the latter to have their full effect in arresting the disease, and in producing softening of the membranous deposit?

The doctor believed that in the management of this intractable malady, we have neglected too much the use of this class of remedies. He would also take this occasion to express his satisfaction with the action of turpeth mineral as an emetic in croup. He had used it on several occasions, since it had been so warmly recommended to the college in the communication of Dr. Hubbard, of Maine, and had been highly pleased with it. It acts promptly and powerfully, without leaving behind it the depressing effects of the antimonials.

The emetic may be repeated at short intervals and continued, as in this case, for many hours without the risk of alarming depression.—*Ibid.*

Part Fourth.

AMERICAN MEDICAL INTELLIGENCE.

I.—*Case of Lithotomy.* 117 *Calculi, weighing four and a half ounces, successfully removed.* By PAUL F. EVE, M. D., Professor of Surgery in the Medical College of Georgia.*

A brief notice of the following case, against the writer's expressed wish, was made in one of our newspapers. It is proposed to record it now, where, if it possesses sufficient interest, it legitimately belongs.

In the severe September gale of 1824, Mr. O'Bannon, then a lad of 18 years, was engaged at work upon a house, which was blown down. In the fall, he was struck upon the back by a piece of timber, and from the injury then received he dates his difficulty in urinating. During the twenty-four years he has been a sufferer, Mr. O'B. has fully tested the prescriptions of the *unprofessional* of several States, and he has travelled far and near in search of relief. He even became a sailor on the ocean; but all to no purpose, his disease continued to harrass him day and night.

For the past two years his difficulty became so great, that to discharge urine at all, he had to assume the horizontal position, and then with the fingers introduced into the rectum, he pushed up the bladder. A large quantity of matter, he says, is also evacuated by the penis. When he sits upon the edge of a chair he experiences a sensation as of crushing (crepitation) a ball of snow in the perineum.

In December he entered the charitable Institution under our Faculty, and a catheter was for the first time attempted to be introduced. This came at once in contact with a calculous mass in the perineum, where a tumor was found, projecting to the right of the raphe running back from the scrotum.

OPERATION.—On the 16th of last month (January) the following operation was performed in the presence of the Medical Class of our College—Chloroform was administered by Dr. Means. After the patient was placed in the usual position for lithotomy, an incision, about three inches in length, was made over the tumor situated in the perineum, as for the lateral operation, except that it was upon the right, instead of the left side. About 56 calculi were removed through the opening, and it was hoped the operation was completed; but upon introducing a female catheter through the wound into the bladder, a second collection of stones was readily detected in this receptacle. A grooved sound was now passed through the urethra and the double lithotome conducted by it into the bladder; the former was withdrawn and the

* We are indebted to Professor EVE for the details of this interesting case; it will also appear in the March number of the Georgia Med. Journal. *Ed.*

bi-lateral section completed, by drawing the latter instrument out somewhat in the line of the external incision made in the skin. With the lithotomy forceps repeatedly introduced, by conducting it upon the finger, 61 stones were extracted from the bladder. Through the opening in the perineum a quantity of pus was discharged. During the operation, the rectum protruded in a large mass so as to interfere with lowering the handle of the forceps, to seize the calculi in the bladder.—The patient also had violent and involuntary contractions of the abdominal muscles, and during the latter stage of the operation the chloroform was discontinued. It lasted one hour. He was so reduced by his long suffering, a period of twenty-four years and four months, that after the operation I took him like a child in my arms and carried him up a flight of stairs to his room.

The following is the analysis of the calculus, kindly made by Professor Means, and addressed to me :

“ The urinary calculus, taken from the bladder of Mr. O'Bannon, has been subjected, at your request, to a chemical analysis, and merits at my hand the following description, viz :

External form.—The particular calculus under consideration, is but a fair specimen, both in its physical properties and chemical constituents, to every other of the entire number removed from the perineum and cystic cavity of your recent patient, and which, by your courtesy, I was privileged to examine both during, and after the extraordinary operation. Being a solid, bounded by four oblique planes, it presents the *tetrahedral* shape distinctly ; its solid angles and lateral edges, instead of being regularly truncated, and replaced by tangent planes, exhibit gently rounded surfaces, which gradually blend with the respective faces, and are evidently the result of constant attrition, kept up for many years.

Physical characteristics.—The exterior furnishes a beautifully smooth, and even polished surface. The *structure* is laminated with admirable parallelism—the respective tunics conforming to the external figure of the stone, and easily separable by the nail—the *fracture* uneven, and the *powder*, harsh and gravelly under the touch.

The predominant *color* is a greyish white, which is frequently substituted, however, in the more deeply seated laminae, by a pale-brown tint. Its *specific gravity*, is 1.02.

Chemical constituents.—I had anticipated the *uric acid calculus*, but the use of the blow-pipe flame, and the application of appropriate acid and alkaline tests, soon revealed the presence of *Phosphate of Lime* almost pure. This form of urinary concretion has been pronounced by Silliman, Gardner and others, as very rare. It is peculiar, however, to the *prostate gland*, in the neighborhood of which the calculi, in your recent operation, were found to be imbedded, and which probably controlled the chemical affinities that subsequently deposited so large a mass in the fundus of the bladder. Its chemical elements are 3 atoms of Phosphoric Acid, 8 of Lime, and 1 of basic water, as expressed in the following formula :— $3 \text{ CaO}, \text{HO} + 3 \text{PO}_5$.

The *Fusible Calculus* (Phosphate of Ammonia and Magnesia) has, in one or two instances, reported in the Philosophical Transactions, for 1809, been found in such quantity as nearly to fill the cavity of the blad-

der, but so large a mass of Bone-earth calculi, is surely a still more rare occurrence.

The whole number extracted was 117, of which the largest weighed 3 ij. and 38 grs.; the two next in size, *each* 78 grs., and the smallest 1 gr.—furnishing an aggregate weight of $\frac{3}{4}$ ivss."

As usual with me, no dressing was applied to the wound, but the patient was requested to keep his knees together and to remain perfectly quiet. He took 40 drops of laudanum the night after the operation, and his diet was restricted to cold lemonade flaxseed tea. He also omitted the medicines upon which he had been placed, viz., Peruvian bark and sulph. iron, with volatile alkali occasionally.

January 7th. Had passed a pretty good night. Some urine had even been already voided by the natural passage, notwithstanding the opening in the perineum. He has bathed himself in warm water; has now no fever, is quite cheerful, smokes his pipe, and has taken some soup, table tea, and an orange.

Jan. 8th. Is doing well. Has had a good night—the best, he says, for years past. Uses a bed-pan to prevent soiling the clothes. Has sat up a little by the fire.

He has continued gradually to improve, notwithstanding the unfavorable state of the weather. No other application to the wound than castile soap and warm water, several times daily.

On the 10th, four days after the operation, he changed his room. He experienced the next day some uneasiness in urinating, and had for a day or two slight diarrhœa.

On the 17th, the 11th day since the operation, he was out in the yard walking about. By pressing the edges of the wound together he could now pass nearly all the urine through the urethra.

On the 24th of January, i. e., the 18th day after he was disembarassed of his numerous calculi, Mr. O'Bannon returned home, a distance of 22 miles. The wound had nearly healed. He is to use, as a tonic, small doses of sulphs. quinine and iron.

A month after the operation, a special messenger reports him entirely well.

In noticing the peculiarities of this case, we remark, first, the cause—an injury to the spinal column, probably by partial paralysis of the bladder favoring a perversion of the function of this organ.

2d. The nature of the calculus—phosphate of lime or bone-earth. This is, I believe, peculiar to disease of the bladder itself. Any calculus may have a coating of phosphate of lime, but when composed throughout of this combination, the evidence is strong, if not conclusive, that the evil originated in the bladder.

3d. The long existence of the disease without its character being detected.

4th. The size and shape of the calculi. They appeared both in the perineum and bladder to have been regularly impacted, one against the other. Occasionally two, but generally one only was seized by the forceps in their extraction.

5th. The membranous portion of the urethra preserved its integrity, while the bulbous was ruptured by the stones. The two deposits, the one in the perineum and the other in the bladder, were about two inches apart

6th. The calculi must have all had a common origin—there being no difference in their shape, color or composition. Those in the bladder were, however a little larger than those taken from the perineum. I agree with Prof. Means in the opinion that they probably originated in the prostate gland, observing the laws of crystalization in their subsequent accumulation in the bladder and perineum.

7th. The remarkable fact that Mr. O'Bannon preserved his virile powers. His wife has borne several children, and is now actually seven months pregnant.

8th. The speedy recovery, in certainly, what must be considered, quite unfavorable circumstances.

II.—*The Philadelphia Medical Examiner.*

From the January number of this Journal, we learn that Professor Houston has retired from the editorial chair of this valuable monthly. It has been our lot to "break lances" with the former editor of the Medical Examiner; but without malice or any unkind feelings on our part. Our best wishes attend him in his retirement from a position, which brings little else than unprofitable vexation and unmerited censure. The Medical Examiner will hereafter be conducted by Drs. Smith and Tucker, in whose hands we trust it may prosper and do good service for the cause of Medical Science.

III.—*Southern Medical and Surgical Journal.*

This excellent periodical, we learn from the last number, will probably be discontinued, after the close of the present volume, for want of adequate support. It has been conducted, under the auspices of Prof. Eve, with great ability, and untiring industry, and we call upon the profession of the South to rally to its support.

We regret to find recorded, in the same number of the journal, the death of Paul F. Eve, jun., youngest son of the editor. We sincerely condole with the Father in his sad bereavement.

IV.—*Prescription for Nausea and Vomiting of Yellow Fever.*

Dr. Mitchell, U. S. N. in a paper published in the *Philadelphia Medical Examiner*, recommends the following formula as almost certain to arrest the nausea and vomiting attendant upon yellow fever:

℞ Kreosote Gutt. xx
Eth. Sulph. ℥ i
Spts, Lavend. Comp. ℥ i M.

Dose.—Tea-spoonful every fifteen minutes until the nausea and vomiting ceased. —

— We have found the following recipe quite effectual in relieving the acidity and nausea, so common in the latter stages of the disease:

℞ Soda. Bicarbonat. ℥ i
Sulph. Morph. gr. i
Aquæ Menth. ℥ ij
Aquæ Distill. ℥ iij. M.

Dose.—Table-spoonful every hour, to be repeated, *pro re nota*.

V. *The Scalpel.*

A new medical journal has been projected in New York, with the above title, by Edward H. Dixon, M. D. From our cotemporary—the Boston Medical and Surgical Journal—we learn, that it is handsomely printed and conducted with great spirit and independence.

VI.—*Tinct. Iodine, an Antidote for the Venom of the Rattle-Snake.*

Dr. Whitmire has reported, through the columns of the *North-Western Med. and Surg. Journal*, several cases of poisoning from the bites of the rattle-snake, viper and copper-head, in which he arrested the progress of the symptoms by penciling the parts bitten with the tincture of iodine. He repeats the operation frequently, and paints the parts as far as the swelling extends. The remedy, if such it proves, is of easy application and deserves a trial.

VII.—*Treatment of Catarrh by Nitrate of Silver.*

Dr. Lockwood, of the U. S. Navy, reports in the *Boston Journal* a new method of treating catarrh. In the early stages of the disease, “when the mucous membrane of the nasal cavities is dry, tumid and red, accompanied with a feeling of fulness and itching of the parts, he applies a strong solution of the *nitrate of silver* to the Schniderian membrane. He uses ten grains of the salt to one ounce of distilled water, and applies it by means of a camel’s hair pencil. It is said to effect a speedy cure.

VIII.—*Trismus Nascentium.*

In the February number of the *St. Louis Medical and Surgical Journal*, Dr. N. Ostrander has reported some cases of this disease, in which he says, he adopted the plan advised by Dr. Sims, of Alabama, with complete success.

IX.—*Georgia Medical Convention.*

A convention of the physicians of Georgia was proposed to be held in Augusta on the 20th ultimo; the object of which was to “adopt such measures for the improvement and benefit of the profession as they may deem proper.”

X.—*Kreosote, a cure for Erysipelas.*

Dr. P. Fahnestock, of Pittsburg, speaks in the highest terms of Kreosote as a remedy for erysipelas. He declares it has never failed, in his hands, to arrest the disease.

“In every case of local erysipelas, he says, I immediately apply the purest kreosote with a camel’s hair pencil over the whole of the affected surface, extending it some distance beyond the inflamed part, and at the same time administering a dose of calomel, followed by jalap, until catharsis is induced.”

XI.—*Hydrophobia.*

A well marked case of this formidable disease has been lately reported by Dr. Hartshorne, in which chloroform was freely and frequently

taken by inhalation; and the only effect was to tranquilize the boy, and produce natural sleep. It seemed, however, to loose its influence towards the last. The patient survived until the seventh day.

XI.—*The Treatment of Persons struck by lightning.*

Dr. Warren recommends dashing cold water on the head and in the face of persons stricken down by lightning; he also causes them to inhale the vapor of *ammoniated alcohol*. Mere sprinkling will not answer, he says; we must dash *pails-full* over the entire body. By this treatment, he resuscitated five or six persons who had been "struck with lightning."

All complained of spasmodic pain in the stomach after they recovered.

NEW ORLEANS, MARCH 1, 1849.

REPORT OF THE BOARD OF HEALTH FOR 1848.

We refer the reader to the *original department* of the Journal, for a very interesting Report of the Board of Health on the sanatory condition of New Orleans for 1848.

That report was drawn up by a committee of three, designated by the Board for that purpose. The following gentlemen compose this committee: Drs. W. P. HORT, J. J. KER, Y. R. LEMONNIER,—Dr. HORT, Chairman.

HEALTH OF THE CITY AND COUNTRY.

The Cholera, though gradually and steadily declining, still lingers in our city, and we know not when we shall be able to announce its total disappearance from New Orleans. It has not declined as rapidly as did the epidemic which prevailed here in 1832. Still it retains a feeble hold upon our population, as *shown* by the deaths from this disease, for the week ending 17th February, amounting to 64, and for the week previous, about 80, thus indicating a diminution of 20 deaths weekly.

About the middle of February, we had several days of rainy weather, when suddenly the wind veered round to the North, and we had a heavy sleet, accompanied with ice more than an inch in thickness. The thermometer fell as low as 26° just before sun-rise; and even at mid-day, evidences of the freezing were abundant in our streets. We have not been able to learn that this remarkable, this unprecedented cold weather affected to any serious extent, the salubrity of our population; on the contrary, we believe it has had a decidedly favourable influence. During the latter part of February we had our usual mild and balmy weather, which produced a pleasing revulsive effect upon the moral feelings, and physical system of the community—the better prepared to enjoy the change, by contrasting it with the previous cold and unseasonable temperature. Whether the cholera superseded or absorbed our usual winter diseases or not, certainly we have been less afflicted than usual with this class of affections.

During the last two or three weeks, immigrants, chiefly Irish, have been flocking to our shores in great numbers; and as might be expected, many of those, who embark for our port, sicken and die during the voyage, of ship or typhoid fever—the result of a crowded state of the vessel, a limited supply of provisions, of a bad quality and neglect of personal cleanliness. It has been our lot to board many of the immigrant ships, when they reach our port, and we are surprised, that so little sickness and so few deaths are witnessed among them.

In the first place, these vessels are too much crowded; they should not be allowed to bring more than one third their usual number. The shipping agents at Liverpool, and the ports of Ireland, regardless alike of the comforts and lives of these people, drive them on board the vessel, like “sheep for the shambles,” to often illy provided with the comforts, necessary for sustaining healthful existence, and many of them, at the time of their embarkation, with the seeds of disease in their systems.

Is it astonishing that disease should be generated and propagated among them during a long and boisterous voyage across the Atlantic? Strange indeed, with their well known want of cleanliness, if some infectious disease did not consign many of them to the bosom of the ocean, far from the sea-girt shores of their beloved Erin. It has been maintained by some English and Irish writers, that typhus is a disease peculiar to the Irish—that it should, in the language of an old sea captain, be called “Irish fever,” because this people, from their total want of cleanliness, and habits of life, carry it with them into almost every part of the world. Certain it is, it can not be developed and propagated to any extent in localities where filth does not abound.

We are not accurately advised as to the health of the neighbouring county; it has however been reported that the cholera spread from this city along the river, both above and below, visiting some of the plantations and towns, near the banks of that stream. As it receded from the river, it seems to have become less malignant and ultimately lost its destructive features. Such is the information furnished us by an intelligent physician, who has observed and noted the fact. In particular localities in this state, generally near sluggish streams and bayous, a disease, which is called *pneumonia*, has made its appearance. It is reported to kill in a few hours, the patient, becoming prostrate, pulseless and cold from the first hour of the attack. It is not attended with either purging or vomiting—but with pain in the chest and dyspnoea, and other symptoms of pulmonary congestion. If this be pneumonia, it kills in the forming stage, in the chill.

We fear that the cholera is doing its work of death in this matter under an assumed name. This is a mere conjecture, for we have not seen a single case of this so-called pneumonia, and therefore cannot compare the two cases.

The river is remarkably high—higher in fact than it has been known for many years; it threatens to overflow its banks, and indeed we learn, that at many points above, the levee has given way and much damage has already been inflicted upon the planters. This inundation above, should the river continue high late in the season, may have a serious effect upon the health of that part of the country bordering its banks.

This number of the Journal contains some statistical facts in relation to this city, which we trust will prove interesting to the scientific observer. We have also laboured to furnish an additional amount of matter from the Charity Hospital, believing that clinical medicine should occupy a large portion of the pages of a medical journal.

We hope to continue our additions to the work from this great store-house of practical medicine.

MAYORALTY OF NEW ORLEANS.

COUNCIL OF MUNICIPALITY NO. ONE, }
Extract of the sitting of Monday, 24th June, 1846. }

Be it further Resolved, That the fact of announcing publicly, by posting hand-bills in public places, the sale of medicines for the cure of diseases, shall constitute a police misdemeanor.

Be it further Resolved, That a fine of twenty-five dollars shall be imposed on every bill sticker convicted of having posted up in one or several places, one or several hand-bills, offering for sale medicines for the cure of diseases.

[Signed]

PAUL BERTUS, President.

A true copy:

A. D. CROSSMAN, Mayor.

The foregoing publication is, we think, deserving a place in the Medical Journal, as significant of the good sense and sound judgment of our city authorities.

The too common practice of spreading before the public eye, by means of *printed hand-bills*, every kind of quack-medicine, purporting to cure *every species* of disease, deserves the severest public reprobation, and should be suppressed by those in authority. To the Council of the First Municipality, with our indefatigable and excellent Mayor at its head, is due the credit—yea, the honor of taking the first step in this matter.

This movement, we hope will be followed up by others, all tending to prohibit this and similar modes of advertising nostrums and quack medicines.

These gentlemen are entitled to the thanks of our druggists and physicians.

STATE MEDICAL CONVENTION.

Extract from the Minutes of the Physico-Medical Society.

The Physico-Medical Society of New Orleans, acting in concert with the Attakapas Medical Society, requests the licensed members of the Medical Profession of the State of Louisiana to meet in Convention, in the city of New Orleans, on the third Tuesday of March, 1849.

THOMAS HUNT, M. D., President.

B. H. Moss, M. D., Sec'y.

We have more than once invited the attention of the physicians of this State to the contemplated Medical Convention, to be held in this city, on the third Tuesday of March, 1849. Shall we urge upon the liberal and enlightened members of the profession of Louisiana, the great importance—the almost absolute necessity of such a Convention at this particular time. The same arguments first put forth in favor of

this step, have lost nothing of their weight, since they were advanced. The move was originally made by the "Attakapas Medical Society," and more recently recommended and ratified as above by the Physico-Medical Society of this city. Why should we not unite, and consult each other upon such questions as may affect the interests and the respectability of our common profession? We have our political and religious conventions, at which measures are concerted and plans adopted to extend their respective principles, and wherefore shall we alone stand still when all is motion—all is action and progression around us?

The object of such a convention should not—ought not to be, to make war upon any of the other branches of the profession or the public; its aim should be higher—the advancement of pure and unadulterated science and the promotion of public health. Then let us unite in a spirit of cordial good will, and pledge ourselves to do all in our power to place the "divine art" in a fair position before the public.

At many of the conventions held in the northern States, the subject of "*medical reform*" has been discussed, and in some fashion disposed of, with but little regard to the interests and the peculiar position of their brother practitioners of the South. This, we do not believe, proceeded from a desire on the part of our Northern brethren to dictate "terms," or to impose odious restrictions upon us of the South; it is because, they are, in some measure, ignorant of our wants, of our position—it is because we have not spoken out in a voice that could reach their ears and attract their attention. Then let us, we repeat, meet together, and if we fail to accomplish any thing great in the eyes of the public, we may at least, knowing each other and cultivating feelings of friendship and good will, do much that may prove useful to the community and creditable to the profession.

Those who may feel disposed to visit the city and attend the convention, will meet with a cordial reception. We shall cheerfully throw open the pages of the Journal to the use of the convention, and aid to the extent of our means, in giving publicity to its proceedings. The folly and futility of attempting to legislate for the profession, must be apparent to all, as nothing good can be accomplished but by the united efforts of its members.

By reference to the resolution of the Physico-Medical Society of New Orleans, at the head of these remarks, it will be seen how heartily that body approves of the approaching convention. Let us work all and work together.

CHARITY HOSPITAL.

For some time past, few important surgical operations have been performed at this public charity, although a great number of interesting cases of almost every variety of disease are daily admitted into its wards. Can it be that cases, requiring the use of the knife, are less numerous, or are we more skilful in the application of the therapeutic means which the recent improvements in our science have placed at our disposal, for correcting deformities and repairing injuries of every kind?

Since the immigrants began to crowd into the hospital, ship fever has made its appearance, and although generally mild and curable, with attention to cleanliness, ventilation and nourishing diet, yet we fear, that as the cases multiply, as they must do, the infectious poison will acquire additional intensity, and the disease, become in consequence more unmanageable.

Erysipelas—that pest of hospitals has recently made its appearance in this institution. The smallest wound, or the simplest surgical operation is exceedingly prone to be followed by erysipelatous inflammation.

Two cases of this disease will be found reported below, as illustrative of the plan of treatment adopted by some of our first physicians. We have often thought that there was some affinity—some *morbid* relation, so to speak, between *erysipelas* and *typhoid fever*, as well as between the first and *puerperal* fever. They often exist in the hospital at the same time, or they may precede or follow each other, thus demonstrating that the same cause may be productive of the same disease, differing perhaps more in their respective localities than in their essentialities.

We now lay before our readers brief notes of a few cases taken at the bed side by Mr. Vincent Boagni, *Intèrne* of the Hospital, in the service of Professor W. Stone.

Case I.—Amaurosis.—On the 11th Novem, A. K. *æt* 31, a fireman, was admitted. He noticed some time ago, that his sight was failing him; and although he has for the last week abandoned his place on board of a steamer, he can now hardly distinguish light from darkness. He complains of no pain whatever, and merely mentions that his bowels are very free.

28th. The mercurial, treatment, which has been faithfully attended to, has not ameliorated his condition; cups and blisters have had no better effect.

Strychnine was now prescribed; iv grains to iv ounces of water, table-spoonful to be taken in the morning, at noon, and evening.

3d Dec. The improvement is perceptible; the patient walks about the ward and gardens.

9th. A. K. was discharged, on being able to discern a pin from a needle.

Case II.—Tic Douloureux.—On the 5th Dec. J. V. *æt* 37, a laborer was admitted. He is a rather pallid man with light hair and skin. The case is a plain neuralgic affection of the left supra-orbital nerve expanding to its ramifications. The conjunctiva is inflamed. His sufferings have continued for the last sixty days, with an exaltation of sensibility towards morning. At night the patient generally succeeded in lulling himself to sleep, but the pain wakes him first in the morning.

6th. Prescribed $\frac{1}{2}$ drachm of quinine, with two grains of opium.

7th. The patient is better. Repeated the treatment.

8th. The pains have subsided, the treatment is continued.

9th. No pain, no redness of the conjunctiva. Ordered one scruple of quinine and one grain of opium.

10th. No medicine was given.

11th. The patient desires to be discharged, and he is very thankful.

Case III.—Erysipelas.—Dec. 1st. O. P. *act.* 28, the nurse of ward No 5, complains of pain and tension in the right temporal region; the free borders of the ear were reddened, the pulse febrile and bowels costive. A saline cathartic was ordered; also water dressings to the seat of pain.

2d. The scalp was now intensely inflamed, the fever high, and symptoms of delirium at hand. The water dressings were replaced by a flax-seed poultice, to be repeated three times a day.

3d. The whole scalp, face, and part of the neck are intensely swelled and painful. The eyes are shut, the patient delirious, pulse 130. Repeated the saline cathartic, and smeared the face and neck with a salve made of one drachm of sulphate of iron to one ounce of simple cerate. The poultices to the scalp were continued.

4th. The redness, heat and sufferings are much increased; the delirium has however subsided, but the patient cannot open his eyes, nor can he talk.

Repeated the treatment of yesterday with the addition of a gargle of honey and borax.

5th. The patient is better, continued treatment.

6th. Still doing well.

7th. Ceased all medication, and ordered a good diet. The patient is convalescent.

Case IV.—Rheumatism.—On the 12th April J. H. *act.* 45, a sailor was admitted. About fifteen years ago he had an attack of articular rheumatism which confined him to bed for several months. Since that time, he never enjoyed good health. The pains have never left him altogether; his breathing has become short, and his ankles occasionally cedematous. Four months ago, his condition was almost insufferable; he was confined to bed, and daily appeared to be on the brink of death.

On examination, a prominence could be observed over the region of the heart, its apex corresponding to the interval between the sixth and seventh rib. The impulse, although visible, was not much increased. On percussion, the dulness extended over a space much larger than natural. On auscultation near the base, there was a loud blowing *souffle* accompanying the second sound, and it was propagated to the aorta.

The patient became gradually anasarctous; his breathing more distressing, and he died on the 21st December.

Autopsy.—The appearance of all the larger joints were altered, they contained a large quantity of synovial fluid, the synovia itself and the ligamentous structure were thickened and vascular. There were calcarious deposits on the cartilages, they themselves offering elsewhere a healthy appearance. The size of the heart was *bovinic*. The left chamber was dilated and its parietes thickened. The aortic valves were apparently healthy, but unable to close, owing to a very considerable dilatation of the aorta. The pulmonary and auriculo-ventricular valves were healthy.

The lungs were infiltrated with muco-serous fluid.

There was serous effusion in the plural cavities.

The organs below the diaphragm were large and congested, with the exception of the liver, which was small and indurated.

Case V.—Tracheotomy.—On the 5th Dec, C. D., a painter, *æt.* 23, was removed from the medical to a surgical ward; he had been treated with mercury for colica pictonum and salivated. His respiration was now stridulous, his condition alarming and painful to witness. The rima glottidis could be felt to be œdematous, and the epiglottis erect. With a gum lancet the epiglottis was freely scarified and the patient temporarily relieved.

6th. The breathing is not so laborious as yesterday, but the countenance is unpromising. The pulse mercurial withal.

7th. The patient is much in the same condition. Ordered that the pharynx should be touched with a solution of nitrate of silver viz; xx grs. to one ounce of water. Quinine and opium to be given by enema.

8th. Suffocation being imminent, tracheotomy was performed. The relief was complete, so that in a few minutes the patient was fast asleep.

9th The fever has much increased, and the face has assumed a cadaveric hue. The breathing is again laborious. There is much pain on the right side of the chest, and here auscultation reveals tubular respiration; on the left œdematous crepitation.

Bleeding was thought inadmissible, owing to the low state of the patient. Blisters were applied, and quinine and opium given by enema.

10th. The patient died early this morning.

Autopsy.—There was pus in the anterior mediastinum. The *Rima glottidis* was ulcerated and the epiglottis thickened. The pleura costalis on either side was inflamed. There was a considerable effusion of serum in the right pleural cavity. The right lung was partially compressed. The left was large, bulging, and infiltrated with muco-serous liquid.

The pericardium was inflamed; there was effusion and shreds of lymph within it. The liver, kidneys and spleen were much engorged.

Case VI.—Retention of Urine by a Chancre, and subsequently, by the pressure of an abscess on the Urethra.—N. C., *æt.* 25, was admitted on the 7th February, for retention of urine. He had not passed his water for the last day and night. Was now suffering intensely and (as he said) very near bursting. His bladder could be felt quite as high up as the umbilical region. A catheter was introduced, and no water could be found. The penis was now closely examined and a fistulous opening was seen very near its middle part.

The patient, a very intelligent man, said that the end of the canal had been obliterated by a chancre situated in the extremity of the urethra, for which he was treated in this city; that since, this opening had formed, and he had passed his water through it for the last month; at first freely, subsequently with more difficulty, until he could not pass it any more. On further examination, a large abscess was found in perineum which on being opened, let out not only a vast deal of pus, but permitted also the urine to pass readily through the fistulous opening.

The abscess after having been well emptied, was treated by pressure made with a .T. bandage, under the use of which the loose skin quickly became adherent to the contiguous parts. Now, a straight bistourie was plunged in the glans penis, and its point directed towards the still pervious urethra. The incision was a little more than one inch in depth. This done, a female catheter was introduced and retained in place.

The fistulous opening was freely cauterized with nitrate of silver, and its edges brought towards each other by adhesive plaster.

22d Feb.—During the past day, the catheter has been occasionally removed to be cleansed. The fistulous opening has closed without any pain whatever. During his sojourn in the ward he enjoyed good health and did not require the least medicine. He was kept on full diet and wine the whole time. This day he was discharged at his own request.

Case VII.—A Case of Severe Injury.—J. W. was at work on the 2d February calking a boat of fifteen tons capacity, when by accident the huge mass leaned on the side and squeezed the poor fellow to death, or *quam proxime*. He was admitted in the ward two days after the occurrence. His chest was shapeless. Six ribs on the left side were distinctly felt to be broken, and the clavicle was dislocated, both at its sternal and accromial extremities. His abdomen was much distended, very tense and painful; in fact, he complained of much pain all over his body, which was severely bruised in many parts. To all appearance it was an unpromising case. Our auscultation was powerless, unless to illustrate the crackling of broken bones; this sound drowned all others.

The patient was bled, and cups applied to the left side of the chest; a dose of castor oil was exhibited, and a bandage applied around his thorax.

3d. Feb.—No sensible amelioration can be perceived. Calomel and opium were prescribed in small doses, to be taken at two hours interval.

20th Feb.—During the two weeks that have elapsed, the patient has passed through the periods of pleurisy with large effusion. At one time, the to-and-fro of pericarditis was ascertained to exist. For all these complications he was treated with cupping, calomel and opium, blisters and hydriodate of potash. Finally, he has recovered, his ribs have united; the respiratory murmur is again normal; the clavicle alone can be moved up and down his neck at pleasure.

Case VIII.—The effect of Chloroform in Luxation.—During the past two months ten individuals applied for relief. They all were recent cases of luxation. *Eight* of the *humerus* in the *axilla*, *one* under the *clavicle*, and *one* with the *ulna* and *radius* thrown backwards. They were all reduced on the first attempt, while the patient was under the influence of chloroform.

Case IX.—Erysipelas.—For the last two months, many cases of erysipelas have been admitted, and several originated in the wards; in most of them the disease involved the face and scalp, spreading downwards over the neck and shoulders. The treatment employed was very simple, and is mentioned here on account of its successful effect.

At first anodyne poultices were applied and renewed frequently. Bowels were freely opened with saline cathartics. Afterwards *quinine* and *opium* in solution were given, to the amount of thirty grains of the one and three of the other daily, with wine repeated occasionally.

In three cases, the use of the ointment of sulphate of iron was beneficial. In two cases of extreme sloughing of the throat and mucous tissue of the noses, injections of chloride of soda, diluted with water, answered the purpose well.

But one of the cases alluded to died. He was an inveterate drunkard, and apparently died of delirium tremens. All the others recovered speedily.

Case X.—Tracheotomy.—P. A., æt. 36, the nurse of a medical ward, complained on the first of February of pain, which he attributed to a tumour newly gathered on the upper and lateral part of the neck.

He did not allude to it again until the 8th, when he went to bed with much fever, anxiety of countenance, and difficult respiration. Upon inquiry, he stated that ten days ago, after a night exposure to the draught of air from a window, he first perceived a small tumour on his neck, which has ever since enlarged, and is now the size of a small orange. This evidently pressed a good deal on the trachea and contributed in some manner, to render his condition very precarious. But mischief was brewing elsewhere, for as the finger was placed in the larynx the glottis was to be felt erect and thickened, and the rima glottidis narrowed and œdematous. These parts were freely scarified, and the patient was submitted to a proper use of the tartrate of antimony. For several days he appeared relieved, but again on the 17th, his respiration became difficult, stridulous; his pulse thread-like; his countenance haggard. Under these circumstances, tracheotomy was performed. Not only was relief immediate and great, but ever since, the patient has improved and is now convalescent. The tumour on the neck has subsided and gives no more inconvenience to the patient.

Case XI.—Ulcer.—T. W. has a large phagedenic ulcer on the inner side of his ankle. He has suffered intensely night and day for many months, and he will now readily submit to an amputation. His health is broken down; he is dejected in mind; his appetite is lost. This day, the third of February, he was admitted in the ward. The preparations of morphine were first resorted to to give him ease. The ulcer was soon afterwards filled with powdered *capsicum*. On the very next day the change was striking. The ulcer had assumed a better appearance; the pain was less, and could be endured without the aid of morphine.

6th. Feb.—The granulations are now healthy, the emplastrum adhæsivum is ordered to effect gentle pressure.

25th Feb.—The ulcer is nearly well. From a circumference of nearly nine inches, it is now reduced to the size of a crown-piece, the patient's health is fairly recovered. He still continues to use the adhesive plaster.

We publish in the subjoined table some interesting statistics, taken from the Records of the New Orleans Charity Hospital, for the year 1848. The table embraces the total of *admissions*, *discharges*, and *deaths*, with their *diseases*, and will be found in the main correct. In some instances, the deaths seem to exceed the admissions, but this can be explained, from the fact that many diseases originated in the hospital, and such patients may or may not die; hence, the inequality also between the discharges and deaths.

We are indebted to Mr. Loubère, one of the clerks of the Hospital, for a copy of the report, at the close of which he has given us some explanations in reference to the table.

NEW ORLEANS CHARITY HOSPITAL.

YEARLY REPORT OF DISEASES FOR THE YEAR 1848.

DISEASES.	Admis- sions.	Dis- ch'g's.	Dths	DISEASES.	Admis- sions.	Dis- ch'g's.	Dths
Arthritis,	11	17		Dysmenorrhœa,	2	2	
Ascites,	34	24	32	Diabetes,	1		
Amenorrhœa,	7	7		Dementia,	2	3	
Anasarca,	29	31	4	Eczema,	5	3	
Abscess, various,	127	122		Enteritis,	48	54	14
Do. of liver,	3		4	Epilepsy,	12	8	2
Anemia,	156	149	7	Enteralgia,	16	11	
Asthma,	10	10		Encephalitis,	7	2	6
Amaurosis,	6	7		Erysipelas,	29	33	26
Amputation of arm,		1	1	Endo-carditis,	6	3	
Do. leg,		1		Epistaxis,	4	4	
Do. hand,		1		Fever, intermittent,	2392	2296	
Do. thigh,		5		Do. remittent,	490	465	11
Do. finger,		2		Do. typhus,	1882	1811	344
Apoplexy,	7	2	6	Do. yellow,	1234	806	420
Aneurism of aorta,	3	1		Do. catarrhal,	13	9	
Acneitis,	1	1		Do. bilious,	177	172	3
Albuminuria,	1	1		Do. ephemerai,	92	88	
Arteritis,		1		Do. congestive,	56	21	31
Arachnitis,	2		1	Do. continued,	6	4	1
Abortion,	1	1		Do. scarlet,	6	4	2
Bronchitis,	183	167	6	Do. puerperal,	1		1
Burn,	27	20	2	Fracture of fingers,	1		
Bruise,	7	8		Do. arm,	8	5	
Bronchorrhœa,	2	3		Do. fore-finger,	13	15	
Colica, bilious,	2	3		Do. thigh,	10	11	
Do. pictonum,	47	38		Do. leg,	19	21	1
Cynanche,	6	7	2	Do. clavicle,	10	10	
Carbuncle,	12	12		Do. ribs,	12	13	
Constipation,	75	64		Do. maxil. supe'r,	4	4	
Catarrhus,	40	37		Do. maxil. infer'r,	2	1	
Convulsions,	1		3	Do. skull,	6	1	5
Corneitis,	16	22		Do. wrist,	1		
Contusion,	186	188	2	Do. nasal bone,	1	1	
Cephalalgia,	24	18		Do. foot,	2		
Cystitis,	6	8	2	Do. spine, [alis,	1		1
Cancer of the womb,	1	1		Fistula visico-vagin-	1	2	
Do. stomach,	1		1	Fistula in ano,	20	18	
Cerebritis,	7	1	6	Furunculus,	15	14	
Cataract,	3	3		Gastritis,	105	77	2
Carditis,	2	1		Gastralgia,	20	23	
Concussion of brain,	2			Gonorrhœa,	49	43	
Cœcitas,	10	3	1	Gangrene of the lungs,	1		
Cirrhosis of liver,	2	1	1	Gastro-enteritis,	39	13	19
Cholera, Asiatica,	662	59	396	Gravel,	3	2	
Coarctatio, aortæ,	6	7	6	Gangrena,	6	2	4
Congestion of brain,	35	4	21	Gout,	2		1
Do. lungs,	3	4		Hypochondriasis,	1	1	
Cholera, infantum,	1		1	Heart, organic disease	21	14	7
Coxalgia,	2	2		Hydropericardia, [of,	14	5	1
Cardialgia,	1	1	1	Hoemoptysis,	4	3	
Dyspepsia,	30	39		Hepatitis,	33	29	9
Dysuria,	1	2		Hernia,	14	12	3
Dysentery,	421	233	168	Hydro-thorax,	7	8	
Delirium tremens,	161	162	15	Hæmorrhoids,	15	16	
Diarrhœa,	450	395	64	Hysteritis,	5	7	
Dengue,	8	9		Hemiplegia,	8	5	2

YEARLY REPORT OF DISEASES FOR THE YEAR 1893.

DISEASES.	Admis- sions.	Dis- ch'g's.	Dths	DISEASES.	Admis- sions.	Dis- ch'g's.	Dths.
Hydropericarditis,	5	7		Pleuritis,	38	44	2
Hyperthrophy of the	3	2	1	Paraplegia,	9	6	3
Hydrocele, [liver,	3	3		Paralysis,	9	8	2
Hemicrania,	6	6		Pericarditis,	10	11	2
Herpes,	2	2	1	Parotitis,	8	7	
Hysteria, [pura,	3	2	1	Parturition,		69	
Hæmorrhagica pur-	1	1		Psoriasis,	6	6	
Hæmatemesis,	3		1	Pleuro-pneumonia,	5	3	1
Hydremia,	1	2		Phrenitis,	1		1
Kidneys, affection of,	1			Peritonitis,	9	5	5
Indigestion,	9	3	1	Prolapsus uteri,	3	3	
Insanity,	2	2		Pemphigus,	3	3	
Icterus,	25	26	3	Prurigo,	2	2	
Inebrietas,	45	47	5	Pompholix,	1	1	
Idiotcy,	3	1		Phlebitis,	2	2	
Impetigo,	6	6		Pneumo-thorax,	2		
Injury of spine,	1			Phthisis laryngea,	1		1
Incontinence of urine,		1		Phymosis,	1	2	
Ischuria,	1	1		Para-phymosis,	1	1	
Ictus solis,	10	5	4	Porrigo favosa,	2	2	
Laryngitis,	7	8	4	Rheumatism,	338	269	3
Leucorrhœa,	3	2		Retention of urine,	2	2	
Lumbago,	25	24		Rubeola,	6	6	
Luxation of elbow,	1	1		Stricture of rectum,	1	3	
Do. shoulder,	7	7		Do. urethra,	20	18	
Do. clavicle,	2	3		Do. aorta,	10	7	4
Do. hip,	2	4		Sphacelus,	1		
Do. radius,	1	1		Scurf,	6	6	
Do. tibia,	1			Syphilis,	193	204	2
Lichen tropicus,	3	3		Scorbutis,	8	9	
Mania,	6	6		Scald,	9	13	
Do. a potu,	8	6	2	Sprain,	32	30	
Do. puerperal,	2	2		Scleratitis,	3	3	
Meningitis,	1		1	Sarcoma testiculi,	1	1	
Metritis,		1		Scabies,	5		
Morsus aranea,	1	1		Splenitis,	4	5	
Menorrhagia,	1	1		Softening of the brain,	8		5
Masturbation,	1	1		Stomatitis,	1		
Necrosis,	9	9	1	Scrofula,	9	9	
Neuralgia,	20	19		Surditas,	2	3	
Ophthalmia,	82	96		Staphyloma,	1		
Orchitis,	16	17		Tonsillitis,	6	6	
Otorrhœa,	4	6		Tetanus, idiopathic,	1		1
Opacity of cornea,	1	1		Tetanus, traumatic,	12	4	7
Otalgia,	1			Tumor, iliac,	1	1	
Odontalgia,	6	3		Tumor, ovarian,	1	1	
Old age,	4	2	4	Tumor, arterial,		1	
Ozœna,	1	1		Urethritis,	1		
Otitis,	4	3		Ulcers,	361	403	
Osteo-sarcoma,	1			Variola confluens,	16	16	
Pneumonia,	69	44	19	Varices,	2	2	
Phthisis, [num.	255	110	158	Varioloid,	15	14	
Poisoned by lauda-	1	1		Worms,	1	1	
Pregnancy,	102	36		Wound, incised,	61	65	1
Pleurodynia,	21	24		Do. contused,	6		
Paronychia,	34	31		Do. punctured,	12	20	1
Podagra,	2	2		Do. lacerated,	22	16	
Pharyngitis,	2	2		Do. gun-shot,	12	8	1
Ptyalism,	14	15		Total, ...	11945	10010	1897

OBSERVATIONS.—1. During the year 1848, there were 295 persons admitted who had been over three years steady in New Orleans at the time of their admission; of this number 122 had the typhus fever, 104 the yellow fever, and 69 cholera Asiatica.

2. It will be seen that not a single case of variola died out of 16 that had been admitted. The reason of this is, every case of variola was immediately sent to the Franklin Infirmary—according to the rules of the hospital. L.

List of Interments in the City of New Orleans from the 21st of October, 1848, to the 17th of February, 1849, being a period of 17 weeks.

Accidental, 5; Accouchement, 1; Adynamia, 1; Anemia, 2; Angina malig., 2; Apoplexy, 40; Ascites, 8; Births, premature, 2; Bladder, disease of, 1; Brain, compression of, 4; do. concussion of, 1; do. congestion of, 25; do. hemorrhage of, 1; do. softening of, 1; Bronchitis, 4; do. chronic, 4; Burn, 9; Cancer, 3; Catarrh, 13; Cerebritis, 26; Cholera, 929; do. Asiatic, 707; do. Asphyxia, 10; do. Infantum, 10; do. Morbus, 62; Cholic, painters, 1; Colitis, acute, 1; Consumption, 233; Convulsions, 80; Cramps, 3; Croup, 15; Debility, 73; Del'm Tremens, 22; Dentition, 31; Diarrhœa, 19; do. chronic, 30; Disease, inflammatory, 2; Dropsy, 19; Drowned, 17; Dysentery, 66; do. chronic, 24; Eclampsia, 1; Encephalitis, 5; Endo-carditis, 1; Enteritis, 34; do. chronic, 13; Entero-Colitis, 3; Epilepsy, 3; Erysipelas, 4; Fever, adynamic, 2; do. congestive, 7; do. bilious, 2; do. intermittent, 2; do. malignant, 6; do. puerperal, 4; do. remittent, 3; do. scarlet, 5; do. typhoid, 84; do. typhus, 18; do. yellow, 59; Fracture, compound, 1; Gangrene, 5; Gastritis, 5; do. chronic, 1; Gastro-enteritis, 26; Heart, aneurism of, 3; do. disease of, 11; do. hypertrophy of, 7; do. wound of, 3; Hemorrhage, 6; Hepatitis, 15; Hip abscess of, 1; Hydrocephalus, 5; Hydrothorax, 3; Indigestion, 2; Insanity, 1; Intemperance, 12; Kidney, disease of, 1; Laryngitis, 8; Lungs, gangrene of, 1; Mania, 1; Marasmus, 4; Measles, 5; Meningitis, 18; Myelitis, 1; Neck, fracture of, 1; do. wound of, 1; Old age, 20; Paraplegia, 2; Paralysis, 2; Peritonitis, 3; Pericarditis, 2; Pertussis, 12; Phrenitis, 2; Pleurisy, 6; Pneumonia, 25; do. typhoides, 6; Purpura, 1; Rheumatism, 1; Scald, 4; Scrofula, 4; Skull, fracture of, 9; Spasms, 7; Small Pox, 43; Spine, disease of, 1; do. injury of, 2; Still-born, 89; Stomach, wound of, 1; Suddenly, 3; Syphilis, 3; Tabes Mesenterica, 1; Tetanus, 27; Trismus Nascentium, 48; Typhus, abdominalis, 2; Uncertain, 176; Urethra, stricture of, 1; Uterus, cancer of, 1; do. inflammation of, 1; Verminose, 4; Wound, (shot) 5; Wound, penetrating, 3. Total, 3,433; of which 734 were under 10 years; 2,801 were white, and 632 were colored.

(Extracted from the Reports of the Board of Health.)

A. HESTER, *Secretary.*

Dr. Brickell's Hospital Reports.

For some time past, I have taken advantage of all good opportunities to test the virtues of Chloroform, and although my observations are as yet quite limited, I will present you a summary of the same, hoping

that I may be enabled to furnish you ere long with more extended, and therefore more satisfactory data.

1st then, *External Application of chloroform*. I have used the agent thus in cases of Facial Neuralgia, in Cephalalgia, in odontalgia and in obstinate Singultus, and invariably with the happiest results.—The mode of application is extremely simple.—Merely moistening the corner of a pocket handkerchief or towel (two to four inches square), applying it to the part affected and placing the remainder of the article over it in folds to prevent evaporation.—The patient experiences a sensation of “burning” almost as soon as the Chloroform is brought in contact with the skin, and vesication may be very easily induced if the application be protracted for a few minutes; this, however, varying in different subjects.—Of the three first mentioned affections, I have seen the most intense attack relieved almost instantaneously; of these, one case of Facial neuralgia and two of Cephalalgia, occurred in my wards at the hospital, and several others, together with cases of Odontalgia, occurred in private practice.—In three cases of Singultus (one in the hospital and two in private), I have applied Chloroform to the Epigastrium, and with great relief to the patients.—In each of these cases the symptoms *recurred* several times, but were always speedily relieved by the reapplication of the remedy.—It may be well to mention here, too, that in the case which occurred in the hospital, the application of Chloroform to the epigastrium, not only relieved the hiccough, but more than once induced *sleep*; and the same thing occurred in the person of a lady to whose temple I made the application for the purpose of relieving an attack of neuralgia.

2nd. the inhalation of Chloroform in cases of Bronchitis.—I have administered the remedy thus in four cases in the hospital; three chronic and one acute.—Two of the former recovered more speedily than I have known such cases under the ordinary modes of treatment, and the third is still under the use of this agent, improving slowly, after having resisted all other remedies I could think of; indeed, I regard it as the worst case of bronchitis I have ever seen. The *acute* case yielded to the remedy in four days from the time of the first inhalation.

In each case I have administered the Chloroform with my own hands regularly every morning on visiting my wards.—I cause the patients to inhale it from a sponge until slight exhilaration is induced.—They all seem to acquire a fondness for it after the first two or three exhibitions, and in one case the effect produced has often been like that of the nitrous oxide gas, causing the patient to declaim most vehemently.—I have seen no ill effect whatever; and I think that patients become accustomed to the use of it in the same manner as to the use of tobacco or alcoholic drink.

Abdominal Tumour.—Operation.—Recovery.

Jan. 29th, 1849, Cornelius Stott, æt. 32 yrs., entered ward 11, Charity Hospital—has a fluctuating tumour midway between the left hypochondriac and lumbar regions of the abdomen, about five inches in diameter and about an inch anterior prominence—quite distinctly circumscribed.—Says he fell from a considerable height some three weeks ago, resting immediately on the tuberosities of the ischia.—Shock

was extreme, and he was very faint for several hours.—About ten or twelve hours after the fall, he perceived a tumour in the above mentioned locality, attended with much soreness and pain on pressure—tumour was much more prominent than it is at this time—was prescribed for by a physician during several days, but with little relief.—

This morning I find not much pain on pressure, but he complains of constant “dull and heavy” pain throughout the left side—looks pale—is quite feeble—appetite pretty good—bowels in good condition—tongue natural—evident fluctuation in the tumour, and throughout the abdomen.

Febr. 11th.—Have tried blisters, poultices etc. etc., up to the present time, but with no apparent benefit.—Tumour is, perhaps, rather more prominent—No pain.—Quite a number of physicians and students of medicine have seen the case, and almost an equal number of diagnoses have been formed—such as, abscess of the spleen, abscess of left lobe of liver, rupture of colon, abscess in parietes of abdomen, etc., etc.—All agree that there is fluctuation in the *tumour*, but there is difference of opinion as to the existence of the same in peritoneal cavity generally.—Of this, however, I think there can be no doubt.

I now, with a sharp-pointed bistoury and director, made an opening into the most prominent portion of the tumour, (half an inch externally, and a quarter do. internally), about three inches to the left and a little above the umbilicus, when out* rushed a dark and thin fluid, more like the lees of wine than any thing I can now think of.—I assisted the discharge by keeping the lips of the incision apart, and in the course of ten or fifteen minutes fully three pints were poured out.—About half of this was discharged without the aid of pressure on any part, but the remainder could be made to flow only through the means of pretty firm pressure exerted with the hands from the lower portion of the abdomen upwards towards the umbilicus.—The walls of the abdomen at the seat of incision appeared to be about a quarter of an inch in thickness, and I could pass my probe perpendicularly (the patient on his back) to the depth of four inches or more without encountering the least resistance.

The wound was brought together with an adhesive strap, and the patient is now (Febr. 25th) entirely well, having recovered without a disagreeable symptom.

Under existing circumstances, I think there can be no doubt that the fluid was within the *cavity* of the abdomen; though some of the gentlemen who saw the case previous to the operation, think otherwise, and for the reason that the presence of a *tumour* in this particular locality is not accounted for.

Feb. 25th 1849.

D. W. B.

Memoir of C. A. LUZENBERG, M. D., President of the Louisiana Medico-Chirurgical Society. By Thomas M. Logan, M. D., published by order of the Society.

We have looked forward with some interest to the publication of this memoir, deeming the subject worthy of particular notice, and knowing that the task was assigned to hands every way competent to do it well.

The late Dr. Luzenberg was unquestionably a man of genius, and during his brief, but eventful life, acted a conspicuous part in the medical affairs of New Orleans. He came to this city a poor and friendless, but bold adventurer, and his subsequent career was signalized by many of those varying phases which so often mark the course of unassisted genius struggling against adversity. Whatever may have been his faults—and like most men of ardent temperament and impetuous disposition, he doubtless had faults—they were certainly not unlinked with decided merits. He now sleeps in the grave—let the mantle of oblivion be drawn over his defects, and naught but the remembrance of his merits be cherished.

We will not attempt to criticise the memoir of Dr. Logan. It is the offering of devoted friendship, and so far as we know, acceptable to the surviving friends of the deceased. But we cannot omit the opportunity to say something concerning the Medico-Chirurgical Society, of which the late Dr. Luzenberg was president.

The organization of this society in 1843 was brought about chiefly by the indefatigable efforts of Dr. Luzenberg. At that time there was no medical society in operation in this city; the old medical society of New Orleans being *defunct* and the Physico-Medical Society *asleep*. Nor was there a medical journal published anywhere in the South. Our medical college, too, was in a languishing condition, having no building expressly for its purposes, and rarely drawing together more than thirty-five or forty students to attend its lectures. At this gloomy period of medical history in New Orleans, the legislature granted a charter incorporating some fifteen or twenty physicians into the *Medico-Chirurgical Society of Louisiana*. It commenced operations on the 1st of April, 1843, and its meetings were kept up with so much spirit and energy that, in a short time, new life seemed to be infused into the entire medical profession in these parts.

The following year a medical journal was started in this city, soon followed by one in Augusta and Charleston. Then came the erection of a college building, and the resuscitation of the Physico-Medical Society, which had lain dormant since 1838. We do not mean to trace all these improvements back to the Medico-Chirurgical Society, but they evidently followed each other in such quick succession as to authorize the belief that there may have been some dependance or connection between them. But these brightening prospects were doomed to undergo a melancholy change. Since the anniversary of 1847, the Medico-Chirurgical Society has languished until it has fallen into complete decay. It has done no scientific business for two years past, and as if with the view of giving it the *coup de grace* and terminating its agonies, a small number of members, *less than a legal quorum*, recently got together, gave away its funds and adjourned *sine die*.

Thus may the society be said to be *dead*. But we object to the *manner* of its death. If the present officers and members of this society be unwilling to do any thing more, let them at least *preserve the funds and archives*—these may form the *nucleus* of a future organization. We shall long remember the regular and interesting meetings of this society during the first two or three years of its existence. There the French and American physicians met to interchange

professional views and opinions, and there the utmost harmony prevailed. Each anniversary might truly be characterized as "the feast of reason and the flow of soul." But now—how sad the retrospect!! A president and two vice-presidents are numbered among the dead, and their survivors have fallen into a state of apathy. Harlan, Slade and Luzenberg are no more; but we hope yet to see the day when the Medico-Chirurgical Society, like the fabled *Phœnix*, will arise again from its ashes.

F.

ABSTRACT OF A METEOROLOGICAL JOURNAL FOR 1849.

BY D. T. LILLIE, AT THE CITY OF NEW ORLEANS.

Latitude, 29 deg. 57 min.; Longitude, 90 deg. 07 min. west of Greenwich.

WEEKLY. — 1849.	THERMOMETER.			BAROMETER.			COURSE OF WIND.	FORCE OF WIND, Ratio 1 to 10.	Rainy Days.	Quan- tity of Rain. — Inches.
	Max.	Min.	Range.	Max.	Min.	Range.				
Jan. - 27	74.0	46.0	28.0	30.43	30.18	0.25	E.	3	1	2.500
Feb. - 3	79.0	60.0	19.0	30.28	30.13	0.15	S.W.	2 $\frac{3}{4}$	0	0.000
" - 10	76.0	44.0	32.0	30.58	29.89	0.69	N.W.	3 $\frac{3}{4}$	3	4.325
" - 17	62.0	30.0	32.0	30.36	29.80	0.56	N.W.	4	2	2.400
" - 24	74.0	30.2	43.8	30.54	30.26	0.28	S.S.W.	3 $\frac{1}{2}$	0	0.000

REMARKS.—The Thermometer used for these observations is not attached to the Barometer, but is a self-registering one, and is placed in a fair exposure. Regular hours of observation, 8 A.M., 2 P.M. and 8 P.M.

The Barometer is located at an elevation of 19 feet above the level of the ocean, and is suspended clear of the wall of the building.

The Rain Gauge is graduated to the thousandth part of an inch, and the receiver is elevated 40 feet from the ground.

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W.

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